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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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COMMENT 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.

REPLY: The reviewer is correct: the text should include an uncertainty analysis. We have now added a section to the paper specifying the sources of uncertainty and (where possible) their magnitudes. (See .pdf included as a supplement).

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Interactive Discussion

Discussion Paper

Regarding including a confidence index: We have made an effort to exclude samples that we had reason to believe might be problematic. We have tried in Sections 3.1 and Section 4 to give the reader the information needed to understand the various sample sets. We don't feel we have the information available to objectively assign a confidence index to the various data sets, and in some cases statistics are given for data from multiple sampling campaigns, which would make "grading" the overall data set difficult.

COMMENT: 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.

REPLY: The use of 6m2/g for BC mass absorption efficiency is a built-in assumption in that this is the MAE of our calibration standards. It is not a hypothesis, as demonstrated by the fact that nothing is done in our analysis to test whether 6m2/g is in fact an accurate value to use. We do now include discussion of the potential source of bias use of this value introduces into our derived values, as part of the new section on uncertainty analysis.

We have changed the text so this assumption is now explicitly stated in the abstract as follows: "The snow is melted and filtered; the filters are analyzed in a specially designed spectrophotometer system to infer the concentration of black carbon (BC), the fraction of absorption due to non-BC light-absorbing constituents and the absorption Ångstrom exponent of all particles. This is done using BC calibration standards having a mass absorption efficiency of 6.0m2/g at 550nm and by making an assumption that the absorption Angstrom exponent for BC is 1.0 and for non-BC light-absorbing aerosol is 5.0."

COMMENT 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

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

REPLY: We did take multiple samples at many sites, typically side-by-side samples at 5 depths for a total of 10 samples at a single site; examples of comparisons are shown in Figures 6 and 11. Of course, more extensive studies would be worthwhile, but carrying them out would take some time and we don't feel it is reasonable to hold up publication of this paper so we can do such a study. We do, however, include in the new "uncertainty analysis" section a discussion of instrumental uncertainty vs. differences in the side-by-side samples.

COMMENT 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.

REPLY: We did note the snow type in samples we collected ourselves, and we are considering including that information in a comprehensive database to be made available on a website. Some information on BC with depth is given in this paper, for example Figure 11. A thorough analysis would require that observations of the snowpack properties be made not only at the time of sampling but throughout the season as the snowpack accumulates, e.g. to keep track of meteorological conditions (snowfall rates/timing, temperatures, etc.). This could be done as a separate study; our focus in this paper is geographical variations rather than temporal variations and in-snow processes. We are working on an analysis of samples from three sites where the snowpack was sampled during the melt season, but these data will be analyzed and reported in a separate paper.

COMMENT 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.

REPLY: This has now been added (Section 6 of the revised paper; see .pdf included as a supplement)

COMMENT 5- The authors 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?

REPLY: We did not intend to convey an argument that snow BC has not declined since the mid-1980's but rather that we don't have the ability to conclude that it has declined. The atmospheric data would lead one to believe that the snow concentrations should have declined, but unfortunately we have only a very limited amount of data from Clarke and Noone from the 1980's, it was all from the periphery of the Arctic, and the measurements were made with a different method which we now know will yield values that are biased high. We are unable to quantitatively assess how much the C&N values were biased high, so we can not adjust their data and then conclude whether concentrations have decreased, increased or stayed the same. We have altered the text as follows so this is clearer: "Given the patchiness evident in our side by side samples discussed earlier, a quantitative evaluation of these differences is difficult. Moreover, we cannot definitively say that the two results differ significantly, because part of the difference is probably caused by the different photometric methods used. CN85 also used an integrating-plate photometer (instead of the integrating sandwich) to analyze their nuclepore filters, and in that method the scattering by particles on the filter can reduce

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transmittance in a way that would be erroneously attributed to absorption (Clarke et al., 1982). We can not quantitatively assess the resulting high bias and therefore can not determine to what degree the decreases shown in Table 9 reflect real decreases in snow BC concentrations versus a change in measurement methods. However, we can at least conclude that concentrations in the areas sampled have not increased."

COMMENT 6- On page 18841 and elsewhere, the authors discuss the effect of snow-pack thickness. 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 snow-pack types would be desirable. Here, the unfamiliar reader does not know whether the critical depth is 15 cm or 1 m.

REPLY: The following information has been added to the paper: "At 500 nm, the e-folding depth for clean Antarctic snow was 25 cm (Figures 3 and 4 of Warren et al, 2006); for snow on Arctic sea ice with some soot pollution it was 6 cm for dry compact snow and 12 cm for melting snow (Grenfell and Maykut, 1977). Schwerdtfeger and Weller (1977; reproduced as Figure 8 of Warren, 1982) found broadband transmittance of 1% at 1-m depth in clean Antarctic snow."

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C9458/2010/acpd-10-C9458-2010-supplement.pdf

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