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Comment on amt-2021-46

Anonymous Referee #2

Referee comment on "Determination of the multiple-scattering correction factor and its cross-sensitivity to scattering and wavelength dependence for different AE33 Aethalometer filter tapes: a multi-instrumental approach" by Jesús Yus-Díez et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-46-RC2, 2021

Comments on « Determination of the multiple-scattering correction factor and its cross-sensitivity to scattering for different AE33 Aethalometer filter tapes : A multi-instumental approach » from J. Yus-Díez et al.

Filter-based absorption photometers are widely used because of their simple operating procedures. All these instruments suffer from several artifacts, from which the multiple-scattering effect is the greatest. This paper compares measurements from AE33 with two different tapes to a more reliable filter-based photometer (MAAP) and to an even more robust instrument, the PP_UniMI polar photometer. These comparisons allow determining the multiple-scattering constant for urban, rural and mountainous environments and the cross-sensitivity to scattering. A dependence between the C value and the single scattering albedo was also found that relates to some extent to Saharan dust outbreaks. These results are a very important step towards a better understanding and correction of Aethalometer data.

Main comments:

- In the methodology part, four different C constant are described (instr, lambda, f and eff). Moreover a new Cconst is introduced at page 16.The first point is that the introduced abbreviation are sometimes "rephrased" or misused (e.g. C(C(λ)) or "the wavelength-dependent C"). But the most important point is that most of the result section just mention C without any specification. This can lead to misunderstanding and does not help the reader to easily understand this technical study.

- The influence of Saharan dust outbreaks on the Ceff values and on its wavelength dependence is clearly demonstrated in sections 3.2 and 3.3. Section 3.1 already introduces and discuss the dust influence but without real proofs. I think that the structure of the paper should be reviewed (to some extent) so that misinterpretations of results of section 3.1 are removed (see the 11 comments I had on section 3.1 during the reading). This can perhaps be solved by a result section followed by a discussion section or by another ordering of the results description.
- You had access to a reference instruments to study the behavior of the multiple scattering constant at three stations with different aerosol composition. A section describing the best way to correct AE33 for the multiple scattering constant as a function of e.g. the type of aerosol, the SSA, the presence of dust as well as if scattering coefficient or MAAP data are available would help the reader and increase the value of your paper.
- The impact of the two different filter tapes should be also summarized anywhere.
- Finally a lot of minor points such as 1) English language, 2) coherence of parameter's abbreviations between the figures and the text, 3) repetitions in the results description, 4) right citations (i.e. cite the right paper in the right context) should had been corrected before the first submission.

Minor comments:

- 1 line 10: C(λ) is not only the parameter with the greatest uncertainty but above all the parameter with the greatest impact on the determination of the absorption coefficient.
- line35-37: I'm not sure that no standard aerosol particles are available for instrument calibration. Polystyrene balls are available to calibration scattering measurement. For absorption, some recent developments are also available (e.g. https://www.tandfonline.com/doi/pdf/10.1080/02786826.2018.1536818?needAccess=true). Please verify and if necessary correct this sentence.
- line38-39: this is already the case in part of the Europe and in north America (https://acp.copernicus.org/articles/20/8867/2020/)
- 1 Measurement site: a reduction of the number of cited references could really help the reader to gain time in searching information related to the stations and the aerosol sources.
- Line 195, replace ";" by "," after (2014)
- Line 238: Schmid et al (2006) did not report a correction including directly the scattering coefficient. He used the SSA value as equ (8). Contrarily to Schmid, Arnott substracted a fraction of the scattering from bATN before to divide by β (corresponding to the multiple scattering constant). Segura took the Schmid algorithm. Please clarify
- Line 241: introduce f(ATN) directly in equation 4 for clarity purpose.
- Line 242: using AE33 the filter loading correction is performed by the instrument. Why is it mention here? Please give your own argumentation/measurement to say that f(ATN) can be assumed to be close to 1 since your correction without loading correction

is not similar to Schmid one.

- Line 253: which other (than scattering) cross-sensitivity could be taken into account? The sentence line 251-253 would be clearer if reformulated.
- g. Lines 256 and 257, 260 (please check the whole document): for your own correction, you defined up to now Cinst, Clambda, Cf and Ceff. Please use one of these instead of "C".
- Lines 257-259: Similarly to scattering coefficient, absorption coefficient can also depends on the particle size and mixing state. Nothing is wrong and it is good to study the dependency on the shape, size and mixing state. Anyhow this § is not scientifically and grammatically precise and it should be reformulated.
- Lines 260-270: please mention the measuring time granularity of each instrument.
- Line 274: the scattering coefficient was probably also similarly averaged?
- Line 288-292: this is partly the same information. Please reformulate these sentences. I also don't understand "In the latter case" which seems to relate to the Deming regression. So you used two methods, the Deming including the uncertainties of the instruments and another one using the statistical density distribution of the C factor?
- Line 295: the median is done with all the simultaneous measurements of the three instruments? How is the uncertainties computed? There is a mixing between Deming method and averaging that should be removed. Please give the results after the description of the method and end with a comparison between them. Similarly you give the results first for M8060 and then for M8020 for the median and in inverse the order for the Deming method. This makes the comparison much more difficult.
- Lines 302-304: The median method leads to larger values that can reach up to 22% (M8060 for MSA) of the Deming method. This cannot be considered as marginally.
- Line 304: medians are precisely used to be not sensitive to extremes!!
- Figure 1: mention in the figure caption that the C (once again, which one?) comes from the medians. It could have been computed from Deming for e.g. each day of simultaneous measurements.
- Lines 307-308: you didn't use the same data for both methods? If this is right, how can you compare the results?
- 10: What shall we trust/use, C from median or Deming? You have to discuss it and give your opinion about the best solution. If the Deming method allows detecting sensitivity to scattering by the value of the intercept, does it mean that you don't used Ceff that is corrected for the scattering? If you do not use Ceff, I don't understand which one you used and why you present this before the Ceff results. If Ceff is effectively used, why is there a remaining scattering sensitivity?
- Lines 318-322: Do you mean that both Saharan dust outbreaks and secondary organic aerosols/sulfate particles increase both the particle scattering efficiency and SSA? OK for the secondary particles, but dust particles absorb light and are usually/partly big particles. The impact of dust particles on SSA is then not straightforward.
- Lines 322-324: to my opinion, C is more constant at MSY than at BCN. The variability for each season is lower and the inter-season variability is quite similar.
- 11 and seasonal cycles: Does the seasonality of ABL influence at MSA also influence the C seasonality? How do you explain that C from M8060 is larger in JJA at MSA but not C from M8020 ? How do you explain that the variability of C at BCN is smaller for M8020 than for M8060 and that the inverse situation (larger for M8020 than for M8060) at MSY? Do you find a diurnal variability of C that could be bounded to traffic intensity at BCN or to ABL influence at MSA?
- 11 and seasonal cycles (bis): You explain that secondary particle formation and Saharan dust outbreaks increase the variability of C. I think that a specific analysis of time with secondary particles/dust (and perhaps other parameters) would be very interesting and could help other stations with similar influences to determine their C values.
- Line 338: you cannot derive SSA value from the solely AE33.
- Figure 3: these results correspond to which tape? Why do we have Ceff comprised between 2-2.8 in Fig. 2 for all the stations and between 3-3.5 for BCN, 2.5-3 for MSY

and 3.2-4.5 for MSA? The comparison with PP_uniMI is done on another set of data, but this large difference should be explained.

- Line 340: If I remember well, Weingartner had not really the possibility to check the wavelength dependency of C with a reference.
- Line 344-348: what is the relation with the C wavelength dependence since the loading correction is already applied in final AE33 data ?
- Line 351: Is it really necessary to introduce the abbreviations ac and ak since they are used only in this part of the study?
- Figure 4 for MSA: there is no continuous increase of ac with SSA, but a shift at about SSA=0.95. Between SSA=0.85 and 0.94, there is even a very small decrease. Similarly there is no decrease with increasing BF since the second point (BF=0.1) is the lowest one. Regarding SSAAE, the two first points are clearly higher. Please give a better description and argumentation and perhaps a value for the statistical significance.
- Figure 4 (bis): I do not understand why there is an ac value of ~1.75 in subplot i) and not in subplots c) and f). Did you use another dataset? Similarly, why the ac value of 0.5 appears only in h) and not in b) and e) ?
- Line 358: one of the lowest ac value corresponds also to a negative SSAAE. Please comment.
- Figure 4 and p. 14: *similarly to the discussion on the seasonal cycle, I would suggest you to isolate Saharan dust outbreaks to compute the wavelength dependence of C for these peculiar events. It would be very useful for the reader to know e.g. that the value of the C wavelength dependence in presence of dust.* **OK this is done in Fig. 5 (see my comment on reorganization):** The results proven in Figure 5 should be discussed only with Figure 5 and not hypothetically with Fig. 4. Please reformulate all the discussion since the conclusion is that other particle properties contribute to positive ac observed at mountain top.
- Figure 5: statistical significance as in Fig. 3 would be valuable.
- Lines 376-378: already explained in the previous §.
- Lines 379-391: a clear description of Fig. 4s should first mention that ak decreases continuously up to SSA=0.9 and increases rapidly for SSA larger values @ BCN (and idem for the other stations). Then you can describe the shift between positive and negative ak. Please note and comment that ak is also positive for SSA> ~0.96 so that very bright aerosol have a similar k behavior as very dark ones.
- Line 383-384: what are the potential conclusions from this sentence "The relationship between k and SSA was similar to that between ak and SSA"?
- Line 400: and for 470 nm ?
- Line 413-414: I think that at this point the reader is well aware of both types of filters. You can avoid the description.
- Table 1: please specify if the C reported by other authors is computed similarly to your's and to which of your C it corresponds.
- Figure 7: Is there a unit problem between figure 7 and the text ? the ms values are a factor 100 larger in the text.