

Interactive comment on “Concentrations and source regions of light absorbing impurities in snow/ice in northern Pakistan and their impact on snow albedo” by Chaman Gul et al.

Anonymous Referee #2

Received and published: 26 September 2017

This study is valuable because it describes measurements of black carbon in snow from the Karakoram/Himalayan region of Pakistan. To my knowledge these are the first such measurements to be reported from this region. Moreover, the reported concentrations of BC in snow are extremely large, indicative of pollution being a major source of snow/ice albedo reduction in this area. The study is also comprehensive in the sense that it applies CALIPSO observations of aerosol type, back-trajectory analysis, and regional chemistry/climate modeling to ascertain dominant sources of pollution to the snow and glaciers in the study area. Despite the value of having new measurements from the Karakoram, a region with a paucity of environmental data, the study has some weaknesses that are described below. Ultimately, I believe these issues lead to conclu-

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sions which are somewhat vague. I suppose the main take-home message, however, is that there is a lot of BC in low elevation glaciers and snow of northern Pakistan, and perhaps this is a sufficient conclusion in and of itself for publication. Below, however, are the major issues I see with the current draft of the paper.

(1) The CALIPSO aerosol source identification analysis indicates that "smoke" is the most frequently-occurring type of aerosol over this region during both summer and winter. As the authors acknowledge, however, biomass burning sources were not included in the WRF-STEM modeling, and thus the dominant source regions identified through the WRF modeling may not be representative at all for the BC that was measured. Moreover, were biomass burning sources included in the RCP emission inventory that was utilized with the back-trajectory analysis? (Please include more information about the RCP emissions that were used.) A third question related to the source attribution analysis is: Potentially how important are local (e.g., within ~10km) sources occurring within the same "grid cell" of the WRF and HYSPLIT models? Contributions of such local (sub-grid scale) sources may be severely underestimated by coarse-resolution models. Some of the discussion suggests that local sources may have been very important, but these sources did not really enter into the assessment (via HYSPLIT and WRF) of source attribution.

(2) The values of BC in snow that were found are extremely large, but it is also acknowledged in the paper that the measurements were taken close to sources of pollution, namely roadways and villages. I am left wondering how representative the reported snow pollution values are of the broader Karakoram cryospheric region. The answer to this may not be known, but some discussion, even if speculative, about this issue would be appreciated. Do these measurements suggest that the glaciers of the Karakoram, in general, are being substantially darkened by BC, or do they simply mean that the ablation zones of a few glaciers near to obvious BC sources are quite polluted?

(3) The authors report that "there was no clear correlation between BC and OC concentrations" (line 269), which I found a bit worrisome given that the two species usually

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originate from common sources and have common transport pathways. The authors do provide some potential reasons for why we could find more BC than OC in the snow (e.g. ,greater melt scavenging of OC), which was also a bit surprising, but I would appreciate seeing some more discussion on why concentrations of BC and OC would be uncorrelated.

(4) Related to the point above, how precisely was OC differentiated from BC in the thermal optical technique? What temperature threshold or thermal evolution profile was applied to separate the two species? Could this have had anything to do with the high BC/OC ratios that were found in the snow samples?

(5) More generally, please describe and if possible quantify, sources of uncertainty in the measurements of BC, OC, and dust in snow.

(6) My sense is that snow grain size and snow texture are larger sources of uncertainty in the albedo reduction / RF calculations than indicated. Although snow grain size was measured with a hand lens (with reported accuracy of 20um), this determination of grain size is usually different from the effective (surface area-weighted) / optical grain size used in the SNICAR modeling. The true uncertainty in effective/optical grain size is likely much larger than 20um, and I think the paper should include greater acknowledgment of this issue. The discussion of albedo variability associated with snow grain size (or snow aging) should also more clearly indicate the ranges in snow grain size that were assumed for the albedo modeling.

Furthermore, references to "snow age" are sometimes used when "snow grain size" would be more appropriate, since snow grain size does not always increase monotonically with snow age, and it is really the snow grain size that matters for optical/radiative considerations. Examples of this is are on line 364: "The estimated reduction in snow albedo by dust and BC compounded by the age of snow..." and line 386: "... exact snow age ...".

(7) Snow albedo and perturbations to albedo are modeled and used heavily in this

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study to derive radiative forcing estimates, but no observations of snow or ice albedo are reported. Are there any observations of snow and ice albedo from this region that could be utilized to help verify or support the modeling? I worry in particular that debris could strongly reduce albedo of the glaciers but is neglected in the model, potentially leading to bias in the modeled albedo perturbations.

Minor issues:

line 211: "... were put in the above equation and got a normalized extinction..." - grammar issue.

line 251: "... with the generally lower deposition on the Gulkin glacier more affected by other factors" - Which factors?

line 257: "is considering as" -> "considered as"

line 274: "... low OC/BC ratios can result from a reduction in OC, greater contributions from BC enrichment..." - It is unclear to me which processes "reduction in OC" and "BC enrichment" refer to. Could the authors please elaborate on these processes?

line 329: "albedo of samples from the two sites simulated at a wavelength of 0.975um ... " - Why are 0.975um albedo values reported here? Light-absorbing impurities exert the strongest influence on blue or mid-visible albedo (e.g., ~0.450um). The 0.975um albedo is affected less strongly by impurities, and somewhat heavily by snow grain size, so it seems an odd choice of wavelength to use for reporting albedos.

line 336: "The results suggest that BC was the dominant forcing factor, rather than dust, as a result of the rapid snowmelt." - The identification of "rapid snowmelt" as the cause for greater BC forcing than dust forcing is confusing here. Perhaps the sentence just needs re-working. Otherwise, what role does snowmelt play in the determination of instantaneous radiative forcing?

line 343: "... reduction in daily mean albedo of 1.8 to 2.9% ... " Are these relative or absolute reductions in albedo? If the latter, please use absolute (non-percentage)

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units. This also applies to other references to percent albedo reduction in the paper.

lines 400-401: Which environments do these RF estimates apply to?

lines 406-410: It should be acknowledged again that dust forcing varies strongly with dust optical properties and particle size distribution. The estimates derived here appear to have utilized a generic representation of dust in the model that may or may not be appropriate for the dust that was actually measured.

lines 461: "BC from East Asia can potentially be lifted up high and transported to the northeast during the summer monsoon season. Nonetheless..." - But transport of East Asia emissions to the northeast does not seem relevant for deposition in Pakistan. Please clarify the relevance of this statement.

line 463: "... low latitude source regions such as South Africa..." - I suggest using "tropical Africa" or something similar here instead of "South Africa" (which happens to also be the name of a country).

line 464: "weak emissions" - Actually, biomass burning emissions from tropical regions of Africa constitute a substantial share of global BC emissions, so "weak" may not be the best word here.

line 479: "considerable" -> "considerably"

line 482: "The concentration of hydrophobic BC, hydrophilic BC, ..." - The description earlier in Methods indicated only that CO tracers were used. Was BC also simulated with this model? If so, were BC tags applied? Please include more description of the BC simulation in Methods. This seems much more relevant for source attribution, since the physics and chemistry of removal for BC and CO are quite different from each other.

line 520: "... and increased grain size and density." - It is not clear to me how snow grain size and snow density should affect the *concentration* of BC, as indicated in this sentence. Please clarify.

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Figure 2: Most of the figure is white space. I suggest shrinking the y-axis range to show the plot values more clearly.

Figure 6: Please specify which emission inventory was used and how many days of back-trajectory were simulated.

Table 2: Are these relative or absolute snow albedo reductions?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-667>, 2017.

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