

Interactive comment on “Toward a standardised thermal-optical protocol for measuring atmospheric organic and elemental carbon: the EUSAAR protocol” by F. Cavalli et al.

Anonymous Referee #1

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General comments

The topic of the paper is interesting and the paper aims at defining a new protocol suitable for OC/EC quantification (mainly by networks) at European sites.

The main criticisms to the paper are:

1) even if results/conclusions are often reasonable, they are not always supported by data i.e. very few data are shown although the authors often mention that many samples were collected at different sites (how many in total? Were the samples collected during different seasons? There is no indication in the paper).

C704

2) Throughout the paper the authors do not mention the possibility of having carbonatic carbon in PM samples. Can the authors exclude CC presence at European background sites? If not, this case should be also investigated before proposing a new protocol for Europe.

3) the work appears not to be as much systematic as the set-up of a protocol proposed for Europe is expected to be (e.g. no systematic comparison with other protocols widely used also in Europe has been carried out). Nevertheless, this paper could be the starting point for the set up of the "European protocol" but it should be stated more clearly in the text (what in the title is "toward" in the text appears as "we got the protocol").

Specific comments

Lines 124-126: can the authors explain the 1.7 constant factor?

Lines 130-131: the authors' hypothesis is that OC particles diffuse inside the filter during sampling or analysis. Can they demonstrate this assumption? Indeed, in standard samplings (especially in background sites where PM concentrations are not so high) the possibility of particles diffusion inside the filter should be quite limited (e.g. about 10-15% as estimated by literature works, e.g. Petzold and Schönlinner, J. Aerosol Science 35, 2004). The diffusion of particles inside the filter during analysis should be proved or referenced too.

Lines 131-134: the referee agrees, WSOC removal is one of the most efficient way to minimise charring. The authors should compare the results obtained with EUSAAR_2 protocol with those obtained on the same filters with WSOC removed. It is clear that WSOC removal cannot be proposed in a protocol mainly devoted to networks but it is mandatory to evaluate possible differences before validating a new protocol.

Line 141: the authors must specify if they are referring to absorption or extinction/attenuation cross section

Line 161: why differences in sigma alter EC split point? Please give explanation in the

C705

text.

Line 173: how many samples? Were they from different sites/seasons or had different composition? Please give details.

Line 172/Table 2: How much is the uncertainty on each analysis? The reported values are quite similar when considering, let's say, a 10% uncertainty.

Line 191: Again, how many samples? Which type?

Lines 191-198: the authors should take into account that in Cachier et al. (1989) pure oxygen was used while they refer to measurements carried out "in presence of oxygen" or with "oxygen-containing carrier gas": what was the oxygen percentage in the gas used for their tests? It must be specified in the text and it should be demonstrated that the same result is obtained with 100% oxygen or different oxygen percentages. The referee suggests discussing this point further, showing data and giving explanations for different results.

Line 202: was it a sample collected in summer or winter?

Line 206: how many samples in total were investigated? Should the authors show at least one for comparison (possibly with different composition)

Line 209: as concerns inorganic oxides, have the authors measured the sample composition? Were there differences between samples? What was the average composition at different sites?

Line 216: the authors must specify if they are referring to absorption or extinction/attenuation cross section because in the latter case it depends on the average composition of the sample.

Lines 229-240: how many LAC samples were generated and analysed? On how many analyses were the standard deviations calculated? A major concern on these analyses is that using LAC-only samples the authors cannot observe possible effects due to the

C706

presence of other compounds (i.e. inorganic oxides) that have an important role as they stated previously in the text.

Lines 254-255: the authors should better specify what is this derivative. If it is the time derivative (as time is the variable on the x-axis) the indication is on the rate of formation/release and the peak integration is needed to have the overall quantity formed/released. Otherwise, the independent variable must be specified. The referee supposes that this derivative is the green line called absC in the graph: please write in the caption or in the text what $\text{absC} \cdot 100$ ($k_{\text{sigma}}=45$) stands for (remind that not all the Sunset users have the same version of the instrument/software).

Lines 268-269: the mentioned 91% was calculated in the He-mode? Probably yes, but please specify for more clarity.

Line 276/Figure 2d: how the authors explain the large difference between the peaks in green and blue? Could they ascribe this result to differences in k_{sigma} value?

Line 277: What exactly the authors mean with the sentence "it is impossible"? Have the authors tried to extend the "residence" time at higher T steps?

Lines 278-282: how many samples were analysed in both cases? What about effects due to differences in composition? Why the "laser derivative approach" this time was not adopted?

Lines 306-308: the conclusion is reasonable but it should be supported by results obtained with tests at different times (please show examples). The range of filter loadings is quite large (about a factor 10): how did the composition change in the analysed filters?

Lines 312-317: again, the authors claim for "several" studies but they show only one case. Please show results. How much "frequent" was the occurrence of a split point at maximum carbon peak? How much was it dependent by the aerosol composition or loading?

C707

Lines 320-324: on how many samples were these uncertainties calculated?

Lines 377-380: to serve as a tool to understand differences in carbon data reported in the literature a systematic comparison between EC/OC results obtained running EUSAAR_2 and other protocols on parallel samples is needed (and it is not present in this work). Moreover, before using this protocol in networks the uncertainties of EC/OC determination on punches from the same filters running the same protocol on different TOT instruments (i.e. in different laboratories) should be carefully evaluated (see for example results given in Schauer et al. 2003).

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