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Interactive comment on "Black carbon concentrations from a Tibetan Plateau ice core spanning 1843–1982: recent increases due to emissions and glacier melt" *by* M. Jenkins et al.

Anonymous Referee #3

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In the paper by Jenkins et al. on "Black carbon concentrations from a Tibetan Plateau ice core spanning 1843–1982: recent increases due to emissions and glacier melt", the authors analyzed black carbon (BC) mass concentrations in the ice core drilled from Guoqu glacier on Mt. Geladaindong. This ice core record would be hopefully useful to understand the BC distributions in the past over the Tibetan Plateau (TP) and the Himalayas. However, as stated by the authors, the ice core measurements unfortunately have much uncertainty in terms of sample treatments before the analysis and the nebulizer issues. I can understand that ice core drilling and its analyses take much time before starting scientific discussions and the effort on all the processes should be really acknowledged. Therefore, I am considering the possible way to the

publication on this paper. However, with such higher uncertainty, it would be hard to lead to the authors' current conclusions within the current analyses and discussions. Before the publication, further re-consideration of the current form and analyses (if the authors focus on the enrichment by melting; see #2 below) would be necessary. I also agree with Reviewer #1 that the current form is like a review paper in which the new ice core data was added with summary of the previous papers. The descriptions on the technical issues and summary of the previous papers were well written.

In my opinion, this paper has two choices for the publication:

1. The paper should be a REVIEW paper with the new ice core data on BC. The authors should summarize the current knowledge on BC and dust deposition, these in snow or glaciers, their impacts on snow albedo and radiative effect over snow over the Himalayas and TP including modeling, observations, and data analyses. More papers to fully cover and summarize this region in terms of dust and BC should be preferable. Then, please clearly state what we have already known, what the crucial issues should be for the near future studies to further understand the snow darkening related topics, and the technical issues on the BC measurements (already summarized in the current form). In this case, the discussion on the BC enrichment should not be emphasized unless the author do more analyses to convince us the enrichment with such as suggested below. If the paper would be REVIEW paper, the word, "Review", should be added in the title.

2. If the authors strongly would like to keep focusing on the enrichment, further analyses are necessary. All the current discussion was carried out with the annual ice core data. However, if the authors would like to discuss the enrichment above the superimposed ice layers, the ice core proxy should be shown along water equivalent depth with available proxy data. It is not necessary to analyze the whole depth but at least pick up several interesting core depths including the years with high and less melt features. The relationships on the depth differences among dust peak, oxygen isotope, ice or high density layer, and BC concentrations would tell us the possible mechanism of the

enrichment due to melt water. The analysis makes us convince the authors' point on the enrichment. For example, at some depths, BC and dust peaks were at similar depth indicating spring season with less melt features. But, at some depths, BC peaks were frequently found below the dust peaks and/or close to the summer layers (oxygen isotope) or ice/melt-experienced layers. Then, the enrichment due to the melt effect would be possible and be interesting to discuss. Showing the Melt Feature Percentage (MFP) such by Kameda et al. (Ann. Glaciol., 1995) using the Koerner's ice-percentage equation (Koerner, Science, 1977) together with the ice core proxies would also be very helpful. I think the authors probably did measure bulk density for each ice core and general stratigraphy before analyzing the proxies and the MFP calculations for some ice core sections for which the authors should look at are possible.

(The other major points)

1. The authors mentioned the reduction of the BC snow darkening efficacy in the presence of dust based on Kaspari et al. (Geophys. Res. Lett., 2011). Some of the co-authors in this paper were the authors in Kaspari et al. (2011). In my speculations, this efficacy discussion might be possible and is interesting. However, I have a question on the analysis in Kaspari et al. (2011) in terms of radiative forcing especially on the snow albedo calculations. Before writing the comments, I also carried out two snow albedo calculations with the SNICAR Online (Flanner et al., J. Geophys. Res., 2007) (see the attached figure): (1) CTL calculation with BC and dust darkening assuming larger grain size; (2) Similar to (1) but mass concentrations of dust for each size bin increased by a factor of five with the same BC mass concentration as (1). Then, actually Case (2) showed lower snow albedo. The calculations indicate that stronger radiative forcing would be expected with BC in the presence of higher dust mass concentration like Case (2). Fig. 7 in Flanner et al. (ACP, 2009) also showed the same features in which dust+BC snow darkening had stronger forcing than dust only forcing. I do not know why the Kaspari et al (2011) showed the opposite results even with the same SNICAR calculations in their Table 1. Did Kaspari et al. (2011) use the special settings on the

SNICAR parameterizations so as to consider the efficacy depending on the mixture conditions? Or possible calculation errors? In any cases, the efficacy discussion is only based on model-based discussion without any robust measurements. Hence, the authors should not conclude the reduction of BC efficacy only based on the model-based efficacy discussion from the one paper with poor ice core analyses in this study on the efficacy. This discussion in the current form is also losing the robustness of the paper. However, the topic on the BC efficacy in the presence of higher mass concentration of other light absorbing aerosol on the snow albedo reduction should be important and interesting and can be discussed but not lead to the conclusion in this paper.

2. The authors carried out the correction to the mass losses due to the nebulizer issue. However, as mentioned at L12-14 and L26-29 on p.4859, the correction cannot fully solve the loss effect on the size distribution and the mass loss has dependency on mass concentrations (high and low concentrations have different loss rate). Under the condition, is it possible to discuss the BC trend even if for the relative trend? Because the mass loss largely depends on the mass concentrations (high and low), the same correction method applied to all the ice core data probably does not work well. In addition, the loss dependency on mass concentration would affect the BC trend too. Then, I cannot understand why relative trend are preserved because each sample had different mass concentrations and then different rates of mass loss.

3. In addition to #2 above, although the authors said that the qualitative discussion with relative BC variations was possible. However, they discussed 2.0-fold and 2.4-fold values as seen at L19-20 on p.4861 and in the abstract. I think these are quantitative descriptions. Based on my question #2 above, are these numbers meaningful?

4. The ice core was drilled on the slope of the glacier (not at the summit). Does glacial flow affect discontinuity of the ice core data or bend the data? If it affects, the interpretation of the ice core would be further complex. Please explain this point.

5. The authors discussed the increase of BC since 1940 in Fig. 2. However, the trend was not clear for me. The annual data seem to frequently have higher spikes since 1940. But the trend was not clear.

6. The close relationship between dust and BC in Fig. 2 was also not clear for me as also mentioned by Reviewer #2. In some years, coincident peaks were seen and in other year they were not. Actually the correlation is pretty lower as shown in the correspondence. The authors added one more figure on the correspondence to Reviewer #2. However, if they would like to say, "close relationship" and "Seasonally it is well documented in this region that dust and BC concentrations are higher during the dry winter-spring", the ice core proxies along water equivalent depth should be shown and discuss the coincidences as I suggested above. The annual ice core data has difficulty to discuss this point.

(Minor points)

L13 P4857: . . ...(days to weeks). Ref. lacked.

L20 P4858: Put the name of the company for Aquadag. In addition, please briefly explain what the Aquadag is.

L26-29 P4859: The sentence on the mass loss dependency on mass concentrations is unclear for me. Please make a figure for readers easy to understand this sentence.

L19-22 P4860: Please add one sentence for the tables and figures that this SP2 measurement has mass losses and the mass concentrations are probably lower bound or something. Although the authors mentioned in the main text, some of readers sometimes use the tables or figures without looking at the sentences in details. So as not to generate the misleading by readers that the measured BC mass concentrations from this ice core might be realistic numbers, it is better to put the caution on the table and figures in terms of its absolute magnitudes.

L8-19 P4864: These discussions are also difficult only in Fig. 3 because unclear rela-

tionships. I again recommend the authors to use water equivalent depth for this kind of discussion rather than using annual data.

Fig. 1: Adding altitude information would be better to understand the location.

Fig. 3: I cannot understand why the authors used May-September temperatures.

The ice core dating method: Please add couple of more sentences to more explain how to carry out the dating on the ice core proxy.

References: I could not find Kang et al. (2013) in GRL and expect that is in review. Then, it should be clearly described as "in review". Similar to Kaspari et al. (2013) but I know that was published ACPD now and revise it properly. The reference, Grigholm et al. (2013) was missing in the reference list.

Interactive comment on The Cryosphere Discuss., 7, 4855, 2013.

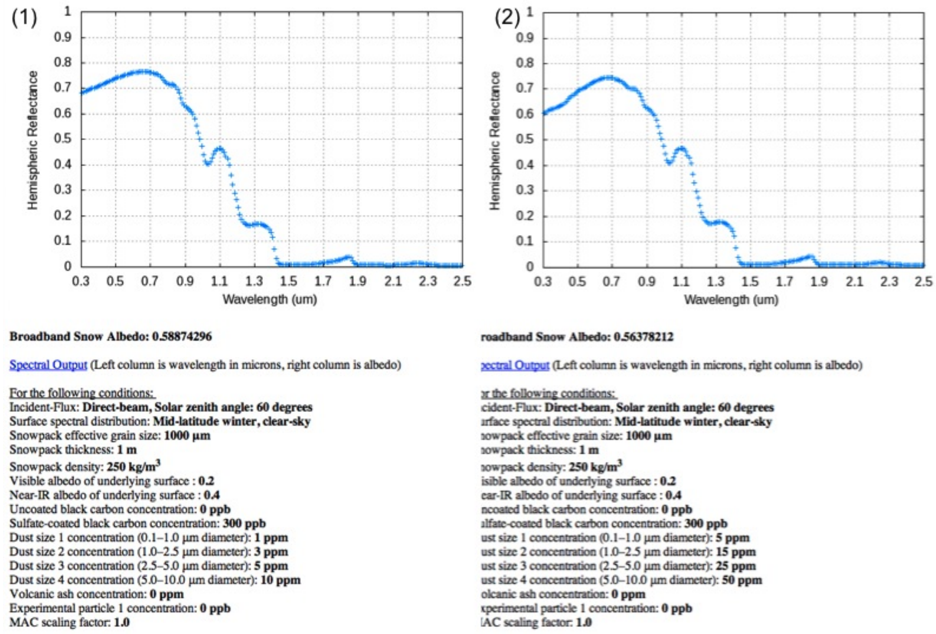

Fig. 1. Two snow albedo calculations by the SNICAR Online (Flanner et al., 2007; http://snow.engin.umich.edu/) with the same BC and different dust mass con. ((1): CTRL; (2) Increased dust by a factor of five)