

Interactive comment on “A 10 yr record of black carbon and dust from Mera Peak ice core (Nepal): variability and potential impact on Himalayan glacier melting” by P. Ginot et al.

Anonymous Referee #1

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The paper, “A 10 yr record of black carbon (BC) and dust from Mera Peak ice core (Nepal): variability and potential impact on Himalayan glacier melting”, by Ginot et al. firstly reported the ice core proxies obtained from the southern flank of Nepalese Himalaya, which include the important light absorbing impurities (dust and BC) on snow albedo discussion. The paper was overall well written and the ice core data were worth publishing. However, I have some major concerns and the authors need to clarify these points before the publication.

(Major concern) 1. They measured BC mass concentration in the ice core using SP2 with the nebulizer. As in detail discussed on the accuracy of the measurement by Jenkins et al. (TCD, 2013; see at: <http://www.the-cryosphere-discuss.net/7/4855/2013/tcd-C3067>

7-4855-2013.html) and Kaspari et al. (ACPD, 2013; see at: <http://www.atmos-chem-phys-discuss.net/13/33491/2013/acpd-13-33491-2013.html>), the sample treatment before the analysis and the measurements of BC with SP2 using the nebulizer had larger uncertainty (very low accuracy). That is why they focused on “relative differences” rather than absolute concentrations though they still discussed the absolute numbers in the current form and they are problem and need revisions. In this study, the authors, however, only used a simplified correction factor of 0.56 and did not consider and discuss such lower accuracy of the SP2 measurement for liquid-phase samples (i.e., melt water samples) with sample treatment accuracy before the analysis. Based on Jenkins et al. (2013) and Kaspari et al. (2013), I do not believe that using such a simplified correction works well for the absolute numbers of rBC concentration in the ice core samples. The authors need to show the validity of using the simplified correction factor to fully correct the rBC mass concentration in the ice core data. If they cannot show, they also need to show the relative numbers in their table and figures of this study. Then, they also should not use the absolute number of BC for the snow albedo and radiative forcing calculations (i.e., only keep discussing dust snow darkening but the discussion of BC snow darkening is difficult in this study in terms of absolute number). Even if they cannot discuss such calculations, still it is worth showing the relative numbers in figures for references. However, SO AS NOT GENERATE MISUSING OR MISLEADING BY ANY READERS, the authors strictly need to choose the decision whether they can use the absolute number of rBC for the discussion or not. If the absolute number will be shown in the published table and figures under the meaning less absolute numbers, readers in cases just grab the numbers to use their papers and that will further generate misusing and misleading. Therefore, this point is the most important comment of my comments. The discussions with Jenkins et al. (2013) and Kaspari et al. (2013) are essential in the revision, in which Kaspari et al. (2013) obtained snow samples from the same Mera Glacier. The discussion of this study with Kaspari et al. (2013) would provide more information of snow darkening over the Mera Glacier.

2. In Section 4.3 and 4.4, they estimated glacier melting based on the proxy informa-

tion with calculation and energy balance model. However, I do not think the application of ice core proxies obtained from an accumulation zone to the ablation zone is meaningful because the proxy condition would be quite different. This further gets readers confused and in cases generates misleading. If the authors would like to discuss the glacier melt amount with dust and BC concentrations, the observations like Kaspari et al. (2013) in the ablation zone should be necessary. If the authors discuss the impact of the proxy in the accumulation zone with the same method, this would be fine. Together with above, the authors need to re-consider the discussion in Section 4.4. In addition, the method of energy balance calculations was not mentioned in details and I could not follow how they calculated. Please explain in details how to calculate. Also, the authors need to summarize the mass balance using a schematic showing from the ablation to accumulation zone of the glacier. Only mentioning the numbers with sentences is difficult to see the whole structure of the mass balance. In addition, I could not understand “effective melting” at P6023L1. Please explain this in details.

(Minor points) P6004L14; P6007L2-6: References lacked.

P6004L29: They mainly discussed dry depositions.

P6005L14-27: The authors should include the discussions from the latest papers by Jenkins et al. (2013; see above), Kaspari et al. (2013), see above), and Ménégoz et al. (2013; ACPD; see at: <http://www.atmos-chem-phys-discuss.net/13/31013/2013/acpd-13-31013-2013.html>).

P6007L25-28: How did the authors decontaminate the surface contaminations of the ice core samples? Removing the surface? Using ultrapure water?

P6008L15: Do you have a reference to determine the dust density?

P6012L25-28: This is an important result and should be in the abstract too.

P6013L13-25: The figures on the EOF analysis should be shown such as the scores in time series of each proxy. They can provide this as supplementary information.

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P6014L4-5: This is also an important result and should be in both the abstract and conclusion.

P6015L11: The smaller numbers should be spelled out (i.e., two rather than 2).

P6018L25-26: Although the dust particle size distributions were shown in Fig. 3, no size distributions for rBC were shown. The authors should add the size distributions for rBC too.

P6020L4-15: Very hard to follow the method of simulation. More detailed explanations are necessary. For example, did they only calculate one-day simulation for each month? Did the one-day simulation include hourly calculations? They can use supplementary information to explain these in details.

Table 1: The authors need to emphasize that the data were obtained from the accumulation zone of the glacier here too.

Fig. 2: If the authors cannot discuss the absolute number of rBC, replace the rBC data to the relative numbers. Dry season should be pre-monsoon season because the dotted lines correspond to the peaks of oxygen isotope.

Fig. 3: How did the authors determine the peak size?

Fig. 4: The gray line for PM is hard to see. Please change the color.

Fig. 5: Very hard to distinguish each by each. Please separate the cases of coarse and fine snow grain size. Then, four figures should be better.

Interactive comment on The Cryosphere Discuss., 7, 6001, 2013.

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