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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.

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Below we have copied the comments from referee #3. Our responses are embedded below, and are marked by a hyphen (-).

This manuscript presents a small dataset of snow and ice samples taken from crevasse profiles and snowpits along an elevation gradient in Nepal and analysed for black carbon concentration. Because of the very limited dataset, issues linked with sample preservation and analytical uncertainty, and the application of an existing radiative model with large uncertainties in the adaptation to the site, I do not consider this

manuscript to produce sufficient new information to be suitable for ACP. I would recommend to publish the dataset in the ESSD journal which is well suited for reporting this type of data (http://www.earth- system-science-data.net).

-Referee #3 is missing that prior to this study there has been no observation data of BC deposition to the south side of the Himalaya. The reason for this is that working in this region is logistically very challenging (as we commented to Referee #2, this study required 5-7 days walk to access the glacier, and working at high elevation (6400 m a.s.l.). The remoteness of the study site prevented keeping samples frozen, which is why in the current study and Ginot et al. (2013) samples collected in the field were not kept frozen. We conducted considerable research over the past few years to quantify the BC losses that occur to the samples when stored in the liquid state (presented in Wendl, I., Menking, J. A., Farber, Gysel, M., Kaspari, S., Laborde, M., and Schwikowski, M., Optimized method for black carbon analysis in ice and snow, Atmosphere Measurement Techniques Discussions). In further analysis of the data (see our first detailed comment to Referee #2 in which we consider the relative absorption of BC vs dust) included in the revised manuscript we bracket the upper range of potential BC concentrations at Mera glacier accounting the range of actual BC concentrations. This study presents much needed observational data of BC and dust deposition on the south side of the Himalaya, and should be of interest to ACP. ACP has published related research in which BC concentrations in snow were estimated based on previously published atmospheric measurements (Yasunari, T. J.; Bonasoni, P.; Laj, P.; et al., Estimated impact of black carbon deposition during pre-monsoon season from Nepal Climate Observatory - Pyramid data and snow albedo changes over Himalayan glaciers, Atmospheric Chemistry and Physics, 10, 14, 6603-6615, 2010.), or modeling studies (e.g. Menon, S.; Koch, D.; Beig, G.; et al., Black carbon aerosols and the third polar ice cap, Atmospheric Chemistry and Physics, 10, 10, 4559-4571, 2010; Flanner, M. G.; Zender, C. S.; Hess, P. G.; et al. Springtime warming and reduced snow cover from carbonaceous particles, Atmospheric Chemistry and Physics, 9, 7 2481-2497, 2009.) The major contributions of this manuscript are providing the first (and much needed) observational data from the south side of the Himalaya that show that there is a strong elevation gradient in BC and dust concentrations, and that albedo reductions due to dust exceed those of BC. These findings are important contributions, and will be of interest to ACP readers.

ï£ijThe authors report issues linked with sample preservation, a topic that could be introduced early in the manuscript.

-This information is provided in the abstract and the methods section, both of which are early in the manuscript.

They propose explanations for the elevation gradient in relationship with deposition and postdeposition processes, and estimate the impact of black carbon on snow albedo, with respect to the impact of other impurities. The introduction would benefit from a specific focus on the area of interest (High Asia) and backgroud about the structure of observed climate trends (e.g. seasonal and elevation patterns in temperature trends) and glacier mass balance changes.

-At the end of the second paragraph of the introduction we have added a sentence on temperature and glacier mass balance changes in the region, and the new section 2.1 (site description) provides further information about what is known about mass balance changes at Mera glacier, and the regional climate. We haven't added details in the introduction about seasonal and elevation patterns in temperature trends in the introduction because the referenced sources discuss these factors in further detail, and a detailed discussion on what is known about temperature in the region digresses from the main focus of the paper. As for focusing on High Asia in the introduction, with the exception of the first paragraph of the introduction that introduces the topic of black carbon, the majority of the introduction already focuses on High Asia (second paragraph on the importance of BC in this region, third paragraph on observations on the north side of the Himalaya and Tibetan Plateau, forth paragraph on the need of observations for the south side of the Himalaya).

It would also be very helpful for the non specialist to report the issues linked with the preservation of snow samples, and the coherency (or not) in-between different studies with respect to the accuracy and standardization of measurements.

-We address the relevant preservation issues in the methods section, and further details of the method are presented in Wendl, I., Menking, J. A., Farber, Gysel, M., Kaspari, S., Laborde, M., and Schwikowski, M., Optimized method for black carbon analysis in ice and snow, Atmosphere Measurement Techniques Discussions, 7, 3075-3111, 2014. This is a non-trivial topic, which is addressed in other publications:

Schwarz, J. P., Doherty, S., Li, F., Ruggiero, S. T., Tanner, C. E., Perring, A. E., Gao, R. S., and Fahey, D. W.: Assessing recent measurement techniques for quantifying black carbon concentration in snow, Atmospheric Measurement Techniques, 5, 2581-2592, 2012.

Torres, A., Bond, T.C., Lehmann, CMB, Subramanian, R. Hadley, O., Measuring Organic Carbon and Black Carbon in Rainwater: Evaluation of Methods, Aerosol Science and Technology, 48, 3, 239-350, 2014.

Lim, S., Cozic, J., Faïn, X., Zanatta, M., Jaffrezo, J. L., Ginot, P., and Laj, P.: Optimizing measurement methodology of refractory black carbon concentration in snow and ice and inter-comparison with elemental carbon measurement method, Atmosphere Measurement Techniques Discussions, submitted 2013.

These publications demonstrate that BC as measured by the SP2 is the preferred analytical method (analysis is negligibly affected by the presence of other absorbing impurities, and is not affected by the filtration efficiency issues that are problematic for thermal-optical techniques). We reference all of these studies in the revised manuscript, but do not go into an in depth discussion as it is outside of the scope of the current study.

I also recommend to report the state of the art with respect to the seasonal deposition patterns and their causes. -Section 3.1 already discusses in detail seasonal deposition and how the seasonality is controlled by variations in emissions, atmospheric transport and precipitation. From referee 2's comment it is not clear what additional information is requested.

A key gap of this study is obviously the lack of direct, in situ albedo measurements, and this could be introduced somewhere.

-This is addressed in paragraphs 4 and 5 of section 3.3. The end of the introduction also mentions that we estimate changes in albedo.

Section 2 (site description) lacks any justification of the choice of the target glacier (Mera glacier). Was it chosen because of its proximity to the atmospheric observatory where BC measurements have been performed (Yasunari et al, 2010) ? What are the specificities of this glacier : recent mass balance changes, local climate trends, sources of aerosols ? It is expected to be representative of a larger area ?

-The newly added Section 2.1 provides more information on the site selection, and addresses the referee's comments. (see response to referee #1)

The methods which are described page 33497 should be critically compared to the methods used for earlier studies discussed in the introduction. The uncertainties associated with the data (end of section 2) should be summarized (what can and cannot be done with these datasets if they are lower limit values). -We refer the referee to the same studies as referenced above (Wendl et al., 2014; Schwarz et al., 2012; Torres et al., 2014; Lim et al., in review) that discuss the method in more detail. In regards to what the data can be used for, the original manuscript addressed this. We also expand on this in the revised manuscript when we compare the absorption of BC versus dust and scale the BC to account for BC losses, bracketing the upper range of BC potentially deposited on Mera glacier.

In section 3, page 33500, the meteorological background is described in the somehow

simplistic way. It would be useful to cite recent works on this aspect, such as a recent overview about the origin of moisture (Yao et al, Rev. Geophysics, 2013) and the residence time of moisture (Gao Jing et al, EPSL, 2013) (albeit these works are focused on water isotopes, they include an investigation of moisture sources and residenceï£ijtime, including satellite OLR information on convective activity).

-The new section 2.1 provides additional information on the meteorology (seasonal origin of air masses, precipitation timing).

Albedo (page 33502). Do I understand well that this paragraph (lines 5-11) is based on visual inspection of photographs ? How valid is this methodology ?

-We have omitted this paragraph from the revised manuscript.

Albeo calculation (page 33503-33504) : how has the SNICAR model been validated with respect to the interplay of snow properties and black carbon and other aerosols ? Is it well adapted for the case study ? What are the main sources of uncertainties ? More generally, what is the relevance of a calculation of local, punctual radiative forcing , considering the seasonal, elevation and areas which would be impacted by such processes? Wouldn't an estimate of surface snow albedo changes as a function of black carbon and total aerosol load be more useful ? Specifically, for the high elevation glacier sites, what is the seasonal aspect of the mass balance, a feature which could be critical for the proposed mechanism by which impurities would remain exposed ? It seems to be that it is feasible to have information on the frequency of e.g. spring/summer snowfall and therefore assess the duration of surface snow exposure time.

-One of the great problems for science in the Himalaya is the nearly complete lack of knowledge of spring/summer snowfall, let alone the seasonal/elevation/aspect variation in deposition of dust and BC. So, yes, we agree with the reviewer that it would be better to have all of these measurements and estimates. However, working in the Himalaya is extraordinarily difficult and dangerous. The reviewer must understand the effort and risk that was put into acquiring such measurements (see photograph of the first author in the crevasse), and as such, they are unique measurements. Modeling local, punctual radiative forcing is warranted. Extrapolation of these to constrain RT modeling on other aspects would be, despite the desire, unwarranted given that we do not have spatially distributed albedo or radiative forcing measurements from remote sensing to guide such extrapolation. The SNICAR model (Flanner et al 2007; Flanner et al 2009) has at its core the two-stream approximation (Toon et al, 1989) to the radiative transfer equation, which has been shown to be quite accurate through the decades (Wiscombe and Warren, 1980; Painter et al 2007). The framework around using SNICAR is well established to handle changes in snow properties and BC and other aerosols. The main issue, which is the issue for all radiative transfer of impurities in snow, is the poor regional knowledge of the variation in dust and BC optical properties. This is acknowledged in the paper but we highlight it further in discussing the modeling as also requested by the other reviewer.

Page 33507, the authors have a very general discussion which an extrapolation of their findings for the whole area, discussing the distribution of snow coverage as a function of elevation. This extrapolation would first require confirmation of the patterns observed from this very limited dataset through other profiles and through different seasons.

-As discussed elsewhere, the logistical effort of acquiring observational data from this region is logistically very challenging, which is why there is a paucity of observational data. While it isn't possible to obtain this data for the current manuscript, this is something we hope to do in the future (a proposed project is in review). We have modified the text in the third paragraph of section 3.4 to read, "Assuming that the elevation gradient in BC and dust concentrations observed in this study also exists on other glaciers in the region, it can inferred that. . ."

A cross-discussion with respect to the elevation range where negative spring summer mass balance can be identified would also be relevant. The discussion of impacts of albedo changes for other glacier areas does not seem fully relevant here (33508).

-We need clarification from the reviewer. It is not clear what is being requested here.

Finally, many aspects of the abstract and discussion/conclusion are very general considerations which bring no clear new finding. I would suggest to make a much shorter manuscript, more clearly focused on the limitations and new findings emerging from this limited dataset, rather than trying to extrapolate these results for a much broader area. An interesting point lies in the overlap between the radiative effects of dust and black carbon, and therefore the need to have more accurate measurements along snow profiles.

-The revised manuscript gives much more consideration to the relative absorption of BC versus dust using the Fe data to inform the dust concentrations (see first detailed comment to referee #2).

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