

GENERAL COMMENT

In this study, the authors developed a model and used measurements to evaluate the impact of BC and MD deposition on the Laohugou Glacier over the hydrological year 2011-2012. They quantified the effect of BC and MD loads on albedo and the glacier mass balance. They estimated an impact on melting larger than 50% related to the combined effect of BC and MD. They also investigated the impact of BC and dust separately and quantified the impact of BC (MD) on melting by about 13(12)%. Finally, they assessed the effect of the temperature and BC increase since the Industrial revolution on the glacier melt. Despite the interest in the study, I recommend major revisions/clarifications of the paper before publication.

MAJOR COMMENTS

1. One of my main concerns is that this study relies on strong hypotheses, that are not well justified and never discussed. These hypotheses mainly concern the method used to compute the LAPs depositions and the snowpack/mass balance model.

I therefore strongly recommend to the authors to better justify the hypotheses made (see specific comments below for more details). I also advise the authors to discuss the hypotheses made (for instance by adding a specific section "limitations" in the discussion section). Additionally, uncertainties in this study, related to measurements and modeling, should be quantified or sensitivity tests performed, to ensure the relevance of the results presented in this study. Indeed, some results appear surprising or sometimes contradictory from the literature (see specific comments).

2. Another concern is about the method used to quantify wet and dry BC depositions.

It is very unclear how BC in precipitation was estimated. It seems to be based on strong hypotheses and is thus prone to huge uncertainties. Then, the dry deposition is estimated from the total BC measured in the ice – wet deposition. This is also prone to significant uncertainties (i.e. measurements uncertainties + the one from wet BC).

Otherwise, BC measurements in snow/ice in 2016 are used to quantify total BC deposition and the atmospheric BC measured in 2012 to reconstruct the timing of the deposition. These data are then used to simulate the effect in 2012. Due to the time inconstancy, a very strong hypothesis is made. Are BC depositions really similar/comparable from one year to another? More information or additional work would be necessary to validate this hypothesis, and uncertainties and limitations of this method should be clearly exposed.

Thus, first I strongly encourage the author to clarify the method (a scheme could help). Second, the time inconstancy should be better justified. Finally, due to the uncertainties and hypotheses made, uncertainties should be quantified (or at least sensitivity tests could be performed) and the method limitations discussed.

3. This study also evaluates the impact of MD depositions. However, almost no information is provided, only results are presented. Thus, I recommend the authors to revise the introduction, in providing additional information about the MD impact. Data used to compute the MD depositions should be clearly mentioned, and the method used should be clarified. The title could be modified by adding the effect of MD.

Besides, it seems that the same method used for BC depositions is used to estimate the MD dry and wet deposition. But is it realistic? The method used for BC repose on hypothesis on temporal deposition from available measurements. Are these kinds of measurements available to reconstruct the timing of MD disposition? If not, how was it estimated? From the same temporal repartition as for BC? In this case, the method would be very questionable as MD are known to be much more episodic than BC (e.g. Kaspari et al., 2014). Please provide more information on the method and make sure the MD deposition reconstruction is realistic.

Kaspari et al., (2014) Seasonal and elevational variations of black carbon and dust in snow and ice in the solu-khumbu, nepal and estimated radiativeforcings. Atmospheric Chemistry and Physics 14, 8089–8103

4. Results presented show a significant underestimation of the mass balance related to the precipitation uncertainties. Why precipitation hasn't been corrected as available data (e.g. MB measurements, SR50) would allow this correction? Or applying a correction related to the wind (e.g. MacDonald and Pomeroy 2007), to better fit with observations?

This uncertainty could have important consequences on the conclusion of this study due to the significant sensitivity of the annual mass balance to the accumulation. Also, as the wet deposition is estimated by the P quantity, an underestimation of P leads to an underestimation of LAPs depositions.

MacDonald and Pomeroy (2007) Gauge undercatch of two common snowfall gauges in a prairie environment. Proceedings of the 64th Eastern Snow Conference, vol. 29, pp. 119–126.

5. The method section is unclear (please refer to my specific comments for more details) and some method information is in the results or the discussion section. In addition, some data are not presented (see specific comments). This makes difficult to understand some points of the study. Please provide a careful revision of these two sections.

MINOR COMMENTS

Abstract section

The abstract should be revised after considering all the comments.

Introduction section

1. As this paper is also evaluating the impact of MD, source and previous studies regarding the impact should be mentioned (see major comment).
2. References mentioned in the introduction seem sometimes randomly chosen. I encourage the author to revise the literature cited in the introduction. When the citation is an example, don't forget to provide 'e.g.'

Study site and data section

L74: "Study Site and Data" => Study site and data.

L78: Is the 2012 ELA mentioned? In this case why not a more recent ELA indicated?

L83: Provide the total time period of the recorded data used in this study as well as the time step (i.e. hourly?).

L84-85: What does "initiate" means? "Calibrate"?

Why was this period chosen? Do you mean that there is a period used to calibrate the model and another period to evaluate the model? Please clarify.

L85-88: This was done in the study performed by Chen et al., 2018 or is it a new quality controlled performed for this study? Please clarify. If a new method is done for the data check, please provide more details: time period of gap filled, method of gap filled, filters used, number of values removed, etc...

L90-91: provide the exact date of the snow pit. Is the second one in the ablation zone?

As 2 snow-pits are done, in ice and snow, provide 2 different names to avoid confusion further in the paper.

L92: "...interior concentrations were..." => "...interior BC concentrations were..."

Please briefly mention the method used for snow and ice BC concentration measurements.

Fig 1: The location map panel is at low resolution. Please precise the name of the area (HMA?). A specific map of the glacier with elevation bands, as well as the exact locations of the measurements (i.e. snow pits, AWS, MB measurements etc.) in addition to the photo could help.

Caption: more details are expected in this legend: for instance the exact location of the map and where this map is from. Provide also the copyrights and the date of the picture as well as a small description of each one.

Method section

L103-104: Is it the BC concentration measured at the snow pit in the accum. zone? Is this measurement used to assess the BC concentration in precipitation? What is "fresh snow" and how it is determined? How long after the snowfall the measurements are done? Over which depth? Please clarify the method.

Also if this estimation is based on one measurement, it is a strong hypothesis. Therefore, please expose the limitations and quantify the uncertainties of this method.

L105-106: This is also a strong assumption, which would deserve to be at least discussed.

L107-112: How is the melted snow concentration measured? Is there confusion between melted snow and dry deposition?

The method used to estimate dry deposition is very unclear. Please provide more details (a scheme could help). It also relies on many hypotheses such as considering a constant deposition.

The limitations of this approach should be mentioned somewhere and uncertainties should be quantified.

L110-112 and L130-131: This is a very strong assumption as the date doesn't match (2010 vs 2016) (see major comment). Please justify the plausibility of this hypothesis, discuss the limitation and quantify the uncertainty.

L112-115: Where are MD measurements from? Data should be presented in the data section. Using the same method as for BC is very discussable (see my main comment).

L116: Is it the snow pit located in the accum. area? Provide a name to avoid confusion (i.e. see my comment L90-91).

L117: "concentration of BC (1746 ng g⁻¹)"=> add the ref to Fig 2a here.

L117-118: "the layer was formed by the intense melting": how is it known?

L118-119: Sentence grammatically incorrect.

L119-120: Is the "total" refers to the cumulated BC measured in the snow pit from 0 to 105 cm?

L122: How the total P is estimated?

L123-129: The method described here is unclear, please clarify.

L139: Is it only the snowpack or it considers the snow+ice layers? A scheme could help to understand how the model is built.

L140: what is a “recent” snowfall? Is there a threshold for time or depth to be able to qualify a layer as ‘recent’

L141 and L152: the water content is estimated following which method?

L142: "recorded" => do you mean simulated?

L148: Does the decreasing of the second layer occur once the first has been removed by melt? Please be more precise.

L150: Is the LAPs increasing related to concentration in the melt layer that "goes" to the underlayer? And then LAPs stay at the surface or are homogeneously mixed in the layer. This is unclear.

L151: Is this ice layer under the snowpack?

L152: Which snow layer is lower than 2 cm? A scheme would really help to understand this model.

L161: The choice of 20% is unclear. Are they 20% of the total BC or of BC lower than 0.22 micrometers?

You know that the proportion of BC lower than 0.2 micrometers is about 50% (as it is centered). And you know from a previous study that 20% of BC lower than 0.2 micrometers is washed out. In your study, it would thus correspond to 10% of the total BC. So why 20% instead of 10% is chosen?

L162-163: As previously mentioned the MD data should be presented in the data section.

L164-165: This is unclear. It seems that your model considers daily BC deposition. So, when there is no atmospheric deposition? In addition, the study performed by Goelles and Boggild indicates that the surface is enriched by LAPs due to both atmospheric deposition and ice melt. Please clarify the method used here.

L168-170: This is based on strong hypotheses. First, it implies considering that BC record at one point performed in 2016 is representative, spatially and temporally. Second, it implies that the limited depth measurements are homogeneous over the entire ice layer and that they correspond to the min measured. Why were such hypotheses done? Are they based on previous studies? Please better justify this choice, and quantify the associated uncertainties.

L184: How is it estimated exactly, with a subsurface temperature gradient? Where are these measurements from? Temperature data should be presented in the data section.

L189-190: Do you mean one point per elevation band? How many points in total? Over which period is done the simulation? More information about the simulation descriptions should be provided, including also a careful description of the different runs presented in the results.

L192: only one AWS is presented in the data. Where is the second one? It provides data over which period? This should be presented in the data section.

Also, is the lapse rate from the study performed by Chen et al., or was it computed for this study? Over which period? More precisions are needed here.

L199: In comparison to the work performed by Gardner and Sharp, here is chosen not to consider the cloud optical depth. This choice should be explained here.

L205-207: Please refer to previous work using this method (e.g. Gabbi et al., 2015).

L207-208: Uncertainties?

L228: How the initial SSA is known? This is unclear.

Table 1: What is the roughness length of snow?

What are the min/max values for the albedo?

Results

L239-241: “We assume... August 31” this is a very unclear sentence. Please clarify.

L241: “May 1 to August 31” of which year?

L241-243: Which “procedure”? This is very unclear. This method should be presented in the method section and should be clarified.

L244-247: Over which period and where (i.e. in the accum. zone, ablation zone) are performed these statistics?

L245: Bias would be a good indication as it allows to evaluate systematic errors in the model.

L247-249: This means that it has been calibrated at one point (the AWS)? In the accum. area? Over snow? And then applied everywhere? Due to the significant temperature gradient with elevation isn't it a strong hypothesis? This should be better justified and uncertainties and limitations should be at least mentioned.

L253: “Snow... sensor” Over which period? These data should be presented in the data section.

L253 and Fig 4: the mass balance data are not presented.

L253 and 256-257: These results highlight an underestimation of the precipitation. Why P hasn't been corrected? Please refer to my main comment.

L255: where is this density value from? Measurements, literature, estimation? What is the associated uncertainty?

L258: Add bias as score.

L258-259: “reasonable agreement” is qualitative. Avoid qualitative information and keep quantitative information.

L260: "The stand_run" should be introduced and described in the method section. Here only results should be provided.
Fig. 3: The simulations have been done over the 2011/2012 year but is never mentioned before (especially in the method section). Please clarify the method that led to these simulations.

a) measured albedo at the AWS in the accumulation area? Please precise.

b) "accumulated" => cumulated. Where are measurements from? Stakes? Should be described in the data. Is this over 2011/2012?

Fig 4. a) Why not correcting the mass balance, especially the accumulation(i.e. the precipitation)? (see mail comment)

b) Where are these measured mass balance data from?

The caption is wrong (it is the one for Fig. 3).

L268: simulated LAPs concentration? At the surface? What is "surface"? (i.e. the first x cm?)

L268: "negatively correlated" is this score shown somewhere? Or do you mean that the snow height decreases when the surface BC concentration increase?

L269: I don't understand this statement. Is it not because when BC increases, this induces more melt, and the snow height decreases?

Fig. 5: it is surprising how the MD deposition fits well with the snow height, as we expect the opposite (i.e more MD=higher melt). Is there a hypothesis or explanation for that?

Here, the dependence on elevation is interesting and could be further discussed (e.g Dumont et al. 2020)

Dumont, M. et al. (2020) Accelerated snow melt in the Russian Caucasus mountains after the Saharan dust outbreak in March 2018. *Journal of Geophysical Research: Earth Surface*

Why is the choice to relate LAPs deposition to snow height done here? Due to the link to the snow melt? In this case, it should be associated with results in section 4.4?

L287-289: this is a method description.

L292 "was more obvious": please provide quantitative information

L294 "much less" same remark

295: "more obvious"... "less obvious" same remark

Fig. 6: a shaded area could indicate ice and snow surfaces.

Caption: add "simulated" before "average albedo"

L297-298: glacier wide albedo mix snow and ice albedo. The impact could be given considering separately the surface type.

Table 2: given here separately ice and snow albedo variations, to better link it to the melt.

L319: is it 14.6% for both annual and melt seasons?

L320-322: This information should be in the discussion.

319: Here, more than 50% is a very high and surprising value. What are the associated uncertainties? How do you explain that this effect is more than 3 times higher than the BC alone and MD alone? It is known from the literature that BC and MD do not have a cumulative effect. Here this value suggests more than the cumulative effect and is really difficult to understand. This result should be better explained and discussed.

L328-229: This is a discussion

L328-329: This is contradictory. With a higher concentration, we expect a higher effect on the melt. Is there an explanation for this? Please better justify this statement.

Fig 7: We expect the same sub-periods than the ones chosen for the Albedo for a better comparison.

Caption: "Accumulated" => Cumulated.

Discussion

347: What are the 3 mechanisms? Please provide a better description.

L348: It is unclear how the BC from melt-out ice is different from atmospheric deposition. Because all the BC in the ice is from, at some point, the atmosphere, no?

L348: What is a "little influence"? Please quantify. Also, is it due to a limited area of ice surface or a limited time that the glacier is icy-surface or probably because of the decrease in albedo? Is it less important over the icy-surface due to the lower albedo of the ice?

Otherwise, results over ice and snow surfaces could be given separately.

L350: Atmospheric deposition is total BC deposition from dry and wet deposition. This is unclear and should be clarified in the method.

L356: First, I am not convinced that a universal effect can be demonstrated by comparing results at 2 sites. Secondly, this is really surprising regarding the literature and due to the very inhomogeneous BC deposition over the world. References should be added here to better discuss this.

L358: What is exactly melt-out BC? Before presenting these results

the data method should be clarified to understand where this BC is from Isn't all the BC from the atmo at some point? If it is from BC added in the ice layer, this is based on one measurement for one year and should be taken with precaution and uncertainties evaluated.

L360: Is there an explanation of the contradictory results from your study and the one performed by Goelles et al., 2017?

L361: "The concentration... inhomogeneous"? Mentioning that, it makes questionable the method used in your study: i.e. a measurement at one point is used for the entire glacier. This should be further discussed and the associated uncertainties should be quantified (or at least a sensitivity study can be performed).

L363-365: "For example....flat are" add a ref for this statement.L365-366:" the removal...magnitude" add a reference for this statement.

L368: "What does a reasonable model mean? Does this mean that the model used in this study is unable to reproduce the effect of LAPs? Limitations should be mentioned and uncertainties estimated.

L368: "Using a constant...high value". Isn't it the method used in this study? Does this mean that the impact of BC deposition is over-estimated? Here again, the limitations of this study and uncertainties should be mentioned and quantified.

L369-372: "To avoid... 2 cm" This is method information and should be mentioned in the method section.

L373: What is a "reasonable range"? Please quantify.

L372-374: How is this hypothesis validated? Is there a reference for the hypothesis? What is the related uncertainty?

Fig 8: This is an interesting result but it is unclear how is estimated the difference between BC from fossil fuel in comparison to the BC deposition mentioned in this study. In addition, the method to provide this result should be described. Finally, this is, according to me, a result and would deserve to be in the result section.

Table 3 last line: remove 1.5 (that appears twice)

L387: if it is approx, uncertainty should be mentioned.

L389-390: The increase of 4.6 is from which date exactly?

L394, L422, L448-449: How all the BC is attributed to human activities? Is there a reference for that?

L397-398: This decrease is in comparison to what?

L401-403: not right. The effect of the BC trend has been studied, at least over the snow cover (e.g. Réveillet et al., 2021) *Réveillet et al., (2021) Black carbon and dust alter the response of mountain snow cover under climate change. Nat Commun 13, 5279, <https://doi.org/10.1038/s41467-022-32501-y>*

L414: "This study" refer to your study? Why it is under-estimating the effect? Not clear. Is there an uncertainty to evaluate this underestimation?

Conclusion

The conclusion would be probably revised after the revision of the manuscript, especially in providing more quantitative results and adding information about the limitations and uncertainties of this work.

L436-437: "the model...4550 m a.s.l."=> this is not mentioned in the method section. Please add this information and details about this calibration.

L439: not only, but also at the AWS right? Please provide the scores of this evaluation.

L450-451: atmospheric deposition is dry + wet deposition?

L462-463: so why not providing results for snow and ice surfaces separately? This would be valuable information in your study and would allow discussing the uncertainties related to snow and ice surfaces separately.