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Atmospheric Measurement Techniques Discussions

Interactive comment on "Contribution of dust and elemental carbon to the reduction of snow albedo in the Indian Himalaya and the Finnish Arctic" *by* Jonas Svensson et al.

Anonymous Referee #1

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Review: Contribution of Dust and elemental carbon to the reduction of snow albedo in the Indian Himalaya and the Finnish Arctic Svensson et al., for ACP 2017

The paper is focused on a significant and compelling issue: the attribution of light absorption in snow between primarily natrual sources (mineral dust), and sources strongly influenced by anthropogenic activities (EC, especially in India). There are several main points made in the paper:

1) Mineral dust is perhaps the dominant light absorbing impurity in Himalayan snow 2) MAC of EC in snow is lower than for laboratory EC 3) MAC of EC decreases with increasing snow depth.

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Although I imagine that points 1 and 3 have a good chance of being correct, all three of these conclusions suffer from some lack of support in the paper. I have substantial concerns about some of the assumptions and interpretations made in the paper, and question the conclusions that can be drawn from it.

My concerns are focused on the following issues:

1) The EC measurement of with the TOM method may be significantly affected by carbonates in the snow.

The authors cite Cavelli et al., 2010 to suggest that any contributions would show up as OC in the analysis, but this is not supported. First, Cavelli et al deals with atmospheric aerosol analysis in which carbonate “is generally <5% [of the aerosol]”. However in snow, and especially in the Himalaya, radically more carbonate can be present. For example see:

Di Mauro, B., F. Fava, L. Ferrero,âĂÍR. Garzonio, G. Baccolo, B. Delmonte, and R. Colombo (2015), Mineral dust impact on snow radiative properties in the European Alps combining ground, UAV, and satellite observa- tions, J. Geophys. Res. Atmos., 120, 6080–6097, doi:10.1002/ 2015JD023287. âĂÍ

Which indicates that mineral dust content in snow in the European alps varied from the cleanest conditions ($\sim$3 ppm dust) to levels of $\sim$200 ppm. 3 ppm of dust represents a factor of $\sim$15 times more mass than the EC concentration estimates of Svensson et al in Figure 12 (which I estmate as $\sim$200 μg/liter = 0.2 ppm EC). Note, too, that Cavelli et al points out for calcite that it “ will possibly be detected as EC”, and pretreatment of ice core samples was a prerequisite for analysis in Ming, J., H. Cachier, C. Xiao, D. Qin, S. Kang, S. Hou, and J. Xu (2008), Black carbon record based on a shallow Himalayan ice core and its climatic implications, Atmos. Chem. Phys., 8(5), 1343–1352, doi:10.5194/ acp-8-1343-2008. As yet, the uncertainty raised by this issue is unbounded. Obviously, high mass ratios of mineral dust to EC in the Himalaya that contribute significantly to the TOM EC measurement are a real possibility that must be seriously addressed;

ideally the authors would test some filter punches of ambient Himalayan samples with and without the type of acid treatment used in Ming et al., to constrain this issue. Note that conclusion #2 above is only valid if the EC determination is accurate. Further, note that conclusion 1, is not weakened by the carbonate issue, but should be reframed to reflect the clear contributions of mineral dust to in-snow absorption in the Himalaya.

2) The laboratory tests of EC and SiC do not strongly constrain uncertainties in evaluation of ambient snow samples.

The authors present several tests to evaluate the performance of their three measurement types: OCEC-analyzer TOM determination EC concentration; PSAP measurement of loaded-filter light transmission before and after undergoing TOM analysis; and loaded-filter light transmission measured with the OCEC-analyzer. However, it is not clear how relevant these tests are to the conditions relevant to the ambient samples.

Comparisons of optical depth measured with the PSAP and the OCEC were presented as if they indicate the validity of the EC determination from the optical measurement; in fact they only illustrate measurement precision since both instruments were quantifying the same thing (optical depth on the filter). Hence the statement “The good agreement between the two optically derived EC values suggests that much of the scatter seen in Figure 5 is due to the uncertainty in the analyzed content of EC using TOM (and FID)” is incorrect and mis-leading. The authors should re-analyze the optical/TOM EC comparison to attempt to ascertain the confounding influences of the SiC, even as an imperfect proxy for ambient mineral dust contaminants. Further, although the EC concentration was not known (page 10, line 5), the relative concentration was. Hence there should be better constraints on the influences of the SiC on the different determinations of EC loading; I expect this dimension of analysis also to be relevant to the ambient samples.

Undercatch by the filter was mentioned, but not given sufficient attention to justify confidence in the dataset. This can be a major problem (close to 100% for some filters!), especially for BC which is largely in the <1 μm mode, and typically <0.5 μm; hence the statement in the paper “smaller particles normally contribute little to total particulate mass” is not relevant to BC and filter undercatch. Note that the linearity of optical depth with estimated EC loading does not constrain filter losses. At a minimum, the authors should test undercatch with their filters by refiltering post-filter liquid from one of their EC standards with a high-efficiency filter. See, for example, Schmitt et al., doi:10.5194/tc-9-331-2015âĂÍ for a treatment of similar issues.

3) Conclusion 3 above is based on the results shown in Figure 13. In my opinion, the data are not statistically robust enough to support that conclusion, and the discussion in p. 23 lines 28-34.

Due to these major issues, I believe that the abstract, discussion, and conclusions should be modified dramatically.

Smaller comments :

Abstract: 1) Snow albedo is strongly dependent on many factors beyond LAI (eg snow morphology, snow depth, underlying surface properties); since albedo is not a focus here, I suggest that the first sentences be reformed.

Introduction 1) Since the SP2 is not used here, including it at this depth in the introduction seems unnecessary

2) The discussion of non-BC LAI (page 4, lines 21-35) should probably be introduced before discussing the measurement techniques

Misc:

1) Page 6 line 1: What is meant by “protected” here?

2) How were the filters dried?

3) P. 7, line 6: I am surprised that it is difficult to gravimetrically determine the dust concentration, as dust is almost certainly the dominant mass source left on the filter (at

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least in the Himalaya, where the optical thickness was too large to be measured!).

4) More information about the SiC used is needed. Manufacturer? Color? Etc.

5) Please specify in the figure axis labels the technique (e.g. "optical" or "TOM" used to extract the various quantifications. For example, on figure 3, the caption merely indicates the EC was from the OCEC instrument, but it was not immediately clear that this via TOM.

6) Page 12 line 7: again, the atmospheric results are likely not relevant in the Himalaya. I think they can be cited only for the Arctic results.

7) What is the author's estimate of the loading of mineral dust on the ambient filters? How does this compare to the range tested in the laboratory?

8) Please provide estimates of statistical relevance of the fits (e.g. rˆ2 values).

9) I saw concentrations presented as μg/L as well as ng/g. Please harmonize to a single unit for the reader (obviously these are effectively the same. . .)

10) Page 17: lines 12-18: this is a lot of speculation without much reason for it. Suffice it to say that your results, for a different area where high spatial variability can be expected, are higher than a previous estimate.

10b) Page 17 lines 19-26: please include broader arctic estimates of EC concentrations for context. E.g. Doherty et al., 2010 ACP, doi:10.5194/acp-10-11647-2010âĂÍ

11) I don't see the value of the comments of page 17 lines 27-33, or figure 8 at this point in the text. This appears merely a technical point about measurement precision (as discussed above).

12) Couldn't the differences discussed in page 18 line 4-17 also be due to EC over estimation for example due to carbonate impacts?

13) Schwarz et al. only presented a theoretical estimate of MAC, not any measure-

ments. Hence this is only suggestive, but it is also possible that the filter selectively catches larger BC. Please consider removing figure 10, as this is quite peripheral.

14) Page 20, line 18: “modes” is clearer than “peaking fractions”.

15) Please specify the wavelength of the optical depth measurement in the OCEC instrument. How does this compare to the 532 of the PSAP? In discussion of relative absorption of mineral dust and BC, it is important to continue to specify the wavelength range for which the discussion is relevant.

16) Please be consistent in referring to the dust “absorption” fraction. For example, in the caption to figure 11, “dust fraction” could be mis-understood by an un-alert reader as a mass fraction.

17) Is it possible to add data points to figure 12 a, so that the spread in results will be obvious to the reader?

18) What is the explanation for the huge differences in EC via TOM and OPTICAL at the sub surface contaminant layer shown in figure 12B?

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