

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

A. Gelencsér (Referee)

gelencs@almos.uni-pannon.hu

Received and published: 9 November 2009

This discussion paper is an important work towards a better understanding of the historical EC/OC controversy. Its main benefits are the meticulously systematic optimization of the most critical experimental parameters, and in particular the use of artificial proxy samples to mimic the conditions likely relevant in atmospheric samples. On page 2325 the authors state that they have optimized their protocol to regional background samples: however, this remains an ill-defined category. It is well-known from recent source apportionment studies that sources of regional aerosols can be very much different in different times of the year: in winter biomass burning is predominant, whereas

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in summer the photochemical reactions between biogenic VOCs and anthropogenic pollutants can produce large concentrations of SOA which can be further aged in the conditions of photochemical smog. Therefore I miss from the test matrices listed on page 2325 biomass smoke obtained under controlled conditions, both in the flaming and smoldering phases. This would be particularly important since from the surrogate matrices inorganic salts are completely missing in spite of the fact that they had been conclusively proven to introduce substantial biases into thermo-optical EC/OC measurements. Additionally, as WSOC was shown to be mainly responsible for charring, a filter loaded with aqueous extract of ambient aerosol would have been very useful (also containing inorganic species but hopefully no EC).

I have two additional problems when any standard measurement protocol is declared to be restricted in use: first, this restriction is often overlooked in subsequent applications (see, for example, what happened with Cachier's 1989 paper, their protocol was declared to be valid only for EC/TC ratio of 0.24, but applied widely without care); second, I miss the discussion of limitations of its extension in use (i.e. what happens if the protocol is to be used for kerbside aerosol and/or biomass smoke?). I understand that such a comprehensive approach may be outside the scope of an optimization paper, but certainly further tests should be carried out before the suggested protocol can become a standardized procedure at least in Europe. I also appreciate that a fixed measurement protocol (even with limitations) is much better than having as many protocols as laboratories and therefore a mess of EC data (which are then readily used without any concern for their comparability and origin).

On page 2330, the authors state (based on references) that pyrolytic carbon (PC) has much higher sigma than native EC. If so, it would also imply higher degree of graphite-like structures, which may also affect its refractory character. To put it differently, is there any guarantee that PC fully evolves before native EC in the thermogram? The authors have done the experiments and observed significant charring then the evolution of LAC in the high final temperature He experiments (750 and 850 C). Were in

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these experiments the formation and oxidation rate of LAC comparable (particularly in surrogate samples which did not contain inorganic constituents, therefore premature oxidation of EC is not expected)?

The paper has important retrospective implications for the most commonly used EC/OC protocols, implying potentially substantial biases to EC determinations. This should be treated with caution until they are confirmed with analyses of more realistic surrogate samples (i.e. those containing inorganic species).

Minor comments: page 2328: “accordingly” - check grammar

p. 2332 line 10 “ca. 80 % only” check grammar

Fig 2. no temperature scale, no indications (a. . .d) on thermograms

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 2321, 2009.

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