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Atmospheric Measurement Techniques Discussions

Interactive comment on "Multiple scattering correction factor of quartz filters and the effect of filtering particles mixed in water: implications to analyses of light-absorption in snow samples" *by* J. Svensson et al.

Anonymous Referee #2

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Svensson et al. (AMT-2019-142) present a laboratory study of filter-based absorption measurements, designed to provide correction factors for studies on light absorbing particles (LAP) in snow and ice. LAP in snow and ice have previously been measuring by melting and filtering the samples, followed by measurements of transmission through such a sample, from which absorption can be estimated. The major goal of this study is to calibrate such measurements, however, this study did not use representative particles of atmospheric LAP.

First, dust (an extremely important LAP type) was not considered. Furthermore, for the

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Discussion paper

LAP type focussed on here (black carbon), an inappropriate surrogate material was used. The authors used soot deposits from chimney walls in Helsinki. This is inappropriate, because these soot particles will have coagulated to form new and unique morphologies (larger and more densely agglomerated particles; Dhaubhadel et al., 2006) that do not represent atmospheric black carbon. My criticism is proven by the authors' Figure 2, where substantial numbers of supermicron particles are shown. Such particles are not observed in the atmosphere nor in snow (Schwarz et al., 2012, 2013).

The most robust result of this work is that ultrasonication had a huge effect on their measured calibration factors (the authors described this as "to further mix the soot solutions", in fact the soot suspensions will have either experienced disagglomeration or further agglomeration, depending on the particles and the conditions used. I suspect that disagglomeration will have occurred based on Wang et al., 2012). This proves that particle size was extremely important, meaning that the authors' unrepresentative surrogate black carbon material (chimney soot) has been proven in the authors' own work to have resulted in severely biased and unreliable calibration factors.

The results of this work therefore do not provide a better constraint on transmission-based absorption estimates for filtered meltwater, compared to the reference case of no calibration. In an important sense, they are worse than no calibration, since non-expert readers will assume that "calibrated" measurements are reliable. I would have recommended that the authors use an integrating sphere method (Grenfell et al., 2012) instead of attempting to calibrate a fundamentally limited method. The filter photometer transmittance method is fundamentally a measurement of attenuation and not absorption. The alternative recommendation is to repeat these experiments using dust surrogate particles and freshly-generated soot particles. Unfortunately, the present results will not bring further understanding or clarity to the community and I am obliged to recommend rejection.

1 Further comments

Further minor comments:

1. The starting sentence of the introduction is incorrect, soot does not only consist of BC and OC but can also include other materials like sulfates.

2. The statistical treatment of the data was inappropriate. Rather than forcing fits through zero, the authors should either follow the recommendations of Cantrell (2006) and/or calculate mean ratios between the two variables.

3. This paper did not cite or discuss recent important work on determining BC in snow (Schwarz et al., 2012, 2013) nor properly discuss the limitations of the filter-photometer methods. Overall, the literature context of the paper was poor and should be improved. The papers referenced above provide some examples of potential improvements.

2 References

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