**Revision of the manuscript <Comprehensive evaluation of black carbon 1 effect on glacier melting on the Laohugou Glacier No. 12, Western Qilian Mountains> by Jizu Chen et al.**

**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).

Answer: We thank the reviewer for the constructive comment.

The main aim of this study is to explore how much on earth effect of BC and increased BC on current glacier melting. For this purpose, we collected data of BC in atmosphere, snow and ice, which could represent current concentration level of BC. We used them in a mass balance year and explored how much on earth effect of increased BC on current glacier melting. Those assumptions used in this study indeed took many

uncertainties to the results. We thank the reviewer again for careful and valuable comments and suggestions. We will provide a profound discussion about uncertainties caused by the model parameters in the revised manuscript.

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.

Answer: We thank the reviewer for the comment.

This are no homochromous measurements of BC in snow pit, surface ice, and atmosphere on the Laohugou No. 12 Glacier, and there are non-existent in any other glaciers as far as I know, because of the difficulty in data acquisition in glacier regions. The aim of this study is to construct a model consisting of deposition, process of post-deposition of LAPs, and access the rough order of LAPs effect on glacier melt. It must be different of LAPs depositions between different years, but we believe that they are in the same order of magnitude, and could support our study. In addition, we will replenish uncertainties analyses according to measured LAPs concentrations in the revised manuscript.

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*

Answer: This study focused primarily on BC by human emission impact on glacier melting, while the MD was only regarded as an essential parameter in the model, just like other parameters, it meets the model requirements if the concentration of MD in a reasonable range. Therefore, the designment of the introduction totally surrounded the issues of BC to underline the theme of the study. We will discuss the resultant uncertainties caused by the treatment for MD.

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.*

Answer: We thank the reviewer for the comment. In this study, measured precipitation was corrected following the method of Yang et al. (1999), we have added this reference in revised manuscript in line 84-85. The modeled snow height was lower than measured in winter at 5040 m in Fig. 4a, we think the most important reason is that, this place is in the firn basin, snowdrift from surrounding hills can replenish snow mass to this place in winter. Besides, we adopted snow density with a value of 0.3 g $cm^{-3}$ to calculate snow height, while the snow was dry and the snow density always was lower than 0.3 g $cm^{-3}$ in winter, which also could cause the underestimation of snow height.

Yang, D., Ishida, S., Goodison, B. E., and Gunther, T.: Bias correction of daily precipitation measurements for Greenland, Journal of Geophysical Research: Atmospheres, 104, 6171-6181, 10.1029/1998jd200110, 1999.

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.

Answer: We thank the reviewer for the comment. According to suggestions in specific comments, we have revised the section of method in the revised manuscript.

**MINOR COMMENTS**

**Abstract section**

The abstract should be revised after considering all the comments.

Answer: We thank the reviewer for the comment. We will re-write our abstract section according to all comments and suggestions from the viewer.

**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).

Answer: We thank the reviewer for the comment.

BC mainly comes from emission by human activity, it can be cut down by emission reduction and then reduce glacier melting. However, MD mainly originates from nature

source. This study focused primarily on BC by human emission impact on glacier melting, while the MD was only regarded as an essential parameter in the model, just like other parameters, it meets the model requirements if the concentration of MD in a reasonable range. Therefore, the designment of the introduction totally surrounded the issues of BC to underline the theme of the study. We want to consult the reviewer, does it reasonable of the designment of introduction?

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.'

Answer: We thank the reviewer for the comment. We have added 'e.g.' to those references.

**Study site and data section**

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

Answer: have been reviesed.

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

Answer: this is the average ELA of LHG Glacier during hydrological year of 2010-2011 and 2011-2012. There were no published ELAs of recent years on the LHG Glacier, measured ELAs were around 4900-5200 in recent years, but those data were not published, so we didn't cite them.

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

Answer: we have added time period and time step in the revised manuscript.

Line 84-85: "Half-hour Data from the AWS acquired between September 2011 and August 2012 was used to initiate the surface energy and mass balance model."

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.

Answer: we thank the reviewer for the comment. This time period is our study period, we have changed "initiate" to "derive".

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...

Answer: this was done in study of Chen et al., 2018, and we have cited the reference in the revised manuscript.

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.

Answer: We thank the reviewer for the comment. There was only one snow pit in the accumulation zone (5040 m) and one ice pit in the ablation zone (4550), we have named them as SP (snow pit) and IP (ice pit), respectively. We have changed the sentence to…

Line 89-92, "A snow pit (SP) was dug in the accumulation zone (5040 m a.s.l) of LHG glacier in August 2016, and the BC concentration was measured at 5 cm intervals (Fig. 2a), the analyses could be seen in Li et al. 2019c. Additionally, an ice pit (IP) was also dug in the ablation zone (4550 m a.s.l) in August 2016 (Li et al., 2019b)"

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

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

Answer: We have added method for measuring concentration of BC.

Line 93-94: "…the BC in snow/ice has been measured using a thermal/optical protocol, detailed laboratory analysis could be seen in Li et al. (2019a, b)"

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.

Answer: We thank the reviewer for the comment. We have revised Fig. 1 according to suggestion from the reviewer.

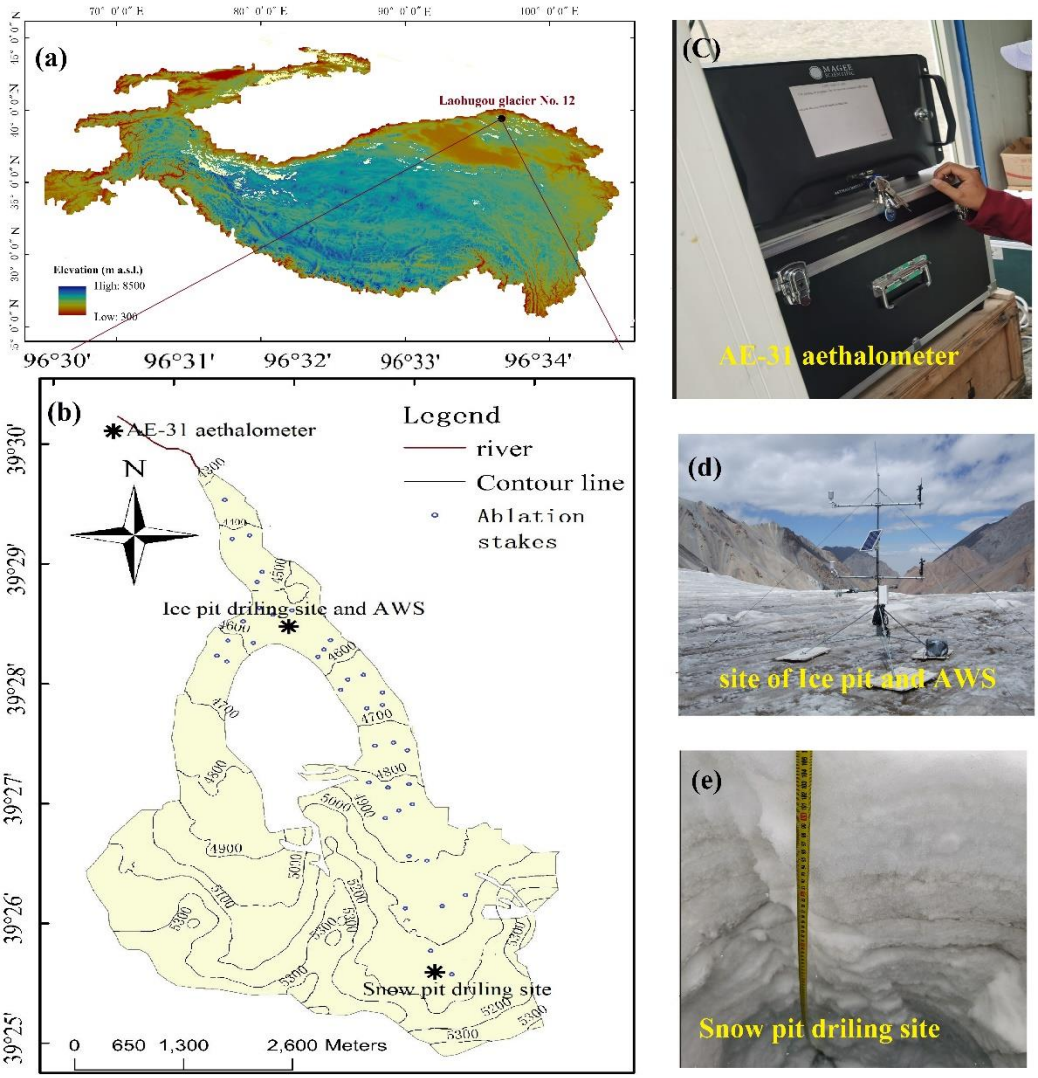


**Fig.1** (a) map of High Mountains Asian. (b) location of Laohugou Glacier No. 12. (c), (d) and (e) are pictures of AW-31 aethalometer, AWS and snow pit, respectively.

**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.

Answer: We used the lowest BC concentration in snow pit to refer to concentration of

precipitation, we have indicated it in revised manuscript. In addition, we have analyzed uncertainty by the concentration in precipitation according to previous studies of concentration of fresh snow.

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

Answer: It has been discussed in uncertainty analyses.

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.

Answer: We thank the reviewer for the comment. The average concentration in snow pit multiplying with removal efficiency was regarded as the concentration of melted snow. We have replenished this to the manuscript. In addition, we will discuss the uncertainties caused by the approach in the manuscript.

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.

Answer: We thank the reviewer for the comment. We will discuss it in uncertainties analyses in the manuscript.

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).

Answer: The measurements of MD were from the snow pit, we have pointed out it in the revised manuscript.

Line 119: "The measurements of MD were also from the snow pit, and the same overall method was adopted for the deposition rate of MD;"

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

Answer: We thank the reviewer for the comment. We have named them SP (snow pit)

and IP (ice pit), respectively.

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

Answer: Have been revised

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

Answer: The layer was judged as formed in the end of summer of the previous year, it is following conventional method of chronological identification of snow pit (ice core). From top to bottom of the snow pit, there were several dirty layers firstly, which were formed by melt events in July and August, and several clean firn layers which were accumulated snowfalls in spring and previous winter. After those layers, there was a very polluted layer at the depth of 65-70 cm, which was distinctly formed in the end of summer of the previous year with intense ablation.

L118-119: Sentence grammatically incorrect.

Answer: We have changed the sentence to "the snow pack above this layer was formed during the hydrological year of 2015/2016."

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

Answer: The "total" refers to accumulated BC during a year. We have changed the sentence to "The total accumulated BC above the extremely dirty layer (5763 ng $cm^{-2}$) was determined according to the BC concentration and density of each snow layer."

L122: How the total P is estimated?

Answer: Precipitation amount was measured by the Geonor T200B gauge, which is an accumulative weighing bucket type precipitation gauge without heating. The total precipitation was the sum of measured precipitation from September 2015 to August 2016.

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

Answer: We thank the reviewer for the comment. We have changed the sentence to..

Line 131-135: The total amount of dry deposited BC (7204 ng $cm^{-1}$) was obtained by that, first the total amount of BC in snow pit firstly subtracted the total BC from the precipitation, then the remaining BC in snow pit added the total amount of lost BC by melted snow (786 ng $cm^{-2}$) according to the amount of melted snow, average BC

concentration, and the removal efficiency.

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

Answer: The model only divides snowpack to three layers, glacier ice is an independent layer. A scheme has been made in revised manuscript.

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

Answer: Recent snowfall means newest snowfall in surface, which has a variable depth.

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

Answer: The water content is a ratio of volume of water to volume of snow layer.

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

Answer: Yes, they are simulated by the model, we have changed “recorded” to “simulated”.

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

Answer: We thank the reviewer for the comment. The first 2 cm layer needs to participate in process of enrichment and removal of LAPs, so the first layer is kept until the second and third layers were completely melted away. We have changed the sentence to…

Line 155-156: “The top layer always kept 2 cm until the total snow dept was less than 2 cm. The second snow layer firstly replenished ablated snow amount of top layer, then third layer.”

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.

Answer: Here is a situation that the entire snow layer was less than 2 cm, when the concentration is very high. With further melting, the snow layer will be less than 2 cm, and the concentration of LAPs in snow layer will dramatically increase. So the surface concentration that used to calculate albedo, was a mixed concentration between snow layer and surface ice under the snow layer.

L151: Is this ice layer under the snowpack?

Answer: Yes, the ice layer means glacier surface ice under the snowpack.

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

Answer: This refers to that the entire snowpack is less than 2 cm. We have changed the sentence to…

"To avoid infinite increase of the LAPs concentration, the LAPs in the snow layer were gradually mixed with LAPs in the ice surface when the total snowpack was less than 2 cm."

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?

Answer: The measurements of removal efficiency were very rare, and hard to be obtained, we can only use the published value in previous researches in the simulation. We will discuss the uncertainties of removal efficiency using values of 10% and 30% in the revised manuscript.

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

Answer: Have been Done.

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.

Answer: We thank the reviewer for the comment. The AE31 obtained the daily atmospheric concentration of BC, but the many days of measurements were lacking. We calculated average monthly concentrations of atmospheric concentration of BC, and distributed them to deposition of every day. When glacier ice was exposed, the source of BC included meltout of englacial LAPs and atmospheric deposition. We have

changed the sentence to…

Line 173-174: "When glacier ice was exposed, we considered the meltout of englacial LAPs besides atmospheric deposition"

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.

Answer: The concentration of BC in englacial ice can be only obtained by the ice pit on the LHG Glacier. We used the minimum concentration in the lowest part of the ice pit, to avoid influence on englacial ice concentration from the LAPs from surface. We will perform uncertainty analyses of LAPs concentration in englacial ice in the revised manuscript according to concentrations of every 5 cm ice pit under surface.

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.

Answer: We have added subsurface temperature.

"$Q_G$ is the subsurface heat flux, which is estimated from the temperature–depth profile, the observed temperature of -4.2 °C at 15 m depth was relatively stable from 1 June to 30 September 2011"

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.

Answer: We thank the reviewer for the comment. We have changed the sentence to…

"The surface energy and mass components were simulated by the model at intervals of 100 m in elevation from 4250 m to 5350 m"

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.

Answer: The lapse rates of temperature and precipitation were determined from two AWSs, one locates in 4550 m and the other one locates in 5040 m. The values of lapse rates of temperature and precipitation were referred to Chen et al. (2017), the detailed description of the two AWSs was presented in this study.

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.

Answer: The cloud optical depth has limited influence on surface albedo compared to solar zenith angle, SSA and concentration of LAPs, in addition, we have no measurements of cloud optical depth on LHG Glacier, therefore we did not consider the influence of cloud optical depth.

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

Answer: We thank the reviewer for the comment. We have cited this study here.

L207-208: Uncertainties?

Answer: The uncertainties of value of MAC will be done in the revised manuscript.

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

Answer: The calculation of SSA is an iterative process, the initial SSA is the SSA of fresh snow which was set a value of 1000.

Table 1: What is the roughness length of snow?

Answer: The fresh snow and firn used a same roughness length. We changed 'firn' to 'snow' in Table 1.

What are the min/max values for the albedo?

Answer: It was calculated according to measured fresh snow albedo.

**Results**

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

Answer: We thank the reviewer for the comment, we have changed the sentence to "The refreezing of meltwater in the snow layer was tuned to the accumulated mass balance during May 1 to August 31, 2012."

L241: "May 1 to August 31" of which year?

Answer: We have added information of year. "May 1 to August 31, 2012."

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

Answer: We thank the reviewer for the comment, we have changed the sentence to “The refreezing rate changed repetitively from 1% to 100% at the site of the AWS until the root mean square error (*RMSE*) between the simulated and measured mass balance was smallest.”

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

Answer: We have added information of period and location before this sentence.

“Fig. 3a shows comparison between simulated and measured mass balance at the site of AWS during May 1 to August 31, 2012.”

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

Answer: We have added bias in the revised manuscript.

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.

Answer: We thank the reviewer for the comment. We have replenished uncertainty analyses for parameters.

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

Answer: Have been done.

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

Answer: The mass balance data shows in Fig. 4a, we added ‘Fig. 4a’ on the bottom of the sentence.

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

Answer: We thank the reviewer for the comment. In this study, measured precipitation was corrected following the method of Yang et al. (1999), we have added this reference

in revised manuscript. The modeled snow height was lower than measured in winter at 5040 m in Fig. 4a, we think the most important reason is that, this place is in the firn basin, snowdrift from surrounding hills can replenish snow mass to this place in winter. Besides, we adopted snow density with a value of 0.3 g $cm^{-3}$ to calculate snow height, while the snow was dry and the snow density always was lower than 0.3 g $cm^{-3}$ in winter, which also could cause the underestimation of snow height.

Yang, D., Ishida, S., Goodison, B. E., and Gunther, T.: Bias correction of daily precipitation measurements for Greenland, Journal of Geophysical Research: Atmospheres, 104, 6171-6181, 10.1029/1998jd200110, 1999.

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

Answer: According to measurements, normally the fresh snow was lower than 0.3 and firn was larger than 0.3, thus we set an intermediate value for snow density. This treatment had limited influence on simulated mass balance, we will discuss uncertainty for it.

L258: Add bias as score.

Answer: Have been done.

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

Answer: We thank the reviewer for the comment. We have changed the sentence to “The *RMSE* was 121 mm w.e. between the simulated and measured annual mass balance at each elevation belt, which was less than 10% of the measured average mass balance (−1218 mm w.e.).”

L260: "The stand_run” should be introduced and described in the method section. Here only results should be provided.

Answer: Have been done.

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?

Answer: We thank the reviewer for the comment. We have specified information of data in the revised manuscript, in addition we replenished measurements of mass balance in the data section. We have changed all of "accumulated" to “cumulated”.

“Successive mass balance measurements were carried out on Laohugou No.12 Glacier during 2010-2012. Thirty plastic ablation stakes were installed on the glacier (Fig. 1b). The ice height changes at each stake were measured in September of every year to derive the annual mass balances, and in addition, during the ablation period (May - September), ablation was measured at some stakes at week-month time intervals. Snow depth and density were also measured when the stakes were covered by snow. Nine ablation stakes were installed near AWS2 and their surface height changes, snow depth and density were recorded from May 1 to Sep 30, 2011 at time intervals of 1-7 days. These measurements were converted to surface mass balance following standard glaciological methods, and used to calibrate model parameters and validate model results.

”

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).

Answer: We used measured mass balance at 4550 m (Fig. 3) to calibrate energy and mass balance model, then used measured mass balance at each elevation belt to validate simulated mass balance. The model underestimated mass balance at 5040 m but in a reasonable range, and simulated mass balances at each elevation belt were well consistent with measured mass balance (Fig. 4b).

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

Answer: The first 2 cm defined as surface layer, which was presented in method section.

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

Answer: We have added the negative coefficient of association in the revised

manuscript.

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

Answer: Here we want to demonstrate that snow layer height increased with occurrence of snowfall, while snowfall covered old surface and turned into new surface, and then the surface concentration of BC became low.

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 heigh done here? Due to the link to the snow melt? In this case, it should be associated with results in section 4.4?

Answer: The figure shows MD is opposite to snow height and fits well with BC, MD is red line, BC is black line and snow height is blue line. We thank the reviewer for the suggestion, we added more discussion about concentration dependence on elevation.

Putting the surface concentration of LAPs and snow height together just want to demonstrate that snowfall can not only increase mass gain but also decrease surface concentration of LAPs and increase surface albedo.

L287-289: this is a method description.

Answer: We have moved it to method.

L292 "was more obvious": please provide quantitative information

Answer: We have added effects of BC and MD in July and August in the revised manuscript.

L294 "much less" same remark

Answer: We have changed it to "far less"

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

Answer: We have changed "more obvious" to "increase", and "less obvious" to

“reduce”.

Fig. 6: a shaded area could indicate ice and snow surfaces. Caption: add "simulated" before “average albedo”

Answer: We thank the reviewer for the comment. Fig. 6 shows averaged influence of BC and MD during a time period, it consisted of snow surface and ice surface when no snow there, therefore it can not mark a line to distinguish ice and snow. We have added “simulated”

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.

Answer: We thank the reviewer for the comment. Here we showed average influence during melt season and the whole year. The model can separate surface types, but we think the separation gives little help to our major results.

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

Answer: Yes, it is for both annual and melt season.

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

Answer: We thank the reviewer for the comment. We have moved them to section of 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.

Answer: The albedo of pure snow and ice is very high, moreover LAPs in snow and ice could greatly reduce the surface albedo. Many previous studies showed LAPs influence with the same order of magnitude. Removal BC and MD together is more than 3 times higher than the BC alone and MD alone, this is because it is an approximate logarithmic relationship between the concentration of LAPs and albedo reduction, i.e., albedo

declines rapidly with increase of LAPs in the case of low concentration of LAPs, whereas it declines slowly with increase of LAPs in the case of high concentration of LAPs.

L328-229: This is a discussion

Answer: We have removed this sentence to 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.

Answer: It is an approximate logarithmic relationship between the concentration of LAPs and albedo reduction, the surface concentration of LAPs is still very high when removal BC or MD alone, therefore albedo slightly reduces compared to albedo including both BC and MD when removal BC or MD alone.

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

Answer: We thank the reviewer for the comment. We have changed the figure to same sub-periods in the revised manuscript.

Caption: “Accumulated” => Cumulated.

Answer: Have been done.

**Discussion**

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

Answer: We thank the reviewer for the comment. We have changed the sentence to “In this study, we identified three mechanisms, namely BC from meltout ice, dry and wet deposition, via which BC affects glacier melting”

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?

Answer: The glacier ice starts to melt when the snow melts always, then the BC in glacier ice will be released and enrich in the surface, it is an important source of BC.

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.

Answer: Table 3 shows each component of BC effect on glacier-wide melting during entire year or melt season. The BC from atmospheric wet deposition only caused 1.4% of glacier melting, thus we demonstrated “little influence” here.

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

Answer: We thank the reviewer for the comment. We have indicated that atmospheric deposition is total BC deposition from dry and wet deposition.

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.

Answer: We thank the reviewer for the comment. We have added more references to discuss deposited BC effect on glacier melting in the revised manuscript.

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.

Answer: Melt out BC means BC in melted ice is released and enriched in surface. We thank the reviewer for the comment. We have replenished data and method to explain them.

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

Answer: The difference comes from design of method. We have set an initial concentration of LAPs in surface ice according to measurement, while Goelles et al. (2017) assumed that the initial BC concentration in surface ice was zero.

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).

Answer: We thank the reviewer for the comment. We will perform uncertainties according to collected ice samples on the LHG Glacier in the manuscript.

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

Answer: Have been done.

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.

Answer: Here what we want to express is that enrichment and removal of LAPs on bare ice surface is complicated and very lack of observations. The preliminary design in this study though achieved enrichment and removal of LAPs on bare ice surface, but there are still many limitations and need further refinement based on more observations. We will perform uncertainties about this in the revised manuscript.

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.

Answer: We thank the reviewer for the comment. We will perform uncertainties about it in the revised manuscript.

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

Answer: Have been done.

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

Answer: It is according to measured concentration of BC on LHG Glacier, if it is beyond an order of magnitude of concentration of measured BC, then it must be unreasonable.

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.

Answer: Deposited BC consists BC from fossil fuel and BC from biomass burning.

Proportion of BC from fossil fuel in deposited BC (67%) is determined according to analyses $C^{14}$ of BC in snow by Li et al. (2016) on LHG Glacier. We have added the information in the revised manuscript.

Li, C., Bosch, C., Kang, S., Andersson, A., Chen, P., Zhang, Q., Cong, Z., Chen, B., Qin, D., and Gustafsson, O.: Sources of black carbon to the Himalayan-Tibetan Plateau glaciers, Nat Commun, 7, 12574, 10.1038/ncomms12574, 2016

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

Answer: Have been done.

l389-390: The increase of 4.6 is from which date exactly?

Answer: We extracted data of annual concentration of BC in an ice core in Eastern Pamirs presented by Wang et al. (2015), we found that the average concentration of BC after the 1990s was 4.6 times higher than that during the early Industrial Revolution.

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

Answer: BC in atmosphere mainly comes from fossil fuel and biomass burning. However, nearly all of BC from fossil fuel comes from emission of human activities

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

Answer: This decrease compared to glacier-wide melting during 2011-2012. Here, we also want to demonstrate that the melt could decrease this magnitude in current only other year.

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*

Answer: We thank the reviewer for the comment. We have cited this reference in our revised manuscript.

**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.

Answer: We thank the reviewer for the comment. According to comments and suggestions, we will provide a new conclusion in revised manuscript.

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.

Answer: Have been done.

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

Answer: We thank the reviewer for the comment. The performance of model was presented in section 4.1.

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

Answer: Yes, atmospheric deposition consists of dry and 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

Answer: We thank the reviewer for the comment. Design for removal and enrichment of BC in bare ice though underestimates BC effect on glacier melting, this provides a general magnitude of increased BC by human activities effect on glacier melting compared to previous studies. This study aims to explore how much on earth effect of BC and increased BC on current glacier melting. However, snow and ice frequently appear alternately on glacier, thus BC effect on snow or ice separately could not reach our aim.