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Interactive comment on "A comparison of three optical absorption photometers at a boreal forest site – effects of different correction algorithms" *by* Krista Luoma et al.

Anonymous Referee #2

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The authors present an interesting intercomparison of filter absorption photometers at a regional background site at Hyytiälä, Finland. The comparisons of this kind are important as a full characterization of the instrumental response is lacking and is compounded by the interwoven non-linearities of the measurement, which presents themselves as measurement artifacts, or as the authors' call this, systematic errors. The comparison of the MAAP, the AE31 and the PSAP partially addresses these shortcomings and presents new viewpoints on an urgent topic.

The manuscript fits well with the scope of AMT and can be accepted for publication after addressing the following major and specific comments.

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Discussion paper

The authors correctly point to the influence of the correction algorithm and its effectiveness on the slope of the inter-instrumental regression, which is used as the multiple scattering correction factor (Cref). The loading effect and the multiple scattering are artificially separated in the correction algorithms. Additionally, the particles, embedded in the filter, cause a known cross-sensitivity of the filter photometers to the scattering, which is explicitly described in the Arnott et al (2005) algorithm. Filter photometers also feature a dependence of the sensitivity on the location/depth of the particles in the filter matrix and are their sensitivity is therefore dependent no the size distribution of the sampled absorbing particles.

Weingartner et al. (2003), Park et al. (2010), Hyvärinen et al (2013), Segura et al (2014) and Drinovec et al. (2015) have discussed different approaches to showing the magnitude of these artifacts and their dependence on the loading of the sample spot. The authors should follow the same principle and plot the attenuation and absorption coefficients, and the absorption Angstrom exponent (AAE) as a function of the loading of the sample spot, for example as a function of ATN, Tr, ln(Tr)... for all filter photometers in the study. This will also serve as a strong argument for using the MAAP as the reference.

Specific comments

Page 2, Lines 30 – P3, L5: There is another systematic error, not considered by the authors – the measurement of flow. The first issue is the reporting conditions of the flow: have they been unified across all instruments? If yes, please state the conditions in the respective Measurements and methods sections. The authors should include a word of caution for the instrumentation and the determination of the leakage – this is a multiplicative factor affecting the slope between instruments, which is (in the experience of the reviewer) often interpreted as being intrinsically instrumental.

P3, L 7-15: Please add the discussion on independent check of the correction algorithms with references to Park et al. (2010), Hyvärinen et al (2013), Segura et al (2014) and Drinovec et al. (2015).

P4, L 16: Please add the widths of the different “wavelengths” in the filter photometers (for example from Müller et al., 2011).

P4, L 21: The reference to Fig. 1 is to a very nice picture of the experimental setup (which should remain in the manuscript) and not to the missing data availability plot. This missing figure could be added to the Supplement.

P4, L 21-28: It is RH change that perturbs the filter measurements, not RH per se. It would be interesting to take into account the RH change rate as well. For example, plot a companion to Fig. 4 with RH change rate, same for other instruments.

P4, L 30: Reference to Fig. 2 is in fact reference to Fig. 1.

P 5, L 8-10: Add the information on the filter material used.

P 5, L 17-18: This is incorrect. The intensities in PSAP and AE31 are normalized to the intensity measured under the clean part of the filter – the reference sample spot. This takes into account any possible drift in the LED intensities during the measurement period.

P5, L28: The sample spot should be measured. It changes with each spot slightly, especially due to leakage, when the filter tape is not well sealed. Was the correction for the differing values of A taken into account in this work?

P 6, L 6-12: The loss of sensitivity due to non-linear effects could be presented better. Please rewrite.

P 6, L 19-20: MAAP artifacts can be checked by a BC(ATN) plot, please see above. This justifies the use of the MAAP as the reference (further below, next paragraph).

P6, L 29: The authors talk about the precision here, not accuracy. Accuracy, however, is the parameter which is of importance. Please see above regarding the justification of the MAAP as the reference.

P 7, L 5: The unit-to-unit variabilities of different aethalometer types is very different - please expand and reference Müller et al. (2011) and Cuesta et al. (2020).

P 7 – 11, sections 2.3._ I disagree with the Anonymous Referee #1, these sections are important for understanding and interpretation the rest of the paper and should remain in the body of the manuscript.

P 8, L9: Please define single-scattering albedo as “omega”.

P 8, L 13: Why linear dependency – compare Virkkula et al. (2007 and 2015).

P 8, L 13: Please define Angstrom exponent as “alpha_sca”.

P 9, L 3, “. . . were calculated from. . .”: Not clear if this relates to o the Hyytiälä measurements or to the Arnott et al. (2005). Please rephrase.

P 10, L 27: Which PSAP filter do these values relate to (Ogren et al., 2017)?

P 11, L 20-21: The averaging of PM1 and PM10 values is non-trivial due to possible regional contributions to BC in the larger size fractions. This does influence the non-linearities, which in-turn cause measurement artifacts that need to be corrected. The introduction mentions no change in the size of the sampled particles. The authors mention this briefly in section 4.1. Please add this information and provide an argument and discussion how this could influence the comparison.

P 13, L 10-11: Why calculate C_NC? It is loading dependent.

P 13, L 12-13: AAE is an absorption property, attenuation features loading effects, making AAE impossible to calculate, especially measurement at the lower wavelengths are heavily loading impacted. This paragraph needs to be extended and additional explanation on the determination of AAE provided.

P 13, L 20: Please extend the description of the fit – regression, it is not completely clear, cite Eq. 19...

P 13, L 21-22: The wavelength dependence of C is discussed in Bernardoni et al. (2020), which can be added to the discussion below (section 4.1, especially P 15, L24), provided it is calculated here.

P15, L 7-8: This is the place to discuss the influence of the correction algorithm performance on the C.

P 16, L 16-19: C_ref is the effective slope relative to the MAAP. Please add some discussion on the artifacts of all methods and their similarities/differences. What about size distribution artifacts? See also P17, L7.

P 16, L23-24: Or it describes the variation of the artifact better. Is this dependence on the parametrization scheme? Averaging?

P 17, L 7-8: This can be quantified, there are relevant measurements at Hyytiälä. Please provide this information.

P 17, L 14-17: This can also be described in a more quantitative manner, please see Virkkula et al. (2015) and Drinovec et al. (2017).

P 18, L 10: The intercept of the linear fit is the scattering artifact.

P 18, L 21: The data featuring low ATN is the one which features low loading artifacts and, therefore, a C with less uncertainty. This can be explored and the uncertainty as a function of the loading determined quantitatively.

P 18, L 27-29: The "smoothing" is site dependent and the non-corrected regression slope is always lower. The r2 of the non-corrected regression nis lower as well. Please discuss.

P 18, L 32: This is surprising, as one would expect that at low loading, the influence would be minimal. Is this a parametrization effect. Please discuss.

P 19, L 1-2: Please elaborate, the text is unclear.

P 19, L 18: This is different than explained above, Eq. 16. It is actually much more quantitative, as it allows the selectin of “good” AAE values by evaluating the fit r2, and ignoring the AAE values with low r2. This is used in French monitoring networks as a parameter to quality control the data and source apportionment of BC. Please use the r2 AAE selection and add this information in the manuscript.

P 19, L 31 – P 20, L 2: Please see above and Bernardoni et al (2020) and add to the discussion.

P 20, L 30: What was the maximum AE31 ATN for advancing the spot? Please add to the instrumental section.

P 20, L 31: Is this an observation of the data reported here (circular reference?) or an observation of Virkkula et al. (2007 and 2015) and Drinovec et al. (2017)?

P 20, L 32: This is not true. The correction algorithms take care of this. AAE dependence on ATN means that the loading correction is not working well. This is crucial as it shows that, except for V2007, the loading corrections do not function well! This is surprising, as this is the only correction not taking into account the cross-sensitivity to scattering. Why is “wavelength dependent k” better than other parameterizations? Same should be done for b_abs.

P 23, L 10-12: This depends on the rate dATN/dt, or the number of spots measured. This number can be counted and can be provided here. It is a good parameter for quality control and an important finding of the manuscript.

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