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Atmospheric Measurement Techniques Discussions

Interactive comment on "Influence of the melting temperature on the measurement of the mass concentration and size distribution of black carbon in snow" *by* T. Kinase et al.

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This paper raises an important issue regarding the reliability and quality of BC measurements in snow (and ice). While the paper certainly presents interesting results some details of methodological information are missing although they are substantial to allow interpretation of the presented results. The following lists the details of information which remain unclear in the current manuscript and discusses why they are relevant for the conclusions made.

(1) What standard material has been used for calibration of the results? The choice of standard material has been discussed in depth previously (Wendl et al., AMT, 2014). This study definitely should be cited in the manuscript since standardization and cal-

ibration is an important issue when analyzing BC in snow and ice. I am aware that the description of standard used in the study discussed here can be found in the literature cited (method section) but I do not think this is sufficient. Other related and important questions are: How was the calibration performed? Once a day, weekly. . .? Most importantly, do the discussed effects also show up in standard samples (e.g. did you analyze standard solutions which were treated similar to the individual sets discussed for comparison/as a reference)? How can the observed differences/similarities between standard and sample behavior be explained?

(2) It is unclear if the containers containing the snow samples were kept closed during melting. I assume they were, but still it should be described clearly in the manuscript. This is important since if they were open, effects due to evaporation cannot be excluded. Evaporation would likely result in an increase in concentration but might not be detected because being masked by other, larger effects having an opposite direction (i.e. resulting in decreased concentration) which then might be underestimated as a consequence.

(3) It remains unclear if once the snow samples were melted they were sonicated in the containers used for melting or if aliquots were first taken, transferred to new containers and sonicated afterwards. If the latter is the case, any wall effects happening in the containers used to melt the samples will not be considered even though they are likely significant (decrease in the observed concentrations, likely size dependent) and would allow a different interpretation of the derived results. This should be clarified and also be discussed if necessary based on the procedure used. It should also be described if the samples were stirred prior/during sonification and/or analysis. This is of particular importance to know for the sample in the 500 cm3 bottle where sedimentation might have a significant effect resulting in the difference observed compared to the 30 cm3 containers.

(4) Further, what was the time passing between sonification and analysis of samples? Was it similar for all samples? Again this information is relevant because wall ef-

fects happening between sonification and analysis can result in the observation of decreased concentrations. If there is a large deviation in the time passed between time of sonification and analysis for the individual sample such wall effects might contribute significantly to any observed decrease in concentrations. Has this been tested? This should be clarified and discussed.

I am fully aware that the author's might have omitted this degree of detailed information because some of it seems rather trivial. Nevertheless, I strongly believe it needs to be addressed carefully for the reasons pointed out.

More general and not considering the above, the fact that only two different samples were used for the investigations seems limiting to reach sound conclusions. Are the samples studied here representative? Their concentration is not so different after all. The question remains if the investigated effects are also significant for samples with a much different BC particle size distribution or much lower/higher concentrations? In fact, even for the results presented here, the significance of the described effects may be questioned regarding the uncertainties (see e.g. Fig. 2) and the fact that the number of samples in each set is rather low (n = 3 for 30 cm3 containers and n = 1 for 500 cm3). Is this also the reason why there is not given a clear recommendation what temperature and melting time should preferentially be used in order to obtain the most reliable results?

Other remarks:

p.7, line 28 – p.8, line 6: Were the stored samples sonicated once again prior to each subsequent analysis? If this is not the case and still no effect of storage time was observed these results would be very different from what has been described in Wendl et al. (2014). Please comment/discuss.

p.2, line 23 ff.: If soot is measured with a thermal optical technique, it is referred to as EC (elemental carbon), not BC. The authors should also cite some of the pioneer work of such measurements in snow and ice:

Lavanchy, V.M.H., Gäggeler, H.W., Schotterer, U., Schwikowski, M. and Baltensperger, U. (1999). Historical record of carbonaceous particle concentrations from a European high-alpine glacier (Colle Gnifetti, Switzerland). Journal of Geophysical Research 104: doi: 10.1029/1999JD900408. issn: 0148-0227.

Jenk, T. M., Szidat, S., Schwikowski, M., Gaeggeler, H. W., Bruetsch, S., Wacker, L., Synal, H. A., and Saurer, M.: Radiocarbon analysis in an Alpine ice core: record of anthropogenic and biogenic contributions to carbonaceous aerosols in the past (1650-1940), Atmospheric Chemistry and Physics, 6, 5381-5390, 2006.

p.2, line 28 ff.: To be complete and because it discusses a lot of similar and complementary issues to the ones addressed in this paper, Wendl et al. which was actually published in the same Journal as this study(!) needs to be cited and should also be discussed (see above):

Wendl, I. A., Menking, J. A., Färber, R., Gysel, M., Kaspari, S. D., Laborde, M. J. G., and Schwikowski, M.: Optimized Method for Black Carbon Analysis in Ice and Snow Using the Single Particle Soot Photometer, Atmos. Meas. Tech., 7, 2667–2681, 2014, doi:10.5194/amt-7-2667-2014.

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