

Interactive comment on “Elemental and organic carbon in PM₁₀: a one year measurement campaign within the European Monitoring and Evaluation Programme EMEP” by K. E. Yttri et al.

Anonymous Referee #1

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General comments:

The paper presents a description of a background monitoring network and results from this network. Some of the work is really interesting, especially the attempts to use an OM/OC ratio based somewhat on the OC composition, the discussion of differences between PM₁₀ and PM_{2.5}, and source influences on the supposedly background sites.

However, I have some concerns, especially over the use (or rather, non-use) of field blank data, and non-presentation of field blank data for PM mass measurements (these are made on quartz fiber filters which tend to be brittle and could disintegrate during handling). Their method of separation of WSOC also needs clarification. There are

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other points regarding their explanations for different effects seen, which are listed in “specific comments” below.

Finally, I'd like the authors to comment on the usability of these sites as “background” sites - what exactly are they the “background” for, if they are influenced by nearby polluted locations (e.g. the sites in continental Europe)? Can they really be used as background locations?

Specific comments:

Introduction:

Instead of “organic molecules”, suggest use “organic species”.

Instead of “pyrolytically generated EC”, suggest use “pyrolytically generated light-absorbing carbon” or “pyrolytically generated refractory light absorbing carbon” or better, “charred organic carbon” - the optical properties of charred organic carbon generated during the thermal-optical analysis seem to be different from that of native EC, e.g. Chow et al. 2004; Subramanian et al. 2006.

What is an “urban background”? That seems contradictory!

Section 2.2.1: WSOC analysis: Samples were sonicated for WSOC. Were the extracts filtered prior to TOC analysis? Sonication will quite likely get some insoluble matter off the filter as well. One way to check would be to run OC/EC analysis on the remaining (post-sonication) filter, if it is still intact.

Section 3.1: Blank OC levels were “ $<0.5 \mu\text{g}/\text{m}^3$ for 13 of the 14 sites” - that seems rather high considering these are background sites. Could these be reduced further?

Page 3865, lines 7-10: The presented concentrations are not field blank-corrected. The reason given is rather strange - “field blank OC level could be reduced by as much as a factor of two when inserted into the sampler and letting particulate free air flow through”. Do the authors have any data to back up this statement, or any reference?

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There appears to be plenty of data to the contrary - e.g. any artifact measurements on backup filters are exposed to particle-free air, yet the OC on the backup filters is much higher than field blank levels. Granted, they sample particle-free air for long periods, but that still satisfies the author's condition and yet disproves their claim.

Page 3866, lines 6-8: The authors should also consider the fact that most of the results in the table are at urban sites, whereas the study most comparable to the EMEP program is the Putaud and Cavalli (2006) study at a rural site. Also, the QBT method is usually a better estimator of the positive artifact.

Page 3866, lines 10-17: The seasonal variation in positive artifact is suggested as caused by lower levels of particulate OC in the summer relative to winter. Are the authors considering absolute values of the artifact, or relative to the particulate OC? In any case, the summer levels of OC are higher than the winter concentrations in the study by Subramanian et al. (2004), which contradicts the results of Viana et al. and thus the argument proposed by the authors. In any case, this result is really from the work of others (Viidanoja et al.) and not the present study, and so this paragraph seems out of place, because the authors don't really use the results anyway.

Figure 1: The axis ticks are unreadable. Are they all plotted to the same scales (e.g. 0-10 $\mu\text{g}/\text{m}^3$)? Making such same-scale plots or if already implemented, stating this fact in the figure caption would improve the readability of the figure.

Section 3.2:

The EC levels in EMEP are much higher ($\sim 3\text{x}$) than for the IMPROVE sites; also, the IMPROVE protocol usually measures more EC ($\sim 2\text{x}$) than a NIOSH-type protocol. Put this together, and the EC levels at the EMEP sites are actually much higher ($\sim 6\text{x}$) than what has been found in rural sites in the US!! Can the authors explain why this is so? (A PM₁₀ v/s PM_{2.5} reason is probably not appropriate since EC is usually found in the fine aerosol.)

Also, the Braganza site, which is southern-most of all EMEP sites, actually shows much less EC than at least three northern sites (Ispra, Illmitz, Stara Lesna) - an equally good explanation appears to be a coastal-to-continental transition, with sites closer to water-bodies showing lower EC values than inland sites. Could the authors comment? (A similar comment could be made for OC.)

Section 3.3:

See the coastal-to-continental transition comment for EC, which could also be applicable to OC.

Comparison of EMEP results with IMPROVE data: Have the authors considered the effect that the size fraction the two studies use could have on the seasonal trends, e.g. biogenic species being more prominent in PM10 than PM2.5?

Figure 5: Could you please mark the sites on the plots - it is very hard to tell where the site is located.

Coarse contribution to PM10: "PM10 might be subject to positive artifacts, while this is not the case for coarse OC" - add some explanation why (restating that the artifact from PM2.5 and PM10 measurements cancel out is sufficient). This may not be immediately clear.

Section 3.7:

See earlier comment on measurement of WSOC and WINSOC.

What are the blank levels for PM10 mass measurements? Quartz filters are rather brittle, so post-weighing after sampling/transport could affect the observed mass concentrations.

RM8785 is the fine fraction of resuspended urban aerosol, whereas the EMEP results are for PM10 from mostly rural sites. The authors have earlier shown that PM10 has a greater biogenic influence at least at one site, compared to PM2.5. How do the

authors then justify using the IMPROVE/NIOSH comparison (which likely depends on the sample matrix) from RM8785 for the EMEP samples?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3859, 2007.

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