Atmospheric
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Discussions

Interactive comment on "Highly time-resolved characterization of carbonaceous aerosols using a two-wavelength Sunset thermo/optical carbon analyzer" *by* Mengying Bao et al.

Anonymous Referee #2

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Manuscript by Bao et al. describes the annual measurement of carbonaceous aerosols in Nanjing, China, using a two-wavelength Sunset Lab. semi-online analyzer. If I understand well, this is the first article where a semi-online Sunset EC/OC analyzer with a dual laser is presented. The manuscript thus shows both an improved instrument for measuring of EC/OC and a demonstration of the newly measured value dEC, which is presented as an alternative indicator to brown carbon (BrC) measurement. From this point of view, the publication in AMT is logical. However, I have a few comments and recommendations.

General comments:

- Since this is the first described use of a two-wavelength analyzer, the article should contain more information about instrument itself. It means how it differs from previous versions of the instrument, what software is used to evaluate data, and authors also should provide a Figure with an example of a typical analysis. What are the differences, for example, in comparison with the 7-wave offline DRI analyzer (Chen et al., 2015). It is also not clear from the description in the methodology how the split between OC and EC is determined. Are there any corrections used?

- How much of the dEC roughly overlaps with the pyrolyzed carbon (PyrC) that was determined at the EC658nm split? This information can be very useful for previous studies where the PyrC level has been reported.

- What was the distribution of dEC within the EC fractions? This information would be particularly valuable for studies in which OC and EC fractions are used for PMF analyzes in determining sources (e.g., Sahu et al., 2011; Yan et al., 2019; Zhu et al., 2014).

- Many studies compare the relationship between BC and EC (e.g., Jeong et al., 2004; Karanasiou et al., 2020) to determine the relationship between quantities that are determined by different measurement methods. While BC reflects the optical properties of the aerosol, EC rather tends to define the chemical composition. However, the relationship between them undoubtedly exists (Petzold et al., 2013). An analogous relationship should be between the optically determined BrC and the thermo-optically determined dEC, which is newly defined by the authors of this manuscript. In the introduction to the manuscript, the authors outline relationship between BrC a dEC, but further in the text, they continue with this statement only as not proved hypothesis that dEC is an alternative to BrC. However, this relationship should be demonstrated from parallel BC data measured optically at different wavelengths, e.g., from aethalometer (Sandradewi et al., 2008). If the authors do not have parallel measured data from the aethalometer, then optically measured BC data can be obtained directly from the EC/OC analyzer measurements (Chen et al., 2015; Vodicka et al., 2020; Ziková et al.,

2016). The subsequent determination of BrC can then be performed similarly to Chow et al. (2018).

Specific comments:

lines 82-85: "This method has been wildly used in present studies applied in the thermal–optical transmittance (TOT) Sunset carbon analyzer based on NIOSH protocol or thermal–optical reflectance (TOR) Desert Research Institute (DRI) carbon analyzer based on IMPROVE_A protocol (Ji et al., 2016)" First, rather "widely" than "wildly". Second, the sentence is not completely correct because it is possible to use both NIOSH and IMPROVE or another temperature protocol on both devices from Sunset or DRI. Third, in a referenced paper by Ji et al. (2016), there is nothing about the IMPROVE_A protocol. About IMPROVE_A protocol is a paper by Chow et al. (2007).

line 110: "Wang et al. (2011) used a two-wavelength Aethalometer...." The original work in which this type of BC distinction between wood burning and traffic emission was used is Sandradewi et al. (2008), which should be noted. Further, a reference to the work of Wang et al. (2011) is not in references.

line 102: "Italian Apennines" are quite broad area. Massabó specifically states the Ligurian Apennines, Italy. By the way, reference to the work of Massabó et al. (2016) disappeared from references in second version of manuscript.

line 116: "important situ data" change to "important in-situ data"

line 148: "January-April in 2017" Why the authors report periods that they do not use in the evaluated data?

line 153: "...collected on prebaked quartz fiber filters..." Indicate temperature and time of filter prebaking.

line 170: "of ∼ 17mm" Filter diameter in Model-4 from Sunset Lab. is usually 16 mm. Is it different in the new type of device with two lasers?

line 172: "modified NIOSH 5040 protocol" If the used NIOSH protocol was modified, the authors should describe it in detail (step, time, temperature) or give a reference where the protocol is described. In addition, the authors should explain why they chose NIOSH protocol which usually underestimate the EC (e.g., Bautista VII et al., 2015; Chow et al., 2001).

line 193: "We also did the measurements of OC and EC in PM 2.5 filter samples using the same method followed by the NIOSH protocol." Were offline measurements also performed on a two-wavelength analyzer? If yes, please provide a comparison of the dEC. Second, were both data sets corrected (or uncorrected) to blank measurements before comparison?

line 202: "(Draxler and Hess, 1998)" Authors of HYSPLIT prefer newer citations to their model - see: https://www.ready.noaa.gov/HYSPLIT_traj.php

line 233: "The average OC/EC ratios in this study was 3.6, which was lower than most of those reported in other studies..." This ratio depends, among other things, on the protocol used. If you are comparing with other studies, they need to use also the NIOSH protocol. If you compare with studies where they use, for example, the IMPROVE protocol, which generally analyzes higher concentrations of EC than the NIOSH protocol, then the OC/EC ratio between these studies is different.

lines 271-273: "The OC/EC ratio could give some information about primary and secondary organic carbon (Turpin and Huntzicker, 1995; Lim and Turpin, 2002)" The OC/EC ratio can be a rough indicator of the presence of primary and secondary carbon aerosols. Usually, this analysis is based on the determination of the OC/ECpri ratio and is applicable under certain conditions (Pio et al., 2011). If the authors want to discuss the share of primary and secondary OCs, they should use some more recent approach – see, e.g., Wu and Yu (2016).

line 294: "...shown in Fig. 5. We also found similar distributions in dEC/OC and OC/EC." Not clear what authors mean by similar distribution in dEC/OC and OC/EC. In Fig.

5, color maps of these two parameters are different. Moreover, the conclusions that authors draw from it are not very clear from Figure 5. Maybe some other depiction of temperature and RH dependence would be more appropriate.

line 303: Replace "aass" by "mass".

line 323: Replace "Local" by "local".

Table 1: Here it makes sense to add average values for the seasons as well. Especially when you mention these seasons in the text, in Figures or in Table S2. And replace "media" by "median".

Figure 6. In what kind of software was the visualization of Figure 6 done? If in OpenAir, it should be quoted – see Carslaw and Ropkins (2012).

Figure 7. In what kind of software was the visualization of Figure 7 done? If in Zefir, it should be quoted - see Petit et al. (2017).

Table S1: This table provides only sketchy data, which are difficult to compare without context. First, it is necessary to distinguish between cities and countries. It is clear that, for example, Spain is smaller than China, but even so, there are different types of sites with different levels of concentrations (e.g., Querol et al., 2013; Sánchez de la Campa et al., 2009; Viana et al., 2006). The same for Italy... Second, similar sites and the same aerosol fraction should be compared (there is a difference in OC/EC ratio for PM2.5 and PM10). Different aerosol PM fractions should be mentioned in the table. There are usually also differences between seasons (typically winter vs. summer) so comparing different periods, for example, annual data with a month of winter data, is also little bit misleading. Third, if other temperature protocols are used in referenced studies, this should also be mentioned, as it also affects the OC/EC ratio. Contrary, if all the cited studies were analyzed by the TOT method, it is not necessary to repeat it in the table and it is enough just to mention it in a table legend. Figure S1: Indicate on which axis the online and offline data are displayed.

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