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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Light-absorbing impurities in Arctic snow" *by* S. J. Doherty et al.

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This compilation of the analysis of over 1200 snow samples for light-absorbing impurities represents an impressive amount of work that certainly deserves to be made available to the community under a final form. Overall, I found the paper well written and for the most part adequately detailed, with conclusions presented with a sound basis. I just wish to offer the authors the following minor comments.

1- Evaluate the measurement error. Although the method is adequately detailed, here or in references, no error bars are given here. Likewise, no test of the reproducibility of the method is given or referenced clearly. Errors include those due to the measurement itself and those due to the treatment of the data, which for example assumes values for Angstrom exponents. I therefore strongly recommend adding a section on error evaluation. Apparently, not all snow samplings were performed with the same care or

expertise, so the authors may also consider adding a confidence index for each set of value. For clarity, I also very strongly recommend mentioning key hypotheses in the abstract. In particular, the determination of the BC mass is based on a mass absorption coefficient of 6 m2/g, while other authors commonly use 7.5 m2/g. This key hypothesis should therefore be mentioned in the abstract.

2- Testing the reproducibility of the method is difficult because it requires multiple sampling, and the intrinsic variability of BC in snow will come into play. On page 18826, the authors present data on spatial variability, but this cannot be separated from the variability in instrument response. I believe that multiple sampling at one site would be desirable, using for example an approach similar to that developed by (Conger and McClung, 2009) do determine variability and errors in snow density measurements. I leave it up to the author to decide whether they can perform such a study before writing a final version or whether they prefer to simply acknowledge the problem and work on it later.

3- Snow processes affect BC concentrations. Depth hoar may clean itself through sublimation-condensation cycles, windpacks may increase their BC content because of scavenging during the airborne phase, etc. It might be interesting to investigate a relationship between snow type and BC content. This would helps investigators working in a well defined context infer BC concentrations.

4- Page 18835, line 20, the author allude to the effect of the Angstrom exponent on the evaluation of the BC content. This illustrates the need for a paper-wide error analysis and for attributing a confidence level to each set of values.

5- The author conclude that atmospheric BC concentrations have declined since CN85, but that they do not see any decline in snow BC. I accept their conclusion that snow BC has not declined, since it is based on a strong data set, but could they suggest why snow BC has not followed the atmospheric trend ?

6- On page 18841 and elsewhere, the authors discuss the effect of snowpack thick-

ness. It would be nice if they could detail which thickness is required for this effect to be perceptible. Of course, it depends on solar SZA, on impurities in snow, and on the vertical profile of snow grain size. But still, a few examples on well defined snowpack types would be desirable. Here, the unfamiliar reader does not know whether the critical depth is 15 cm or 1 m.

Conger, S. M., and McClung, D. M.: Comparison of density cutters for snow profile observations, Journal of Glaciology, 55, 163-169, 2009.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 18807, 2010.