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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 #3

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Review of "Contribution of dust and elemental carbon to the reduction of snow albedo in the Indian Himalaya and the Finnish Arctic" by Svensson et al.

Svensson et al. use a combination of a thermal-optical detection method and a particle soot absorption photometer to determine elemental carbon mass, its mass absorption cross section, and the contribution of mineral dust to the optical thickness of light absorbing impurities in snow. They conducted a series of laboratory assays with chimney and standard soot, as well as two different types of mineral dust, and mixtures of the various components to test the instrumental set-up. Subsequently, the method was applied to ambient snow samples from the Finnish Arctic and two Indian glaciers. The

main findings are: a) the MAC of EC in snow seems to be lower than that of the laboratory test soot; b) dust plays a larger role in light absorption on the Himalayan glaciers than in the Arctic; and c) the MAC of EC in snow seems to increase with deposition age.

Generally, the study follows a careful design and is well described. It addresses the very important challenge of how to quantify the albedo-reducing effect of elemental carbon and mineral dust and their combination in snow, and contributes to the understanding of uncertainties by highlighting the importance of accurately determining the EC MAC in snow.

Nevertheless, the work has some shortcomings: It is not clear why the MACs of two very specific dust types are determined in the laboratory and discussed when this is meaningless for the ambient dust samples with unknown absorption characteristics. An explanation of why the MAC of EC in snow is variable is missing, even though this is featured as a main result. Also the observation of a potential trend in the EC MAC value with snow pit depth is not backed up sufficiently. There is only a small number of data points, only one snow pit is discussed and the statistical significance of the trend is not given. Further, the value of addressing Arctic and Himalayan snow samples together in this manuscript is not evident. Additionally, the manuscript language needs to be improved.

I recommend addressing the above and following comments before a publication can be considered.

General comments

The title implies a discussion on snow albedo reduction through EC and mineral dust. However, the manuscript does not provide actual values of snow albedo reduction but rather focuses on accurately determining the mass of EC in snow and the contributions to optical thickness of EC and mineral dust. I suggest changing the title accordingly.

p. 10, l. 16/22: Information on light absorbing constituents in SiC and stone crush are needed. The light absorption of mineral dust is strongly influenced by e.g., hematite, goethite etc. There is no point in determining a MAC of a “random” dust sample in the laboratory to apply it to the ambient samples with unknown contents of light absorbing constituents as the authors state themselves on p. 20, l. 13. It is not clear, why dust samples are tested in the laboratory that have no relevance for the ambient dust samples. Also, in Figure 7, the discrepancy between the gravimetrically determined SiC mass on filters and the estimated based on the MAC when values are > 7 g / m2 are just discarded without any further discussion. Is it possible, that the method does not work for high dust loadings? I suggest that the dust related aspects of the laboratory assays are drastically shortened to the information relevant for the ambient samples.

l. 13 – 27: What about local dust sources? There are most likely bare rock or mountain walls from which mineral dust can be deposited on the glacier. In general dust sources and in particular local dust sources can be highly variable so that any kind of interpretation in terms of trends is difficult, especially if no information about the origin is available. If the authors had mineral dust size distributions at least a statement on potential role of local sources if very large particles are present could be made.

l. 28 – 34: The interpretation that the MAC is decreasing towards the top of the snow pit is not convincing and an explanation why this might be the case is missing completely. There are not enough data points to conclude a trend in the MAC. The profile rather shows that LAI deposition is highly variable. Towards the bottom of the snow pit the ratio is also lower and if the point at 10 cm depth were not as low as observed, probably no trend would be inferred. The authors introduced on p. 20 the hypothesis that potentially large loadings on filters play a role for the observed variability of the MAC (see comment further above). To test this and to develop a potential explanation why the MAC changes, I recommend plotting in Figure 13 the ratio of the optical and TOM EC versus the ratio of Dust/EC and OC/EC to check if there is a relation with the

overall LAI content. Also, indicate in the figure which ratio is meant. In the conclusion, the potential reasons for the observation should be given as well.

The authors recommend additional work to constrain the optical properties of EC in snow but do not say how this could be done. Some more precise ideas would be useful.

Specific comments

p. 2, l. 3: What do you mean with “impact on climate”? LAI deposited on snow do not directly impact the climate but rather the hydrological cycle. Only if seasonal snow and glacier retreat significantly, local climate will change and LAI are hardly the major cause for it.

p. 3, l. 2-3: Specify which radiation budget you mean and through which mechanism snow melt can be enhanced?

l. 6: “In this context” refers to mountainous glaciers. Why are mountain glaciers more important than seasonal snow or ice caps?

l. 12-14: I suggest removing this sentence and focusing only on BC effects in snow. In addition, BC has health effects everywhere where a human being is exposed, not only in cities and where open cookstoves are used.

l. 21: is Bond et al. 2007 really the most up to date reference?

l. 34: more elaboration on the thermal-optical analysis is needed here.

p. 4, l. 3 – 8: The description of the SP2 can be shortened because this instrument does not play any role in the set-up of this work.

l. 10f: An explanation of why it is important to keep the samples frozen before analysis is needed.

l. 18f: The statement that small particles contribute little to particle mass in this context might be a bit misleading. More elaboration is needed. What is meant by small particles, what is the threshold that Lim et al. (2014) refer to? If it is < 100 nm the statement is ok, if it is < 300 for example, it would not be, because of the size distribution of BC. Also the last sentence does not take into account that the TOM method will also quantify OC, which can also absorb light and reduce snow albedo.

p. 5, l. 21: atmospheric concentrations of what?

p. 6, l. 3f: A description of how the filters were dried is missing here and later on for the microwave heated samples as well.

Figure 1: The location of the glaciers and the valley is not really visible, the site symbols are too small and the lat/lon numbers in the middle of the plot are distracting. Also the location of the sampling sites in Finland are not visible at all. Think about including two maps that show the details for each region, while keeping the global map.

p. 7, l. 6: Why is it difficult to gravimetrically determine the dust load on filters? This is a standard method and the mass of dust very often is much higher than the mass of other LAI. Do you mean it is difficult in regions where little dust is present?

l. 33: It is unclear from the description what the original value of transmittance is.

p. 8, l. 5ff: Why are the authors confident that no acid treatment of the samples is necessary to eliminate carbonates? In ambient mineral dust calcium carbonate or other carbonates are a common constituent which might strongly affect the OC/EC analysis and not only the quantification of OC as you explain in l. 11f (split point).

l. 8-12: Do the authors have any estimate of the loss of small particles at least for the laboratory experiments using the same filters? Since the laboratory pre-assays are meant to quality control the results of the ambient samples this is a crucial piece of information that should be obtained.

l. 18 – 21: From the description the methodology is not clear. Did the authors use duplicate samples, so 2 times original and 2 times heated, or two punches and one

was heated while the other wasn't?

p. 9, l. 9f: What is the reason for this? Is the effect of the filter so overwhelming that the scattering effect of the actual sample is by far outweighed?

l. 20: It needs to be stated more clearly that the MAC values reported here cannot be compared to other studies.

p. 10, l. 8: Information on the homogeneity of the chimney soot is missing. How comparable are samples? Why was chimney soot chosen, is it representative for soot deposited on snow in Finland and India?

L. 35 f: What are twin samples, and what does "separate instruments" refer to? The meaning of the sentence is unclear.

p. 11, l. 1: how many filters were tested and what was the result in numbers?

l. 19: What is divided by Cref?

Figure 4: How would the slope look like if the point with the highest loading of SiC were not considered? It drives the correlation result and how would results look like that are presented in Figure 7?. In the caption the wavelength information is missing.

Figure 5: Add the 1:1 line.

p. 14, l. 8ff: The spread in the comparison of the two optical EC determination methods is not necessarily an indication for variability in the evolved carbon determination of EC. It is not clear what is meant with "uncertainty in the analyzed content of EC using TOM. . .". Which factors introduce uncertainty in this method? Which introduce uncertainty in the optical methods?

p. 17, l. 11f: Concentrations are given in μg / L and ng/g, one consistent unit should be chosen.

Figure 8: Is the outlier at Sunderhunga taken into account for the linear regression? If

so, what would the slope look like without? It would be even greater than 19 % and that is a relatively large deviation between the optical measurement of EC with TOM and the estimated EC based on the MAC and the PSAP data. What does this discrepancy mean for the validity of the methods?

p. 18, l. 12: A reference and an explanation is missing.

p. 20, l. 9ff: The underlying assumption why EC is less absorbing in this case is not provided and not evident. Do the authors assume that the EC is embedded so thickly in OC that light does not penetrate to the EC? If the authors imply matrix effects of the sample, the question is how relevant this is to the ambient snow? On the filter the particle mass is concentrated and the packing of the particles on the filter is not representative for the packing in the snow.

p. 21, l. 10: Do the authors mean the average EC concentration when saying "composite"? Be more clear.

p. 23, l. 3: How do the authors know that those are different seasons over various years and not melting and freezing cycles within one year? An explanation is missing in the text.

l. 7: Water soluble constituents might percolate, this is not known because it hasn't been investigated. So there cannot be a statement that this is not the case.

Technical comments

p. 2, l. 3: write "with subsequent implications for snow melt. To more accurately quantify changes in snow albedo, . . ."

l. 5-6: write ". . .from the Indian Himalaya and pared the results to snow samples. . ."

l. 18: "dust deposition"

l. 22: Do you mean deeper pits?

p. 3, l. 5: There is a newer AMAP report from 2015.

l. 9: you should list rather fuel types than activities if you refer to carbon-based fuels.

p. 4, l. 21: "can" instead of "may".

l. 22: "microorganisms" instead of "microbiology"

l. 33: "Other methods consist of using transmitting light. . ."

p. 5, l. 1: "in the Indian" l. 4: tests

l. 8: "2.1.1 The Indian Himalaya"

l. 12: "valley-type glaciers in the Ganges basin. . . ."

l. 14: delete "residing"

l. 27: "Dust from local sources has. . ."

l. 31: delete "designated"

p. 6, l. 14: "sampled snow"

l. 16: "(the first. . ."

l. 17: ". . . where details of the area. . ."

p. 7, l. 31: " for pyrolysis (darkening of the filter) occurring during the. . ."

l. 34: delete "filters" before EC

p. 8, l. 14: "uses"

l. 32: write micrometer instead of millimeter

p. 10, l. 2: Start the sentence with "A series of . . ."

l. 5: replace "minute" by "small"

p. 11, l. 15: "dependent on"

l. 16: delete “thus many influences on it.”

l. 20: delete “somewhere”

l. 21: “for our BC solution data”

l. 22 “Bond et al. (2013) report. . .”

Figure 3 Caption: “comparison of. . .”, here and in several other captions. Also, sometimes OCEC-analyzer, TOM or Sunset analyzer is used. A more consistent use of the method name is needed.

p. 12, l. 4: “Figure 4 shows results analogous to Figure 3. . .”

p. 13, l. 9: “The data are scattered,. . . regression is within 17 % of the 1:1 line.”

p. 14, l. 7: “As observed, the EC amounts derived by two optically different methods show a consistent relation. . .”

Figure 6 caption: “between the optical measurement of EC. . . on the substrate using PSAP data and the . . .”

p. 15, l. 9: “Two slopes are presented, . . .”

Figure 7 caption: “containing all data points”

p. 16, l. 14: “with material and quantitative impurity. . .”

p. 17, l. 2: “these surface samples contained LAI mostly originating from the post. . .”

l. 3: “studies of BC”

p. 17, l. 19: “For reference, the EC concentration in the surface. . .”

l. 22ff: “. . . in Pallas might result from the fact that the majority of samples was taken later in the snow season. . . and EC has likely concentrated. . .”

l. 25: “On a larger scale, Northern Europe and Arctic, the concentrations. . .”

p. 18, l. 9: “a smaller absorption efficiency”, replace “absorbing efficiency” by “absorption efficiency” here and in several other places in the text.

l. 17: delete “originating”

p. 19, l. 9: “demonstrates”

l. 12: “for BC we use the same complex. . .”

Figure 10: Explanations for the symbols in the equation are missing.

p. 20, l. 4: “liquid”

l. 7: “a scattering medium shows enhanced. . .”

l. 8: “was” instead of “were”

l. 13: exchange “applicable” by “possible”

l. 18: “modes” instead of “fractions”

l. 19: “with modes at 35 % and 65 %.

l. 20: “by LAI other than BC. . .”

p. 21, l. 3f: “reach as much as 56 %.. in the Tibetan. . . as a fraction of the optical depth of LAI on the filter,. . .” l. 5: delete “an” before albedo

Figure 11 caption: “Frequency of the occurrence of dust optical thickness fractions at the three sampling sites.” Otherwise the mass fraction might be inferred.

p. 23, l. 2: delete “evidently”, replace “core” by “pit”

l. 3: is “alternating” meant by “altering”?

l. 8: replace “that” by “who”

l. 13: replace “at best ca.” by “potentially”

l. 30: “decrease” instead of “be decreasing”.

p. 24, l. 14: “EC deposited on snow”. The EC does not originate from the snow.

l. 14 f: “Our finding of a MAC value of about half of . . . EC particles, can have implications for the snow. . .”

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