The Cryosphere Discuss., 9, C281–C285, 2015
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The Cryosphere Discussions
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Interactive comment on "Soot on snow experiments: light-absorbing impurities effect on the natural snowpack" *by* J. Svensson et al.

Anonymous Referee #1

Received and published: 30 March 2015

Svensson et al. report on the impact of black carbon (BC) in snow on the albedo and the melting of the snowpack. They performed experiments in the field, during which they artificially caused the deposition of additional absorbing impurities (including an overwhelming fraction of BC) on natural snowpack. The experiments were performed with different set-ups during three winters (2011, 2012, 2013) at three different sites in Finland. The presented results concern measurements of BC concentrations in the snow and how they evolved after the BC deposition until the melting of the snow, albedo measurements at undisturbed and affected sites compared to simulations with the SNICAR model, and observations of physical snowpack properties related to the experiments. The authors claim that in general albedo measurements and simulations agree. Further conclusions consist of a set of recommendations for further field experiments concerning the impact of BC and other absorbers on snow albedo and further

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processes. BC in snow has recently attracted strong scientific interest due to its potential to lower the snow albedo impacting metamorphism, snow melting, and radiative forcing. Global, regional, and local model studies based on radiative transfer theories applied to snow demonstrated the potential impact of BC in snow in regions like the Arctic or the Himalayas even with the relatively low BC in snow concentrations encountered in these regions. However, dedicated field experiments based on manipulating BC concentrations in real snow to quantify the link between BC and albedo and snow melting are very limited. Therefore, any new measurements to study these links are highly welcome especially during the melting period of the snowpack. Unfortunately, the presented experiments and the analysis seem to be seriously flawed so that sound conclusions are either limited or even impossible. As a result, the novelty of the presented results is rather limited. In the comments below, I describe a number of my concerns regarding the manuscript. I can only recommend refusing the publication of the manuscript in the Cryosphere.

Comments

Chapter 3.1: The authors discuss in this chapter the evolution of BC in the snow during the SoS2013 experiments. For example, because the concentrations decreased from 1465 to 529 ppb between 8 and 17 April they state that only 36 % of the initial soot particles were observed at the surface after 9 days. Such a reasoning based on observed concentrations is wrong. The calculation can only be based on budgets of BC in the snow, not on concentrations. The authors go even further to calculate a sum of observed concentrations (see Table 2, for example 746 as a sum of the concentrations observed in five snow layers at spot 7 on 17 April). Such a sum is useless because it depends on the number of samples. For instance, it would be a factor of 2 higher if the authors had sampled 10 layers. The authors continue to calculate the fraction in the different layers based on the concentrations and the sum of the concentrations. This gives more or less correct fractions only if the entire snowpack is represented by exactly the five sampled layers. It must also be assumed that the density variation inside

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the snowpack is negligible, but it remains unclear if that was the case in all sampled snow pits. Finally, a direct comparison of the concentration in a top layer, which is in one case 7 cm thick (8 April), with the concentration in the top layer of the subsequent snow pits, which is 5 cm thick (17 and 24 April), is misleading if the different thicknesses are not considered. This is even more important in these experiments, where a strong BC gradient between the surface and the underlying layers can be expected due to the design of the BC deposition experiments.

Chapter 3.1: To derive reliable data on the BC in snow trends several steps are needed. 1. The background BC in snow concentrations for the entire snow columns need to be measured. The authors give an average BC in snow concentrations for SoS2011. Is this only surface snow or the entire column? But no background data for SoS2013 are given except maybe the entry "9B (reference)" without any further description in the text. 2. BC budgets for all sampled layers need to be calculated. This requires BC concentration, BC background concentrations, and snow density measurements. However, it appears that at the start of the SoS2013 experiments only the surface snow layer was sampled. 3. Total BC in snow columns can be calculated by summing up the budgets of the individual layer and taking into account the full snow column. According to the manuscript it seems as if the authors do not have all necessary data to perform such calculations. If that is the case, their results remain qualitative and appears impossible to derive scientifically sound numbers.

Chapter 3.4: The authors perform a linear regression using their observations as shown in Fig. 4. How is this justified if the widely accepted radiative transfer theory predicts a non-linear behavior between BC concentrations and albedo?

Chapter 4: The authors list seven recommendations for future studies regarding BC in snow experiments. In my opinion, at least five of these seven recommendations (1. experiments with low BC in snow concentrations are needed; 4. experiments over longer periods needed; 5. more detailed measurements are needed to follow changes in the snowpack; 6. studies on further absorbers like dust needed; 7. measurements

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at undisturbed reference sites needed) are not related to this study and were actually known before. Only the third recommendation is directly linked to the performed experiments.

P. 1232: The authors claim in the introduction that the "focus is on the effects of the artificial impurities right after deposition, without further snow-soot interaction processes." However, the majority of chapter 3 deals with trends in the BC (or EC) concentrations during the different experiments, trends in the snow albedo, and changes in the physical properties of the snowpack. These discussions are backed by table 2 showing BC concentrations during the SoS13 experiment on three different days and figures 2 and 3 showing snow profiles also during the SoS11 and 13 experiments and time series of albedo and meteorological measurements for SoS11 and 13 from the start of the experiment until the melting of the snow. I actually find the development after the initiation of the experiments the most interesting part and it makes sense to focus on these trends, but this is in direct contrast to the statement in the introduction.

Page 1232: Even in the Himalayas BC in snow concentrations above 100 ppb are rather exceptional. No references are given for the claim that in the Himalayas and the European Alps can be higher than 100 ppb.

Page 1234: The SoS2012 experiments are described, but the only results used further seem to be the characterization of the BC particles (size distribution, SP2 measurements) while still in the gas phase in the cylindrical chamber. (By the way: How reliable are the SP2 measurements made inside the chamber? Any effects due to the walls? No further descriptions of the measurements are given, nor of any details how the results were derived.) However, in the SoS2013 experiment the blowing system to transfer the soot into the air was modified impacting the size distribution of the particles. This leads to a couple of questions: How useful are the SoS2012 measurements for the SoS2013 experiments? The authors claim that changes were either small or only concerned the largest particles (page 1235). Was this tested? If yes, how? If they are not comparable, the description of the SoS2012 experiments may as well be

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deleted.

Pages 1237/8: At which height were the albedo measurements made? What is the field of view of the downward looking instruments? How do the fields of view correspond to the manipulated area? Any impact on the measurements due to the strong differences in BC inside and outside the manipulated area? All these details that may be important for the interpretation of the data are missing. In contrast, the second paragraph of chapter 2.4.2 describes spectral albedo measurements that are not used in the manuscript but have been (or will be) presented elsewhere. This paragraph could be deleted without any impact on the manuscript.

Pages 1238/9: The authors report that snow temperatures, densities, SSA were performed. Why are the results are not shown in the snow profiles presented in Figure 2? For example, the SSA measurements were only used in the manuscript to derive an average optical radius for the SNICAR simulations. No further details concerning the SSA data are presented.

Interactive comment on The Cryosphere Discuss., 9, 1227, 2015.