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Atmospheric Measurement Techniques Discussions

Interactive comment on "Determination of black carbon mass concentration from aerosol light absorption using variable mass absorption cross-section" *by* Weilun Zhao et al.

Anonymous Referee #2

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The study that is discussed in this manuscript is focused on developing a more robust methodology for the derivation of the mass absorption cross section (MAC) that relates the mass of black carbon to its light absorbing properties. As correctly explained by the authors, the MAC can't be a constant such as typically applied by the majority of those users who deploy instruments that measure the light absorption of atmospheric particles. The primary objective of the current study is to derive a MAC that depends on the size of the BC core and thickness of the shell, with some assumptions about the real and imaginary refractive indices of the mixed phase particles. There is nothing fundamentally incorrect about the approach that they take but there are so many problems with the manuscript itself that I am unable to accept this paper without some

major changes as I list below.

1) The authors have ignored the recommendations as proposed by Petzold et al. (2013), recommendations that are generally accepted by the scientific community, on how black carbon (BC) should be reported when derived from instruments that measure light attenuation, i.e. filter based or photoacoustic sensor. BC derived from these techniques should be reported as equivalent BC, or eBC, If or when this paper is resubmitted, the title should reflect clearly that it is eBC that is being discussed, not BC.

2) A large fraction of the introduction is devoted to the importance of BC for climate change due to radiative forcing. What the authors fail to understand is that in the context of their study, the corrections to the MAC that they are proposing is completely irrelevant. Sensors that measure light absorption like the Aethalometer, are already providing the necessary information that is relevant to climate change, i.e. it is not the mass concentration that is important it is the optical cross section. I will address this further below with respect to the mixing state of BC, but the primary point is that the mass concentration of BC is not important when doing radiative transfer calculations if you already have the primary measurements of the coefficients of scattering and absorption. The authors also mention that BC might be efficient CCN or IN, both true statements but again irrelevant with respect to their study. Hence, the introduction needs to be completely rewritten to explain the real relevance of the current study, and that is to set some error bounds on eBC derived from Aethalometer measurements and NOT a cutting edge, new methodology that will in any way improve the accuracy of such measurements.

3) This study should be written up as a detailed analysis of the uncertainties in the MAC related to the mixing state of BC, i.e. the refractive indices, real and imaginary, the wavelength of incident light, and the relative sizes of the core and shell. Secondly, in the introduction, it should be made quite clear how this analysis differs from the many others that have already been published.

4) The methodology that is discussed in this paper is being promoted as a way to derive a more accurate eBC but this is misleading because in order to apply this you need a lot of additional complementary information about the size distribution of the BC, the fraction of particles that are mixed with BC, etc. If you had all the necessary information to begin with, then you wouldn't even need to try and derive eBC using a variable MAC because you would already have enough information to estimate BC without the light absorption instrument. This should be made quite clear in a resubmission of this paper.

5) It is my opinion that the modeling that is being discussed with this study has as much importance for setting the error bars on light absorption derived from the filter based measurements as for setting error bars for deriving eBC. There are many corrections that have been proposed to adjust the light absorption measurements for the impact of overloading, filter matrix effects, etc., but perhaps the results from the current study could also be used to establish how mixed state BC leads to under/over estimates of the absorption coefficient. The authors should give this serious consideration if they want their study to have more relevancy than it does in its current state.

6) The sensitivity studies of how the assumptions impact the results have to be in the main text, not in the supplemental material because these sensitivity studies are critical to the conclusions that are drawn. The current sensitivity studies are too limited and do not take into account all of the assumptions that go into the model. The validity of every assumption must be defended and used to set the limits of the errors eventually reported.

7) I am providing an annotated copy of the PDF with additional comments, questions and suggestions to be addressed by the authors.

Reference Petzold, A., Ogren, J. A., Fiebig, M., Laj, P., Li, S.-M., Baltensperger, U., Holzer-Popp, T., Kinne, S., Pappalardo, G., Sugimoto, N., Wehrli, C., Wiedensohler, A., and Zhang, X.-Y.: Recommendations for reporting "black carbon" measurements, Atmos. Chem. Phys., 13, 8365–8379, https://doi.org/10.5194/acp-13-8365-2013,

2013.

Please also note the supplement to this comment:
https://amt.copernicus.org/preprints/amt-2020-337/amt-2020-337-RC2-supplement.pdf

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