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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal and elevational variations of black carbon and dust in snow and ice in the Solu-Khumbu, Nepal and estimated radiative forcings" *by* S. Kaspari et al.

Anonymous Referee #2

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The authors describe measurements of BC and iron in snow samples collected in the high altitude area of the Khumbu valley in Nepal (two crevasse profiles and four snow pits from the Mera glacier, nine fresh snow samples from the Pyramid NCO-P station). The samples were analyzed using a combined nebulizer-SP2 system to determine BC concentrations, Fe was determined using ICP-MS. Furthermore, dust was determined in some samples using a gravimetric method. The authors describe in detail the uncertainties and limitations in the detection of BC in the samples including the uncertainty introduced due to the melting and storage of the melted samples and retain that the reported BC concentrations are possibly largely underestimated. In contrast, the two (!)

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reported dust concentrations are a factor of ∼8000 and ∼35000 higher than the measured BC concentrations in the same samples. If these concentrations are correct the impact of dust on the snow albedo and all further implications on the melting of snow and ice in this region is probably overwhelming (as even indicated by some limited calculations in the manuscript, Fig. 4). However, a detailed description of the detection method including an estimate of the uncertainty of the dust concentrations is missing. If the dust concentrations are correct, which is difficult to judge with the limited information provided by the authors, it would rectify our current understanding regarding the role of BC, dust, and other absorbing impurities in the snow for this sensible region. Since it has been demonstrated that the cryosphere and its behavior in this region may have important implications for the regional climate, glacier mass balance, and water resources in this region, such information would definitely warrant publication. Instead, the authors present only a number of general conclusions and remarks regarding the impact of absorbing impurities in the snow on the cryosphere without adding further many new findings.

Moreover, many of the BC, Fe, and dust concentrations were determined in samples collected from crevasses, which may have modified the concentrations due to numerous processes occurring during or after the formation of the crevasse. These potential processes and their impact on the measured concentrations are not even mentioned in the manuscript, although the reported BC and dust concentrations in the snow are to my knowledge the highest ever reported from this region. Despite this additional uncertainty (which probably could have been avoided with the sampling in conventional snow pits), a large part of the conclusions regarding concentrations gradients and impacts are based on the concentrations from the crevasses.

In summary, the authors report some data and describe some methods, which would be extremely important for the community working on cryospheric sciences in this sensitive region. Unfortunately, they fall well short of a detailed description of all their methods (including their limitations) and the potential impacts of their results. The

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manuscript can not be published in its current form. The reasons are described in more detailed in the major comments below. I support the idea of the anonymous reviewer # 1 that the data itself can be published in a journal like ESSD. However, only if the authors can demonstrate that the used methods are not seriously flawed.

Major comments: The sampling of snow in a crevasse adds substantially uncertainty regarding the snow stratigraphy and the measured concentrations. Fig. 3 shows that the top of the snowpack at the rim of the crevasse was variable and not well defined. The structure of the wall makes it apparently difficult to determine the exact depths for each sample even with a well defined snow surface. How did the authors determine the top of the snowpack and the depth for the samples? Although crevasse stratigraphy has been applied for more than 30 years to study surface mass balance of glaciers, its use has been restricted to high accumulations glaciers to minimize the disturbance of the stratigraphy. Moreover, crevasses form as a result of the movement of the glaciers and the resulting shear stress. They can be accompanied by liquid water formation. Did the authors investigate if such processes modified the observed stratigraphy? In addition, the polluted layers exposed in the horizontal wall may have been undergone substantial sublimation. In the photograph in Fig. 3a cavities are clearly visible in the polluted layers possibly formed due to sublimation or even liquid water formation? It is difficult to see, but also the enriched layers in Fig. 3b seem to have different structure compared to the layers in between. Did the cavity formation and/or additional sublimation in the exposed crevasses contribute to an enrichment in BC and Fe in the samples and, thus, to the high measured concentrations? What was the aspect of the sampled crevasse walls? Nevertheless, sublimation possibly occurred throughout the exposed horizontal walls impacting not only the layer with high concentrations, but also other layers to a lesser degree. As a result, the measured background, average, and maximum concentrations could all be biased high in both crevasses even after the removal of 10 cm of snow. Therefore, the measured concentrations from the crevasses remain questionable and should be handled with caution. All these aspects regarding the sampling in the crevasses are not discussed in detail nor even mentioned in the manuscript.

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The uncertainty in the measured concentrations also concerns the discussed altitude gradients, the impact of the impurities on the albedo and the radiative forcing (Ch. 3.3), the implications for the glaciers and the snowmelt (Ch. 3.4), and the conclusions (Ch. 4).

An enrichment of the impurities in the exposed crevasse walls may also be concluded from a comparison with the snow pit data from November 2009. While in the summer layers of the snow pits sampled at the same locations (Mera La and Mera High Camp) measured concentrations remained below 1 μg L-1 (page 33501, line 6), the smallest measured concentrations in the samples from the crevasse walls were $\sim$2 μg L-1 with average background concentrations around 8 to 10 μg L-1 (all values estimated from Figs. 3a and b). However, at the only location where the samples originated in both cases from snow pits, background concentrations were similar. Fig. 3c shows that the measured BC concentration remained below 1 μg L-1 at depth greater than 15 cm. Higher concentrations were only determined in the surface layer, possibly also due to sublimation? The authors argue that the samples from November 2009 are less reliable due to the long storage at ambient temperature (which was certainly not helpful) (page 33496, lines 7f)? Nevertheless, I find the differences between the snow pit and crevasse samples striking, but unexplained in the manuscript.

If the reported dust concentrations are correct, dust is the dominating factor for the snow albedo and for further implications regarding snow and ice in the Himalayas (see below). However, the description of the method of the determination of dust is limited to two (almost identical) sentences (page 33496, lines 13ff and page 33503, lines 14ff) briefly indicating the limitations of the applied method, but without any further specification of the uncertainties or errors. Throughout the manuscript only two values are mentioned (both from the crevasse samples from Mera La). The authors definitely need to expand the description of the applied method, the results, and potential errors. If these values are correct, they would reverse our current understanding of the role of absorbing impurities in the Himalayas. However, all other values on dust in snow and

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ice in the Himalayas I am aware of are orders of magnitude lower.

The authors clearly state that the determined and reported BC concentrations are uncertain due to two major effects: loss of particles during the process of transferring the snow into the gas phase using a nebulizer and due to the handling of the snow samples (melting prior to the analysis and long storage times). During their description, the authors refer several times to a publication in preparation by Wendl et al. suggesting that further details and tests are, or will be, described in detail in this manuscript. This technical manuscript should be published (or at least submitted) first.

The authors claim that they determined an altitude gradient in the measured BC gradients. While I understand the arguments for the gradient regarding the maximum concentrations mainly due to dry depositions and post-depositional processes, this is much less obvious for a similar gradient regarding the background concentrations. These are probably mostly influenced by wet deposition according to large-scale precipitation events during the monsoon period. Isn't it more likely that such events lead to rather homogeneous concentrations for an altitude range from 5400 to 6400 m a.s.l.. Even post-depositional effects impacting the concentrations in the surface layers are limited due to the high accumulation during a relatively short period.

The authors suggest that the albedo in the visible range was different at different elevations on the Mera glacier (page 33502, lines 5ff). This statement is based on the photograph shown in Fig. 2b. While the albedo may have been different, I am not convinced that such a statement can be supported by a photograph. The apparent albedo on a photograph depends on many parameters like the incoming radiation, the solar angle relative to the aspect of the terrain and the location of the camera, the settings of the lens and aperture, and atmospheric parameters. For example, in Fig. 2b the albedo in the region Mera La seems to be very low, while the apparent albedo at the surface in Fig. 3a also for Mera La seems to be very high.

The authors state that the maximum observed concentrations at 154 cm depth at Mera

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La may “represent convergence of multiple years of impurities” (Page 33503, line 23). This statement is in contradiction to the conclusion presented in ch. 3.1 that higher concentrations correspond to the winter-spring layer and the thick low concentration layers can be attributed to the snow from the summer monsoon (Page 33501, lines 1ff).

The calculations of the radiative forcing are based on the albedo values calculated with the SNICAR model. Unfortunately, numerous parameters to reproduce the calculated values are missing. I was able to simulate the average and spectral albedo for the pure snow and the snow containing 258 μg L-1 BC with the online version of the model (snow.engin.umich.edu/) using for example the default parameters for the snow thickness (1 m) and density (0.2 kg m-3). Are these the parameters used by the authors? However, such a thickness is completely unrealistic because Fig. 3a shows that the assumed BC concentration was only encountered in the uppermost sample maybe representing the top 3 cm. Below this layer the measured BC quickly drops to values around 100 and 50 μg L-1. Such a thin snow layer with high concentrations of impurities has obviously a much smaller impact on the albedo compared to a 1 m thick layer. As a result the presented albedo values may be greatly underestimated compared to realistic calculations using the observed profile. This effect is probably much smaller in the case of dust, which is an efficient absorber using the assumed concentrations. However, the decrease in Fe and, thus, in dust is even more pronounced. By the way, the size range used for dust has an important impact in the SNICAR calculations. What size range was used? In summary, a full calculation of the albedo using the entire observed profiles is possible with the SNICAR model and needs to be performed to obtain reliable albedo values. Only with these values the instantaneous radiative forcing can be estimated. Finally, the authors present calculations for MAC values varied by less than 30 %, while the uncertainty in the measured BC concentrations (estimated to be larger than 60 % alone to the nebulizer and a possibly even larger uncertainty due to the sample storage, Ch. 2.2) and how they translate into uncertainties of the simulated albedo is not even mentioned here. The same is true for the uncertainty in the

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assumed dust concentrations.

Ch. 3.4 does not bring substantial new information. First of all, many statements are related to the high measured BC concentrations in the crevasse walls, which are uncertain (see above). However, the further discussions here remain extremely general repeating already well known information: impact of absorbing impurities is largest in the 4000 to 6000 m altitude range because of the by far largest snow-covered area; absorbing impurities can have an impact on glacier mass balance, water resources, and radiative forcing; melting of snow and ice is accelerated by BC and dust; other absorbing impurities can also contribute, but the contributions of the different species remain difficult to quantify. Instead, the following more important conclusions are warranted if the authors are convinced that their determined concentrations are correct. The overwhelmingly high dust concentrations dominate the snow albedo. Fig. 4 clearly shows the negligible impact of BC on the albedo in the presence of 9.3 g L-1 dust even with the high BC concentrations reported here. (I am convinced that the negligible contribution of BC will not change with a correct calculation of the albedo as proposed above.) In that case, BC concentrations do not matter regarding all discussed further impacts on snow and glaciers and so on. According to their own calculations it is incorrect to write that "the impact of BC is diminished in the presence of dust" (page 33509, line12). With the proposed values for dust and BC, the impact of BC is negligible. In fact, it also does not really matter if the BC concentrations measured with the SP2 are correct or not. In contrast, the sources of dust (anthropogenic vs. natural?) and its behavior in the snow becomes more important and needs to be studied. Other absorbing impurities (brown carbon, organic compounds) will become only important (and need to be studied only) if they can compete with the high dust concentrations. The same applies to ch. 4.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 33491, 2013.