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Comment on acp-2022-604

Anonymous Referee #1

Referee comment on "Meteorological export and deposition fluxes of black carbon on glaciers of the central Chilean Andes" by Rémy Lapere et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-604-RC1, 2022

The work presented by Lapere and co-authors investigates the seasonal and spatial variability of black carbon deposition fluxes in the Central Andes. The topic is not very well represented in current literature; hence the present manuscript is of scientific interest for ACP. Overall, the manuscript is well written, however, the spatial (regional) and temporal (1 year) scale of deposition analysis is not coherent with wind analysis and observations (which goes from continental to local and from decades to hours). From my point of view, this undermines the conclusive message of the manuscript. I do recommend the authors to work on the manuscript for a second round of review. I hope that the following major and minor comments will help the authors in improving the quality of the present manuscript.

MAJOR COMMENTS

INTRODUCTION: The scientific topic of the paper is clearly the identification of sources of BC in snow. However, the forcing mechanisms, and the climatic implications, in the context of the sever draught currently striking Cile, are not sufficiently explained and/or justified in the introduction. If the mass balance of glaciers is dramatically decreasing, modifying the hydrological balance of the region, also the role of BC and its faith will change in the future. What would be the climatic impact of BC in-snow in the current and future context of a short "snow season" and reduced glacier extent? What will be the impact of massive release of BC from the glaciers on the dissolved organic carbon of rivers, lakes and oceans? These questions, which are often disregarded by the BC

atmospheric community, should motivate the authors to extend the motivation of their work in the introduction. I might recommend the following literature: https://doi.org/10.1016/j.gloplacha.2022.103837 ; https://doi.org/10.1016/j.earscirev.2017.09.019 ; https://doi.org/10.1038/s41598-019-53284-1 ; https://doi.org/10.1038/nature04141

SECTION 3.1: This section is particularly long; and, I have the impression that the seasonality and spatial variability of deposition fluxes are repeated multiple times in various subparagraphs and figures. To simplify and shorten the section I recommend the following. 1) Merge Figure 1 and Figure 2, focusing on the seasonality of emissions and deposition fluxes on a larger regional scale, neglecting the direct influence of Santiago emissions (up to me, it is easy to guess that the influence of Santiago emissions will decrease with transport distance). As it is Figure 2 is a mere repletion of Figure 3, with a broader resolution; 2) Focus on the detailed influence of Santiago on the single glaciers as nicely done in Figure 3. Potentially, it might be a good idea to introduce subsections 3.1.1 and 3.1.2.

SECTION 3.2: The authors provide many information on wind conditions, potentially a bit more than needed. The deposition fluxes are based on two months of 2015 (July and January), the synoptic atmospheric circulation is based on 10 years of monthly averaged reanalysis data, the wind profile measurements covers the 2017-2019 period with 3 months average and hourly resolution, and the local ground measurements covers a variable spans of years. It is evident that none of the timescales and averaging periods are coherent. Similar discussion could be done for the spatial scale, the manuscript moves from regional scale in Figure 1,2,3,4 to continental scale in Figure 5 and then local ot punctual in Figure 6,7. The consequent question is, do all these data, directly, support the analysis of BC regional export and deposition for 2 months of 2015? The authors might think of using solely the local data available for 2015, while synoptic scale might stay as it is.

SECTION 3.3: given the wind speed resolution of figure 5, 6, 7 is hard to believe that a change of 0,03 or 0,02 m/s in wind speed would influence export or deposition of BC in 10 years. Similar comment could be done for the 1.4° change in the wind direction for Figure 7. This section is definitely interesting, and should be kept in the final version of manuscript, but the authors should underline the big uncertainty related with this "projection": standard deviation is approximately 10 time larger than starting and ending values (Fig8a-b); precipitation pattern might change, modifying removal mechanisms; etc … Since no future projections for deposition fluxes are ever shown, this section is mostly based on speculation and open guesses.

SPECIFIC COMMENTS

L27: Is there an estimation of the forcing caused by dust in snow? Is it comparable to BC?

L28-35: Here the authors states that Cile is facing an extreme drought, accelerating the melting of glaciers. One can argue that the absence of precipitation and rise of atmospheric temperature are the leading factors causing glacier melting, rather than BC. I suggest the authors explaining more in details the role or the implication for BC budget in snow at line 34-35.

L51: remove "but"

L153: are these total deposition fluxes (dry+wet)? Worth mentioning.

F1: colour scale reads "molecules s-1 cm-2". Shouldn't be "particles s-1 cm-2"?

L159-161: are these studies showing higher BC loading for winter or summer? Are they supporting your deposition seasonal cycle? Without specifying the cycle of BC in concentration in snow, this part of the text does not provide any relevant scientific information and could be easily removed.

L163-168: if the "comparison" with Tibetan Plateau is provided in the discussion section, I suggest to minimize the explanation here and simply report that the deposition fluxes of this study are higher or lower than in other regions.

L227-245: I am quite confused by the dashed line in Figure 4a. This is basically scaled with a constant factor using July emission. It should be shown that deposition fluxes are directly proportional to emission intensity, in the supplementary. Overall, this approach is quite questionable, what would happen when scaling deposition fluxes in July using emissions of January? Figure 4b can go in the supplementary. Second comment, glaciers should also be divided in distance from Santiago (or latitudinal bands), especially in the January period. As shown in Figure 3a, the highest deposition fluxes are in the proximity of Santiago in January, at what altitude are those glaciers?

L249-256: repletion of what already said in L246-249. Group these phrases, avoid repetitions. Do not use "in conclusion".

L258: what is the pressure or altitude level for wind speed derived from ERA5?

F6 Panel b and c are too detailed, compared to the monthly resolution of deposition fluxes, they should be removed together with the discussion in the text

F4-6: elevation appears to be different or not well labelled (Figure4). Make it consistent.

Section 4: the discussion section repeats, mostly, what already discussed in previous section. Considering the length of the paper, I suggest removing the full section. Part of the section can be implemented elsewhere in the manuscript.

L459: the manuscript does not provide any proof of BC causing faster melting in the Andes. Plus, very little insights are given on the forcing mechanisms of BC in snow. Authors should be more careful on these generic statements.

F8: show standard deviation for both simulations or for none.