

Interactive comment on “Black carbon and mineral dust in snow cover on the Third Pole” by Yulan Zhang et al.

Anonymous Referee #1

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Review for TC-2017-111 Title: Black carbon and mineral dust in snow cover on the Third Pole

General opinion: The authors report the observation of light-absorbing impurities in snow over Tibetan Plateau. Based on these field observed data, they calculated the albedo reduction induced by black carbon and mineral dust, and the corresponding impact on snow energy budget. The field data reported in this work are valuable for quantifying the impact of light-absorbing particles on snow albedo and the analysis based on this field data are informative. However, some discussion and conclusions given in this paper are not accurate and need to be modified. This manuscript also contains a lot of typos/grammar errors (some obvious errors are listed below) that need to be corrected before this manuscript can be considered for publication. Here are the

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suggestions/comments that the authors may find useful.

Comments and Questions

Page 1 Line 30-32: BC is recognized as an important climate forcer not only because it absorbs sunlight, but also because a large fraction of BC are emitted from anthropogenic sources. Please be more accurate at here.

Page 2 Line 16: "... snow covered range" → snow covered region?

Line 20: "Confirming radiative transfer modeling". It is not very clear to readers what does the authors mean. Please consider revise this.

Line 24: "..., but also to" → but also important/crucial to

Line 26: "... is the most sensitive" how could tell it is the most sensitive? Please remove most or provide supporting data.

Line 33: "... and result in perturbation in" → ...and perturb

Line 34: "5 – 25 mm in the snow water equivalent..." → 5-25 mm snow water equivalent

Page 3 Line 1: "snowpack on the TP is associated with the ..." by associated you mean the snowpack on the TP is influenced by summer monsoon? Or the snowpack will influence summer monsoon? or both? Please be more accurate here.

Line 4-5: "Simulation studies of BC in snow over TP have inherent uncertainties because of the lack of large-area observations of BC data in seasonal snow cover". This is not correct. Model simulations have inherent uncertainties due to the physics/chemistry/transport schemes used in the models - such as uncertainties in BC emissions sources or deposition rate. Large-area observation of BC in TP will be useful for model evaluation, but it will not help with the inherent uncertainty of the model simulation. Please revise it here.

Page 4 Line10: "... and is dominated by the Indian monsoon" → A region cannot be

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dominated by monsoon. Do you mean “affected by”?

Line 11: “Region III has one site (LHG) is located in the northeastern part of the TP”
→ “Region III has one site (LHG) that is located in the northeastern part of the TP”

Line 15: it seems the snow samples were only collected from the top 5 cm, so how did you calculate the snow albedo for snowpacks thicker than 5 cm? Did you assume the BC/MD mass mixing ratio is constant through the entire snow column? Please clarify this.

Line 17: how did you measure snow grain size in the field? Please clarify this.

Line 24-26: what is the accuracy of the weight you used to measure filter before and after filtration? It seems the author assume BC/OC/MD are the only insoluble particles deposited on the quartz filter, could you please provide more evidence about this? If MD were the only other particle in snow (besides OC and BC), do all MD absorb sunlight? It might be a good idea to include these discussions in the uncertainty analysis.

Page 5: Line 6: what is the filter blank for? is this a blank filter? It seems there is some particles on filter blank as well since it weighs more than 0 C/cm-2.

Line 8: “separately analyzed” → analyzed separately

Line 18: “. . . is or not influenced by BC emissions” → is influenced by BC emissions or not

Line 23: what is “down-sun”?

Line 24: “when the weather was clear”→ when the sky was clear. It seems the albedo measurement were made for clear sky only, but later in the albedo comparison, the measured albedo is compared against the albedo calculated using SNICAR for all-sky case. Is this a typo or this is wrong?

Line 28: “e.g. Doherty et al., 2010”: Doherty et al 2010 did not use SNICAR, it only reported the observation in the Arctic. Please remove this citation.

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Page 6: Line 9-11: Please revise this sentence. Is this a model or is this just the method you used to calculate snow water melt in this work?

Line 16: “for clean snow. . .” what is cause of albedo reduction for clean snow case? a different snow grain size due to snow aging? Please be more accurate here.

Line 18: “assumed snow depth” what is the assumed snow depth? How did you assume it?

Line 22: “older wind-packed snow” → old wind-packed snow

Page 7: Line 2: Please consider cite paper Doherty et al., 2016. It also reports LAIs in snow in North America. Doherty, S. J., D. A. Hegg, J. E. Johnson, P. K. Quinn, J. P. Schwarz, C. Dang, and S. G. Warren, (2016), Causes of variability in light absorption by particles in snow at sites in Idaho and Utah. *J. Geophys. Res. Atmos.*, 121, no. 9: 4751-4768.

Line 6-8: How could you tell the LAIs in snow over TP were generated from biomass burning in surrounding region from Table 2? And from Table 2, how could you tell if the fraction of LAIs emitted by biomass/fossil fuel burning in TP is larger than that in the other regions? Please provide more explanations.

Line 9-11: How could the ratio of OC/BC be used as a standard to determine the emission sources of biomass burning? What is the OC/BC ratio if all LAIs were emitted from biomass burning or non-biomass burning sources?

Line 15: “LHG3 AND LHG6 (Figure 3)”, do you mean Figure 1b?

Line 15-21: It seems Figure S2 is an important figure for discussions in this part. If there is no restriction on the number of figures, please consider include this figure in the paper.

Line 28: “Open burning sourced BC” → BC emitted from open burning sources. Does the BC emitted from open burning sources contain BC emitted from biomass burning?

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or are they the same? Please be more specific about what you mean by “open burning”.

Page 8 Line 3-4: “In the southern TP. . . . due to influence of BC sources in the south Asia”. This statement is too vague. Do you mean the total emission is larger in southern Asia? or the BC deposition rate is larger ? or both? This is a major conclusion you have in this work, please be more specific.

Line 8: You used SNICAR model to calculate snow albedo for all-sky cases, but the model set up is not stated. For example, what is the cloud fraction for all sky case? As mentioned in the previous comments, the albedo measurement was performed under clear sky, so in what accuracy do you expect this to agree with the SNICAR calculation (Figure 5)?

Line 14: “The deviation between measured and simulated reflectance by MD may be a result of the upper boundary of the SNICAR model in particle dimension”. This does not make sense, is the deviation due to upper boundary condition (and what upper boundary condition)? or MD particle size? Please revise.

Line 18-19: “This result is import, showing that the SNICAR model simulations can represent albedo changes of snow cover in the Third Pole region”. This is a really strong conclusion. I don’t think the authors can make this conclusion based on the results shown in this paper. Especially it is unclear to the readers that how did the authors set up the SNICAR calculation. Please remove this or include more details about SNICAR calculation.

Line 26. As the author said, the BC snow albedo forcing over TP is highly uncertain partly due to the uncertainty in simulated BC concentration. But it is also important to point out that a large fraction of such uncertainty is resulted from uncertainty in simulated snow-cover fraction/snow depth. Please consider including this in the discussion.

Page 9 Section 3.3: Why did you pick SW flux of 220, 270 and 310 W m-2? Line 27: “SD plays an import role” → SD plays an important role? What do you mean by SD

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plays a role here? Deeper snow is supposed to melt slower given the same amount of radiative forcing; it is not because snow depths play a role here. Please clarify this point.

Page 10: Line 13: “However, the results presented in this study for which these assumptions are not critical”: This is not true. All the quantities listed in this paragraph will influence the snow albedo and most of them will influence the albedo reduction induced by BC. For example: BC-snow internal mixing increases the albedo forcing by 40-60% compared with external mixing (He et al. (2014). The author should discuss the uncertainty of this study resulted from the assumptions they made, instead of claim these quantities will not impact their results.

References: He, C., Q. Li, K. N. Liou, Y. Takano, Y. Gu, L. Qi, Y. Mao, and L. R. Leung, 2014: Black Carbon Radiative Forcing over the Tibetan Plateau. *Geophys. Res. Lett.*, 41, 7806-7813, doi: 10.1002/2014GL062191.

Line 32: “Our study confirms that and further reduces snow albedo,”: this is not true. BC and other LAI can reduce the snow albedo even if the snow aging process is not accelerated. Please revise this.

Figures3: Is the color bar showing height? Please define the color bar and unit.

Figure 5: Are MA1-4 measured albedo? Are the dashed lines albedo calculated using SNICAR? Please clarify these details and modify the corresponding text.

Figure 6-8: please clarify the figure convention in each figure. Do the boxes represent average values from central estimate? For example, in Figure 6, you say the rectangles are central estimate, so what does the box mean? standard deviation of central estimate? maximum and minimum of central estimate?

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