Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-935-RC1, 2022

Comment on acp-2021-935

Anonymous Referee #1

Referee comment on "Measurement of light-absorbing particles in surface snow of central and western Himalayan glaciers: spatial variability, radiative impacts, and potential source regions" by Chaman Gul et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-935-RC1, 2022

General comments on the overall quality of the preprint

The manuscript seems to be closely related to a 2021 EGU presentation (https://meetingorganizer.copernicus.org/EGU21/EGU21-8515.html), and a 2021 paper in Environmental Pollution (https://www.sciencedirect.com/science/article/pii/S0269749121001226?via%3Dihub), by many of the same authors as the submitted manuscript. However, the latter is focused on results from 2017, whereas the present results focus on 2016. Overall, the methodology is sound; in particular, analysis of black carbon and organic carbon in snow is based on Thermal-Optical Analysis, which is widely employed to distinguish organic carbon from elemental carbon in atmospheric aerosols. A novel aspect of the work is the integration of WRF-Chem to compare to observations of impurities in snow.

Specific comments addressing individual scientific questions/issues (section)

- Introduction line 87: I am not sure what "snow shape" is. And I think "snow texture" might mean "snow surface texture". For both, it would help if the authors clarified the scale of these features: millimeters? meters?
- I find the description of the snow sampling to be inadequate. On lines 133-134, the manuscript refers the reader to a 2021 paper by Gul et al for details, but that paper describes sampling in a different year (2017), with specific references to dates that obviously can't apply to the present work, e.g., Before the commencement of snow sampling on May 1, 2017, there was fresh snowfall around the study site. The mean snow thickness of fresh snow was around 15–18 cm and we collected samples from the top 7–10 cm layer. Critical aspects of the sampling protocol employed in this manuscript are therefore unclear to me: were the samples taken in 2016 (this work) also mainly of fresh snowfall? How thick was the fresh snow? At what depths were

samples taken? At altitudes above 5000 meters, it will be obvious to some readers that the area lies above the tree line (which is a common source of debris at lower altitudes), but it would be useful to state that; a photograph of the sampling might also be useful.

- I want to commend the authors for their careful analysis of the discrepancy between the magnitude of BC loading in snow as reported here, compared to other regions -- on the order of 100-1000. I have just a few items I would like clarified about this.

- The authors allude to a possible cause of this discrepancy as "strong melting of surface snow and ice in the glacier ablation zone [that] could lead to BC enrichment which causes high BC concentrations (Li et al., 2017)". But how is that consistent with the description of the samples as "fresh" snow (see my comment #2)? [I know that the authors are aware of these considerations: a recent paper in Earth-Science Reviews co-authored by Kang, states that BC concentrations on an "intensely ablated surface" can be on the order of 1000 ng/g.]
- Use of WRF-Chem runs to explain these numbers is a really great idea, but I feel that characterizing the discrepancy between those results and observations (in the abstract) as "a relatively smaller magnitude" understates it, as does the phrase "almost similar" in line 330. In contrast, looking at Figure 2, the Sachin May results, I'd say the observations are ~5x the WRF-Chem results. Also, and I guess more fundamentally, does the WRM-Chem model incorporate impurity enhancement due to snow ablation?
- I appreciate the discussion of uncertainties in the paragraph beginning on line 297. However, numbers reported elsewhere in the manuscript have unreasonably high precision, in my opinion. For example, in Line 210, the authors report an average concentration of BC at Sachin, during May, to six significant figures. The authors should justify the number of significant figures in values they report, and revise accordingly.

4. Figure 3 does not seem to be referred to in the body of the manuscript.

Technical Corrections

Line 1: Why is "Light" capitalized in the title?

Line 29, Abstract: "were quite higher compared" => "were quite high compared"

Line 68, Introduction: "Mountain glaciers are the most important freshwater resources to the lives of arid and semi-arid regions" => "Mountain glaciers are the most important freshwater resources to the inhabitants of arid and semi-arid regions"

Line 69, Introduction: What is "The great Himalayas"?

Line 78, Introduction: “it is still large uncertainties for” => “large uncertainties remain regarding”

Line 113: I think “mostly covered by **firm**/snow” is intended to be “mostly covered by **firn**/snow”.

Line 119: Punctuation issues on this line, and other places in this paragraph, need fixing.

Line 131: I think “few snow samples were also collected” should be “a few snow samples were also collected”.

Line 159: I don’t understand the sentence: “RF-based on measured BC and dust concentration in our samples were estimated using the following equation.” I understand that “RF” means “Radiative Forcing” … but what is “RF-based”? Also, the formatting of the equation on the following line is unconventional.

Line 169: The sentence “To identify the potential source region of pollution for the central and western Himalayan glaciers, the weather research and forecasting (WRF) model coupled with chemistry (WRF-Chem version 3.9.1.1) (Grell et al., 2005) tagged-tracer simulations for the selected sites” seems to be missing a verb.

Line 207: “Possible reasons for the lowest concentration” => “Possible reasons for the lower concentration” (I think).

Line 389: The Gul et al citation does not seem to include the year of publication.

Line 558, Figure 2: I don’t understand the simulation results for Sachin(Oct) – maybe the figure was garbled? Also, the caption doesn’t explain the meaning of the “x” and “+” symbols.