Review report AMT 2019-5 manuscript version 1

We strongly acknowledge the Referee for the valuable and precious comments and suggestions.

The paper describes the application of a modified Sunset Lab Inc. EC/OC analyzer with a two wavelength set-up for analysis of ambient aerosol samples. It strongly relates to earlier work of the author which is described detailed in previous publications, nevertheless it extends to additional findings. The results derived from the comparison of two temperature protocols, NIOSH5040 and EUSAAR2, for the 405 nm wavelength as well as the reported MAC values are a valuable addition to the literature. However, the paper lacks in structure and suffers in use of proper scientific English which limits its potential. It is recommended that the text is reviewed with focus on syntax and vocabulary. Particular attention should be given in the first paragraph of the abstract and the first two paragraphs of the introduction. Further:

We tried to improve English for both syntax and vocabulary. In the following, a point-by-point reply to all the comments is given.

Sentence starting in line 115: "The hypothesis under such choice…" should be rephrased.

Done

The terms "real samples", "real-world samples" and "real-world aerosol samples" are used throughout the text. The use of one term is recommended.

We changed using the form "real-world aerosol samples throughout the text.

"λ=" , "@λ" and "@λ=" are used to state wavelengths. Please consider using one form (λ=) for consistency.

Done

Sentence starting in line 59 should be revised. "Standarized" should be replaced by standardized. Since 2017, when EN16909 was published, there is uniformity in OC/EC analysis methodology, at least for EU.

Done

Paragraph starting in line 157: Any specific reason why this subset was analyzed with EUSAAR2 only?

After the first positive results with pure Aquadag samples and considering the slope close to one, we preferred to make a "reproducibility study" (i.e. we used 2 punches for each laser in each sample) by EUSAAR_2 protocol. We got a very good agreement and final results only have been reported in the text. We'll make clear this point in the revised text.

Paragraph starting in line 165: PM10 samples are known to add complexity in OC/EC analysis due to minerals, refractory material and oxides present in coarse fraction. Did you consider sampling/analysis of PM2.5 samples and have you noticed any of the above interferences?

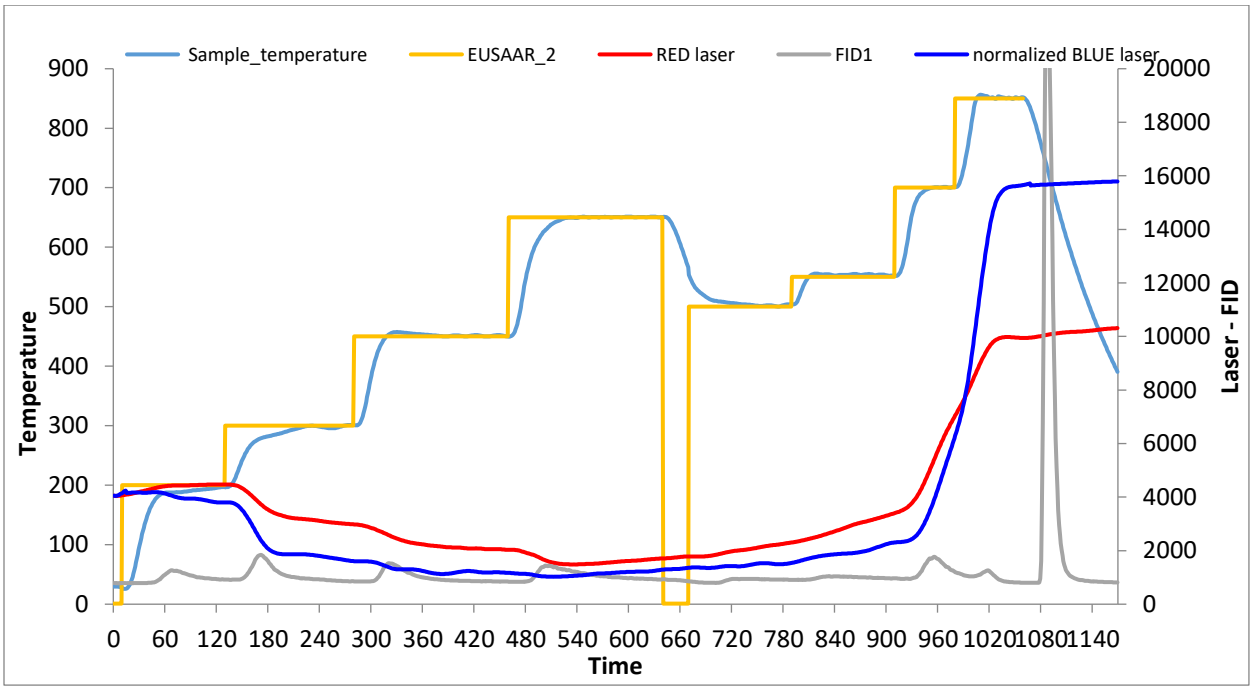
This point is very interesting, we thank the Referee for this valuable suggestion. Unfortunately, we had not the possibility to apply our methodology on PM2.5 samples but for sure we will try in the next future.

Line 174: It sounds like two different subsets were created, one for analysis with EUSAAR2 and one for NIOSH5040. If that is the case, why was that choice made instead of all samples being analyzed with both protocols?

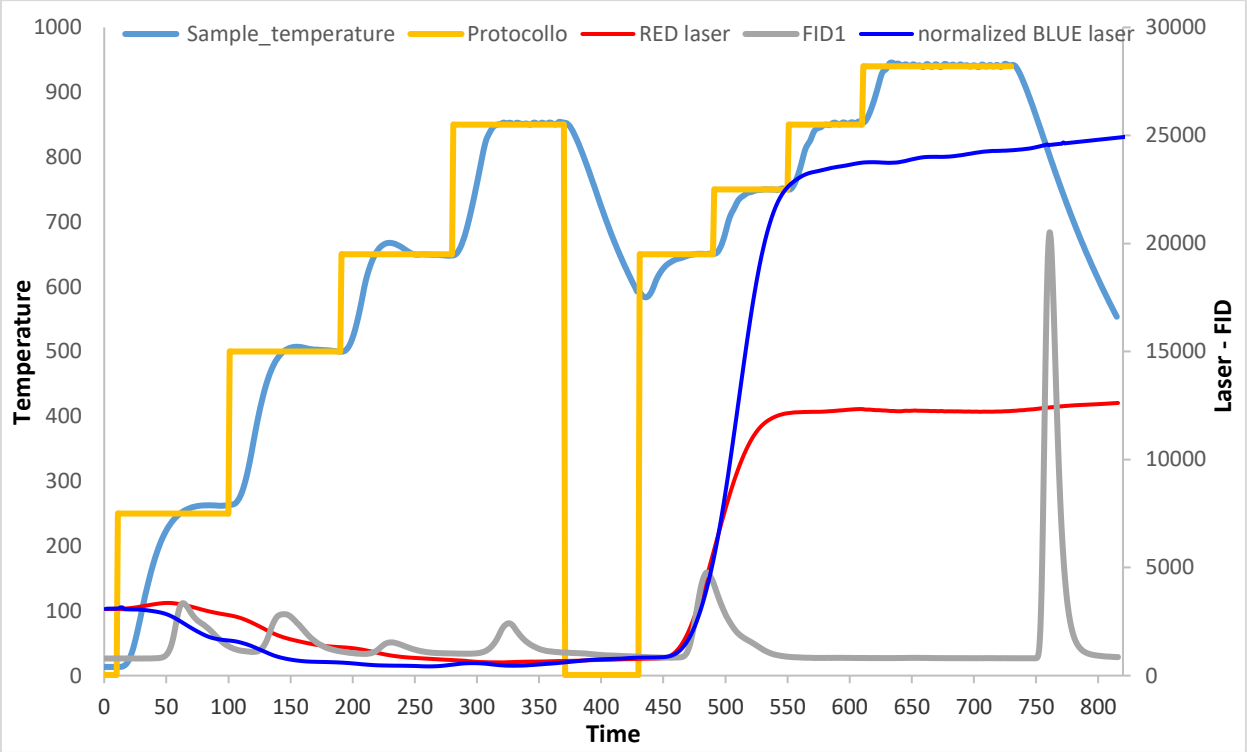
As explained above, we preferred to have a reproducibility check by measuring 2 punches for each laser in each sample.

Line 193: It is not clear to me why the discrepancy between EUSAAR2 and NIOSH5040 is mainly driven by charring. In a sense more pyrolytic carbon would result in a later split point and less EC reported. Further, since the blue laser diode resulted in later split points for EUSAAR2, wouldn't that rate it as less sensitive to charring instead of more, as mentioned in the text?

Actually, as shown in the example below (please note that the red and blue transmittance are normalized at the same initial value), in the EUSAAR_2 thermograms we observe a steeper decrease of the "blue" transmittance in the first phases (low temperatures). This corresponds to a higher sensitivity to pyrolytic formation when we use the blue laser diode.



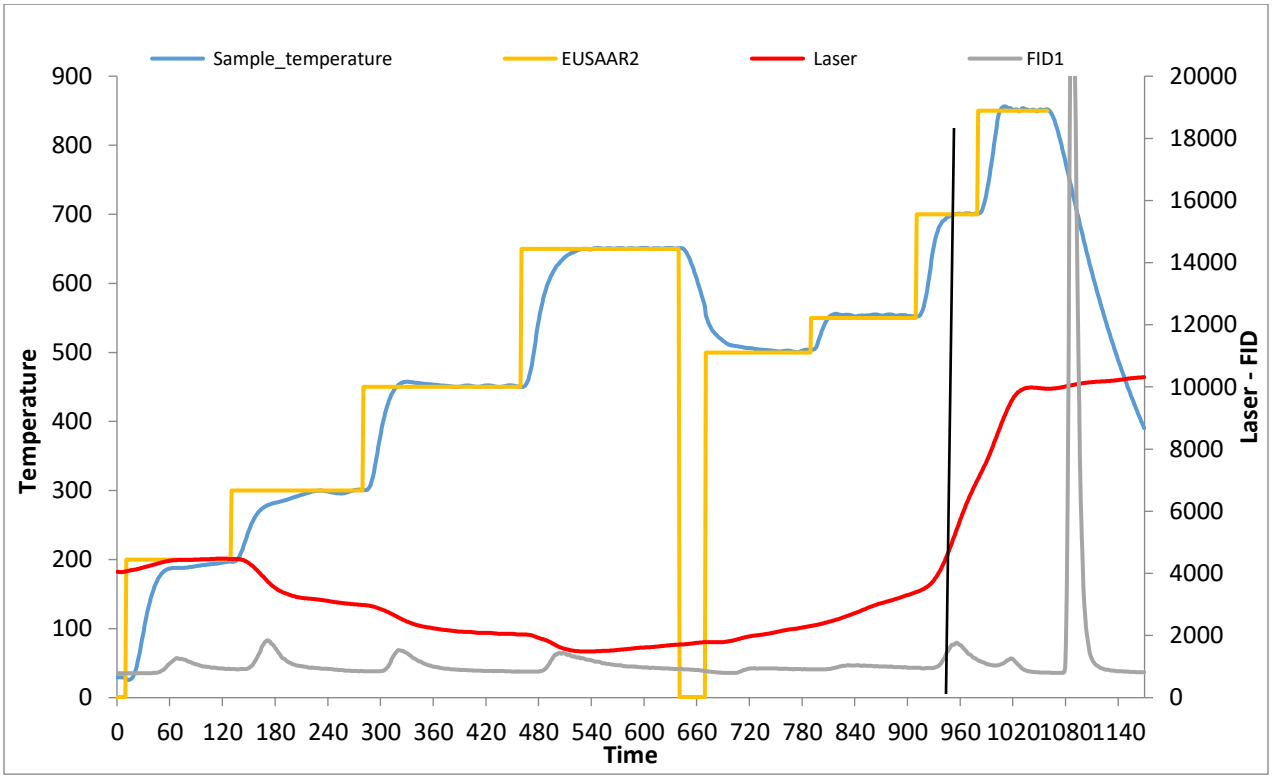
EUSAAR_2



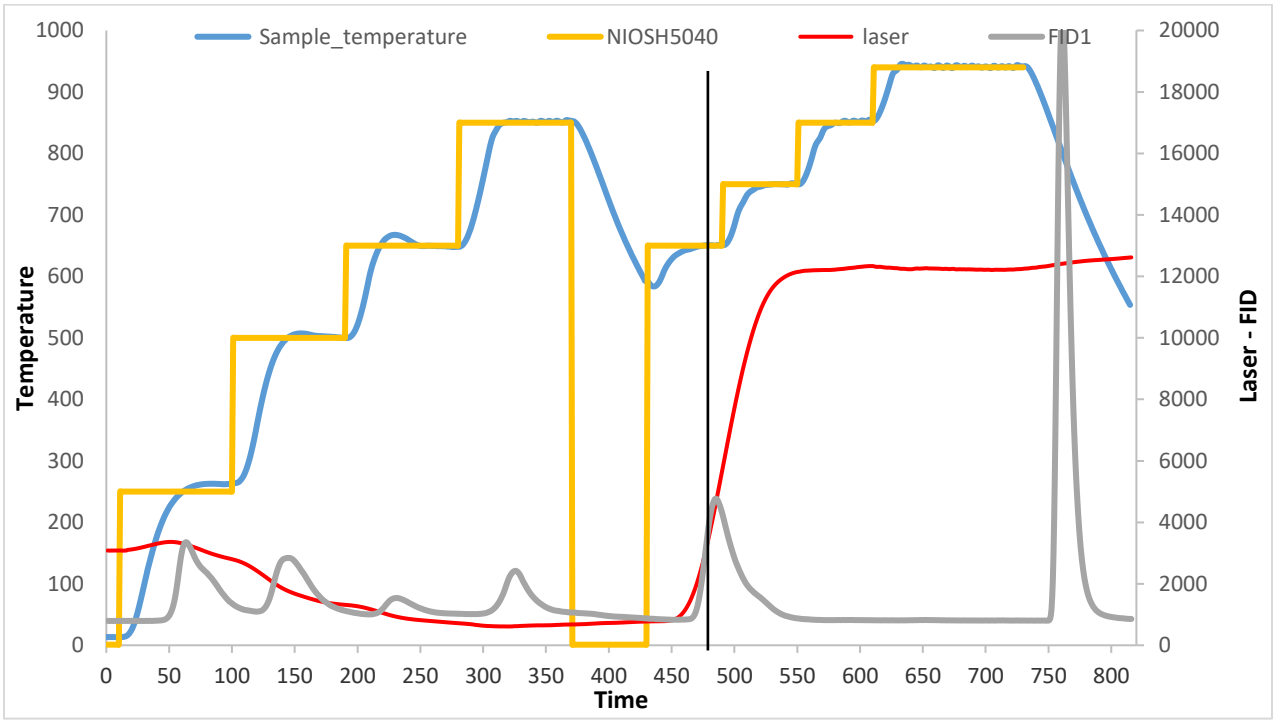
NIOSH5040

Is it possible to include a figure and/or representative thermograms that illustrate the consistent 40% discrepancy between EUSAAR2 and NIOSH5040?

As reported in the text we unfortunately had not the possibility to repeat the NIOSH and EUSAAR_2 protocols at the two different wavelengths on the same filters. This is why we based our discussion on the analysis of literature and previous results. We anyway include here two typical thermograms, NIOSH and EUSAAR_2, of the same urban samples batch and showing the typical transmittance trend linked to the quoted discrepancy.



Eusaar2, split point ≈ 950 s



Niosh5040, split point ≈ 480 s

Line 267 and elsewhere in the text: The term “Sunset set-ups” could be altered to a more descriptive term.

Done

Line 289: What would be the value of 1σ? It seems that the difference between the two MAC values reported is substantially greater than the reported uncertainty.

The Referee is right, we did a material mistake in the text: the one sigma uncertainty is 0.4 $m^2 g^{-1}$ (and this is right) but the discrepancy between the MAC values calculated in 2016 and the present work is slightly above 3 sigma (we change the text accordingly).

Line 316: “Brow carbon”

Done

Line 338: This sentence could be rephrased for easier comprehension.

We rephrased the sentence in this way: “We retrieved Brown Carbon concentration values directly from the Sunset thermograms following Massabò et al., 2016. Exploiting the synergic information provided by the Multi Wavelength Absorbance Analyzer, MWAA (Massabò et al., 2015) we could obtain the MAC(BrC) at the two wavelengths”.

Line 348: Same stands for this sentence.

We rephrased the sentences in this way: “In our findings, the ratio between BrC and Levo concentration values depends on the wavelength of the transmittance signal adopted during the thermo-optical analysis. This behavior could be due to 1) a better accuracy of the results in blue-light, more sensitive to BrC, or 2) the definition of BrC itself, which has to be considered wavelength-dependent. The present results do not allow any conclusive statement on this issue: actually, the label “Brown Carbon”, as well as the widely used “Organic and Elemental Carbon”, comes from an operative definition not without ambiguity.

Figure 4: It is not clear which relationship applies to which trendline.

Figure 4 has been amended.

Figure 6: It seems that 2 separate subgroups are formed, one equal and above the trendline and one below the trendline. Are those related to the specific sampling strategy or to any other parameter?

We noted this strange behavior with the formation of two separate subgroups. It doesn’t depend on different sampling strategy neither other evident parameter. Unfortunately, we didn’t find a reasonable explanation for this.