EGUsphere, referee comment RC2
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# Comment on egusphere-2022-625

Anonymous Referee #1

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Referee comment on "An optimised organic carbon□/□elemental carbon (OC□/□EC) fraction separation method for radiocarbon source apportionment applied to low-loaded Arctic aerosol filters" by Martin Rauber et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-625-RC2, 2022

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The paper egusphere-2022-625 by Rauber et al. proposes a new methodology to improve the determination of the fraction of modern carbon for different carbonaceous fractions of atmospheric aerosol.

In the reviewer's opinion the paper is well written, the proposed methodology is extensively discussed, and the topic is of interest for the scientific community. Nevertheless, some weaknesses are present in the manuscript and major revisions are necessary before final publication.

********** Major issue **********

The most critical aspect concerns the determination of the EC yield required for F(EC). It is determined by comparison of ATN after WINSOC removal and the initial ATN on washed filters.

First of all, differences between optical and thermal quantification of EC by thermal-optical methods has been evidenced in the literature and it should be evidenced and properly discuss, as it strongly impacts the presented results.

Furthermore, some critical aspects concerning the correction approach in the text are not properly discussed:

- pyrolytic carbon (PyC) attenuation coefficient was demonstrated to be different from the

EC one (e.g. Yang and Yu, 2002) – and more specifically to be higher than the EC one (e.g. Boparai et al. 2008 and therein cited literature, Subramanian et al., 2006). The authors state that they assume that 50% of the formed PyC evolves in other WINSOC removal steps. Nevertheless, they do not mention the values assumed for the attenuation coefficient of EC and PyC respectively. This information is mandatory to clarify the calculation procedure – and thus the evaluation of EC yield and F(EC) correction. Moreover, as both attenuation coefficients are affected by large uncertainties, an estimate of the impact of these uncertainties on the correction scheme should be evidenced

- the lack of a standard reference material for atmospheric EC/BC (already evidenced more than a decade ago – Baumgardner et al., 2012) and of a gold instrumentation still impact all the discussion in the paper. This implies that the "true" value for the yield is unknown. Thus, it should be mentioned in the discussion and conclusions that this method is a step towards improvements in F(EC) determination – but there is currently no real way for a validation of the proposed methodology.

- it is unclear how was PyC quantified. Indeed, PyC formation can be masked by concurrent EC evolution

- evolution of not light-absorbing materials during S1-S3 steps can in principle modify ATN value even if no evolution of light-absorbing components occurs, due to the impact of not light-absorbing materials on transmittance signal related to scattering properties. This effect should be limited by sample washing, but residual effect cannot be excluded. Did the authors evaluate this effect as negligible?

In the reviewer's opinion, these limitations have to be discussed and insights into the role of these topics on the results have to be gained before manuscript publication. More in detail:
- introduction should be revised evidencing these problems;
- more investigation is needed to revise and improve the discussion in paragraph 3.1 and figure 2. The uncertainty related to the role of PyC in the evaluation of the EC yield and F(EC) correction merits to be evidenced and widely discussed in the text, identifying if it is a major or minor source of uncertainty in the proposed F(EC) correction.
- conclusions have to be extended and the impossibility of a validation of the method related to a lack of a reference material should be evidenced.

********** Minor comments **********

- Introduction: a deeper discussion on the problems concerning EC quantification and isolation should be carried out. More in detail,

- Throughout the paper, “EC measurement”, “EC data”, … are often used instead of “F(EC) measurement”, “F(EC) data”, …. This makes the reading quite confusing. Please perform thorough check and modify where needed.

Line 104 vs. line 475. Is F(WINSOC) available or not?

Line 128-129: WSOC was not analysed on back filters, whereas TC was because of quantities. Thus, do WINSOC dominate on back filters?

Lines 147-148: was possible adsorption of VOCs on the filter during sample removal and storage quantified?

Lines 169-171: how was cross contamination quantified? Did the authors test cycles of heating and cooling of zeolites to verify complete CO2 release?

Line 262: using ATN to determine EC yield can further complicate the estimate compared to the considerations

Line 346: TC on back filters look very similar to the values on the front filters. Please check (and, if confirmed, please comment on this).

Line 360: why the paragraph on the development of preparation methods is not in section 2?

Line 384-385: this sentence is obscure.

Line 409: please change “Al” with “All”

Line 479-480: how can fossil contribution be dominant if F(TC)=0.85?

Line 489: are you saying that some fossil sources emit more WSOC than EC compared to non-fossil sources? Any reference for this? If this is not the correct interpretation, what

does your result imply?

********** References **********

Baumgardner D. et al. (2012), Atmos. Meas. Tech. 5, 1869-1887

Boparai P. et al., (2008), Aerosol Sci Technol, 42, 930–948

Subramanian et al. (2006), Aerosol Sci Technol, 40:763–780

Yang, H. and Yu J.Z. (2002). Environ. Sci. Technol., 36, 5199-5204