Answers to Referee #1

We thank the referee for their useful comments. We answer all comments point-by-point below each statement in blue font.

Review on the manuscript entitle “Understanding monsoon controls on the energy and mass balance of Himalayan glaciers’

General comments:

Overall manuscript has provided a comprehensive study of the glacier energy and mass balance for seven sites and further generalized for the whole Himalaya. That may be the reason; the title has come up with Himalayan glaciers. However, this study has focused on the only on the seven glaciers and circled around the Nepal Himalaya and Tibetan Himalaya. So Eastern Himalaya is more appropriate.

We thank the referee for acknowledging the comprehensive nature of our study. The reviewer has a good point with regard to terminology. Our intention was not to generalize our results for the whole Himalaya and we are sorry if the title suggests this. We agree with the reviewer and changed the title to “Understanding monsoon controls on the energy and mass balance of glaciers in the Central and Eastern Himalaya”. We think however that it is appropriate to use both Central and Eastern Himalayas, following Bolch et.al (2012), where “Central Himalaya”encompasses mostly the Nepalese Himalayas, and where “Eastern Himalaya” and “Western Himalaya” refer to the regions to the east and west of the Nepalese Himalayas, respectively. By moving “glaciers” in front of “Central and Eastern Himalaya”, we hope to reduce the impression of generalization additionally.

There is several new information which is really valuable for the understating the summer accumulation type glaciers. One of that is: At all sites, ice melt is the dominant mass loss component, accounting for 65.8% (Changri Nup) to 95.4% (Hailuogou,) of the total mass losses.

We thank the reviewer also for appreciating the novelty of our results. Regarding the numbers that the reviewer cites and from which it is evident that ice melt is the dominant mass loss component: this is so as we have only considered the mass losses during the ablation period for which measurements are available (May to October for Changri Nup and mid-May to October for Hailuogou). The numbers for the year-round mass losses would include a greater snowmelt component. We modified the Results section of the manuscript in the way shown below, to make sure this is clearer to the reader (L323-330):

4.1 Modelled mass balance

The ablation season average melt rates vary considerably across sites: the highest value of $42.7\,mm\,d^{-1}$ is reached at the low-lying site with thin debris cover, Hailuogou, and the lowest value of $6\,mm\,d^{-1}$ is evident at Langtang, a site at moderate elevation but with the thickest debris cover out of all study sites (Figure 4). The largest average seasonal mass loss component at all sites is ice melt, with a minimum of 65.8% of the mass losses at Changri Nup (Figure 4c) and up to 95.4% at Hailuogou, (Figure 4g). This is followed by snowmelt, accounting for only 0.1% at 24K (Figure 4e) but as much as 33.1% at Yala (Figure 4c) of the seasonal mass losses. Sublimation from ice and snow represents a very small share of the seasonal mass losses, and ranges from 0.01% (Lirung, Figure 4a) to 1.2% (Changri Nup, Figure 4d). It mostly occurs under dry conditions during pre-monsoon at the highest sites (Changri Nup, Yala).

Few more general comments, in fact it is query to be generalize.

The manuscript only talks about pre-monsoon and monsoon period, what about post-monsoon? Does it differ from pre-monsoon?

We have also analysed the post-monsoon period and compared it to the other two seasons. The main reason why we did not include this analysis in the manuscript is that the AWS data for the post-monsoon were unreliable at the two highest sites (Yala after mid-September and Changri Nup after August), especially with respect to the precipitation and snow depth measurements. We were concerned that this would bias the resulting energy and mass balances. Although we had multi-year timeseries at hand, the chosen years for those two sites contained the most complete records of an ablation season. We have described this for Changri Nup in section *4.1 Modelled mass balance* and will also add this description for Yala. We also felt that the paper already contains extensive results that allow identifying the distinct characteristics of the monsoon. Since Referee 2 noted that the paper is already dense and contains much information, and both figures and text would become overly complex if we also added the post-monsoon (this necessitates two additional comparisons, pre/post and monsoon/post), we decided to refrain from including an analysis on the post-monsoon into the main text, and hope that the reviewer will agree with us.

(ii) There is no discussion about the effect of winter precipitation on the energy and mass balance of the glaciers. Although the manuscript deals with understanding monsoon controls on energy balance and mass balance, but winter precipitation has equal control over the energy and mass balance.

We fully agree with the concerns the referee raises: the timing and quantity of winter and spring snowfalls greatly shapes the **annual** energy and mass balance through the albedo effect. However, while an analysis of the influence of winter precipitation would be a worthwhile analysis, it goes beyond the scope of the present study, which focuses on identifying the influence of monsoonal conditions on the ablation season energy balance.

Section wise comments:

L2: “large temperature amplitudes” make it simpler like large temperature ranges.

This is a good suggestion and we modified the text.

L5-6: This sentence, I would like to see at the end of the introduction, where citation of work may validate it.

We agree that this might not be an appropriate sentence for an abstract. As the last paragraph of the introduction started with a sentence of similar content, we removed it from the abstract, and instead stressed the importance of energy balance studies in a shorter sentence (L3-4). *"Glacier energy and mass balance modelling using in-situ measurements can offer insights into the ways in which surface processes are shaped by climatic regimes".*

L7: 'Himalayas' it is for curiosity on using 'The Himalayas' instead 'The Himalaya'. I am actually not sure which one is better.

We shared the reviewer perplexity here. Looking this up, according to the word origin (Sanskrit, "hima" = snow, "alaya" = abode), the name refers to the mountain range as an "abode of snow". Thus, from the etymological perspective, the singular "Himalaya" is more appropriate. In modern times, it was misinterpreted as referring to the single mountain, hence all the Himalayan mountains together were turned into the plural "Himalayas". In published cryosphere literature, both writings are frequently used, so we decided to use the etymologically more correct way, and switched to "Himalaya" throughout the manuscript. We thank the reviewer for this hint!

L 19: "dirty-ice glaciers", somewhere it was mentioned as thin debris, so does the dirty-ice glaciers are the same ?. If so then thin debris is mostly lies over the patches or around the higher elevation. Whereas, it has mentioned here as dirty-ice glaciers, which what I understand is that the whole glacier has dirty-ice only.

We revisited these definitions and realized that "dirty ice" may after all not be the right term to describe the surface of Hailuogou glacier's ablation zone. According to Fyffe et al. (2020) dirty ice is only "partially debris-covered", "patchy" or "discontinuous". According to our own field-observations, Hailuogou's ablation zone is to a large extent continuously covered with a thin layer of fine clasts and scattered with coarser clasts, which would leave the thin layer visible, and directly influenced by the atmosphere. Co-author Liu Qiao, who has maintained an AWS on Hailuogou between 2008 and 2013, has measured a debris thickness of 1cm at the AWS site. We have therefore decided that using the definition "thin debris" is more appropriate and removed the use of "dirty ice" everywhere. We also revised the description of Hailuogou glacier in L105-113 and removed the mention of dirty ice in the *Introduction* section and a related citation in L55-59.

L 21: (Yang et al., 2017), please check.

We removed this. This is an artefact in our LaTeX code.

L28: "Karakoram, Pamir and Kunlun ranges in the east". I think it should be 'west'.

We corrected this mistake.

L55-57: This has no information except to show that these researchers have published work on debris-covered glaciers.

We respectfully disagree here, because this citation lists all the studies introducing energy balance models for debris-covered glaciers and thus represents the evolution and state of the art of this type of model.

L62-63: In continuous to the pervious comments. Here are some other references having in situ observations on the central Himalayan glaciers with the perspective of debris cover and thickness influences on ice melt (Shah et al., 2019 and Pratap et al., 2015).

Thank you for these suggestions. We already cited Shah et al. (2019), who conclude that debris thickness has a stronger control on glacier melt than elevation. We however missed Pratap et al. (2015) and now also include this study in L54-55.

L73-75: this whole paragraph, I dint see any sense before to define the objectives of this study.

We agree that this part seems disconnected and interrupts the flow of the introduction. It does also not contain essential information for motivating the analysis, so we decided to remove it from the text.

L87: 'glacierised' I generally practice to use 'glacierized' as per Cogley et al., 2011 (glossary of glacier mass balance and related terms).

Both the US American ("glacierized") and British ("glacierised") spellings are accepted and used in the literature. We decided to generally adopt English spelling in the manuscript and thus kept "glacierised".

L92: Table 2 cited before Table 1, check it with journal style.

Thank you for spotting this. We changed the order of these two tables.

L104: This might be the ablation area that has disconnected from the accumulation area. if this is the case then in the Table , the Lirung Glacier's characteristics needs to be revised.

We are not sure we understand the reviewer's comment here and would kindly ask him/her to clarify how we should revise Lirung glacier's characteristics in Table 2. Currently, the table contains the characteristics of both the accumulation area and the (dynamically disconnected) ablation area together, e.g. the sum of the areas of both glacier parts. We now made this clear in the caption:

Table 2. Characteristics of the study sites. Planimetric glacier and debris surface areas, mean elevation, slope and aspect were calculated using the updated Randolph Glacier Inventory 6.0 by Herreid and Pellicciotti (2020) and the USGS GTOPO30 digital elevation model. Slope and aspect are mean values for the whole glacier. MI ('Monsoon-Index') is the mean June-September portion of the ERA5-Land total annual precipitation (1981-2019); For Lirung, where the ablation zone has dynamically disconnected from the accumulation zone, the glacier characteristics represent both zones together.

Figure 2: Caption: "(blue bars)" For me the color is aqua and not blue. "area on the x-axis [km2] and altitude on the y-axis [m.asl]", This information isn't shown in the figure. Area (size) of the glaciers is not clear, therefore additions of a scale bar and direction arrow is required. "Black crosses" this sign need to change as at Yala Glacier it entirely covers the glacier. Make it red dot with AWS on the side as a legend.

We agree that "aqua" is a more suitable name for the colour. We also changed the colour name for the area/bars indicating debris cover to "olive". The glacier area is expressed in 100m elevation bands in the diagram. We agree that it is not easy to judge the glacier size and orientation without a scale bar and direction arrow. We now added these elements to the figure. We also decreased the size of the x-indicators and arrows for better readability and added a legend, but we kept the black color for contrast and style reasons.

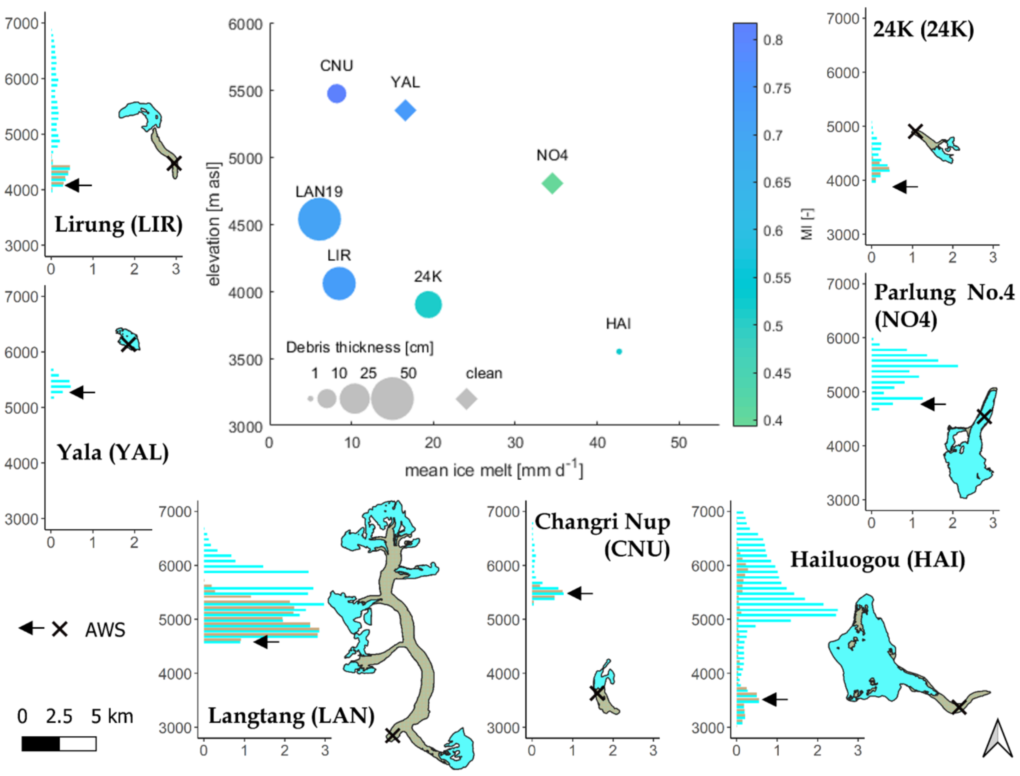


Figure 2 revised

L134: The figure description is not in order.

We will change the sequence of the text, so that the references (a-e) are called in order. Based on comments from Referee #2, we moved this part *"Climatic and meteorological conditions",* including the Figure itself, to the supplementary.

L138: 1st if one consider the lirung and yala glaciers with an elevation difference 1000 m asl in the same catchment, and 2nd by including the fully debris covered ablation area and other clean ice , how it can be justify that the mean monthly 2 m Ta is very similar on the both sites. Though, it is an observation (Fig. 3a) but just to rethink.

We thank the referee for this useful and insightful comment. The referee is very right that the similarity in air temperatures between Lirung and Yala glaciers is unrealistic for precisely the reasons the referee mentions. But please consider that the climatology for each glacier in Figure 3 is taken from one gridcell of 9x9km horizontal resolution of the ERA5-Land reanalysis product and represents average conditions within this gridcell, and not the conditions at the actual elevation of each glacier. We refrained from adjusting the ERA5-Land outputs, as the purpose of this figure is only to show that the study year for each site falls well within the typical interannual range – these data would need careful downscaling to adequately represent on-glacier conditions. We had already acknowledged this circumstance in L130-133 (old version): *"Here, we use the monthly averaged ERA5-Land reanalysis data (Muñoz Sabater, 2019) to provide an overview of the long term climatic patterns, … and evaluate the representativeness of the AWS records in terms of* *seasonal variability ..., while acknowledging that the absolute values from the reanalysis dataset might be biased."*

Motivated by the referee's comment, we reformulated this sentence and added to it a sentence on the representativeness (L115-119). In the new version, we also moved most of the detailed description to the Appendix, based on a comment by referee #2:

We use the monthly averaged ERA5-Land reanalysis data (Muñoz Sabater, 2019) to evaluate the representativeness of the AWS records in terms of seasonal variability (Figures A3 to A9), and to provide an overview of the long term climatic patterns, e.g. the average monsoonal regime from June through September (Figure A1). We thereby focus on the qualitative aspects, given that the absolute values from the reanalysis dataset are not representative for the AWS location at the glacier surfaces. A detailed description is given in the Supplementary.

L192: "surface temperature Ts". Please elaborate that how Ts was calculated?.

The reviewer is right that our formulation was confusing. The calculation of surface temperatures was explained in L176-179 (old version) *"To close the energy balance, a prognostic temperature for the different surface types (T_{sno}, T_{deb}, T_{ice}) is estimated for each computational element. Iterative numerical methods are used to solve the non-linear energy budget equation until convergence for the ice and snow surface, and the heat diffusion equation for the debris surface, while concurrently computing the mass fluxes resulting from snow and ice melt and sublimation."*

We did not inform the reader however that T_{sno}, T_{deb}, T_{ice} are equivalent to T_s in the equations that are not specific to a surface type.

We now added a sentence clarifying the use of the symbols (new version L148-150) :

"In the case of snow, debris and ice surfaces, T_{sno}, T_{deb} or T_{ice} are equivalent to the element's overall surface temperature T_s. In the following, we use the surface type specific symbol for surface specific equations, while we use T_s for equations valid for all three surface types."

3.2 Mass balance in T&C. if it is the same name used before, i would suggest to use T&C model throughout.

We changed to use "the T&C model" everywhere.

L312: delete 'We vary'

Unfortunately, we do not understand why "We vary" would be unnecessary here. As there might be a confusion around the word "vary", in the revised version we use the word "perturb" instead (L295).

L340: choose other word as it was already used with Tibetan plateau.

We tried to find an alternative, but found no other word that would describe our observation as precisely. "plateau" is used as a verb here, while in "Tibetan Plateau" it is used as a noun or name.

Figure 4. Caption and legend.

Measured and Obs., change to single. Black circles seems to be black dot.

Thank you for pointing us at these inconsistencies. We made the changes in the caption and legend.

Figure 5. (i) what is the reason for using different color scheme for same component. I cannot differentiate the ice melt and sublimation for the LIR glaciers. I think use of single color like for LAN glacier would be ok.

This is a good question. We gave each study site its own color signature throughout the manuscript. We were hoping that this would help the reader to intuitively recognize the study site by color in addition to the name. We had the experience in earlier studies, that using only the name of several study sites would sometimes confuse the reader, and the reader would have to spend extra time to repeatedly relate the name to e.g. the geographic location.

We changed the color indicating sublimation to allow for an easier differentiation between sublimation and ice melt.

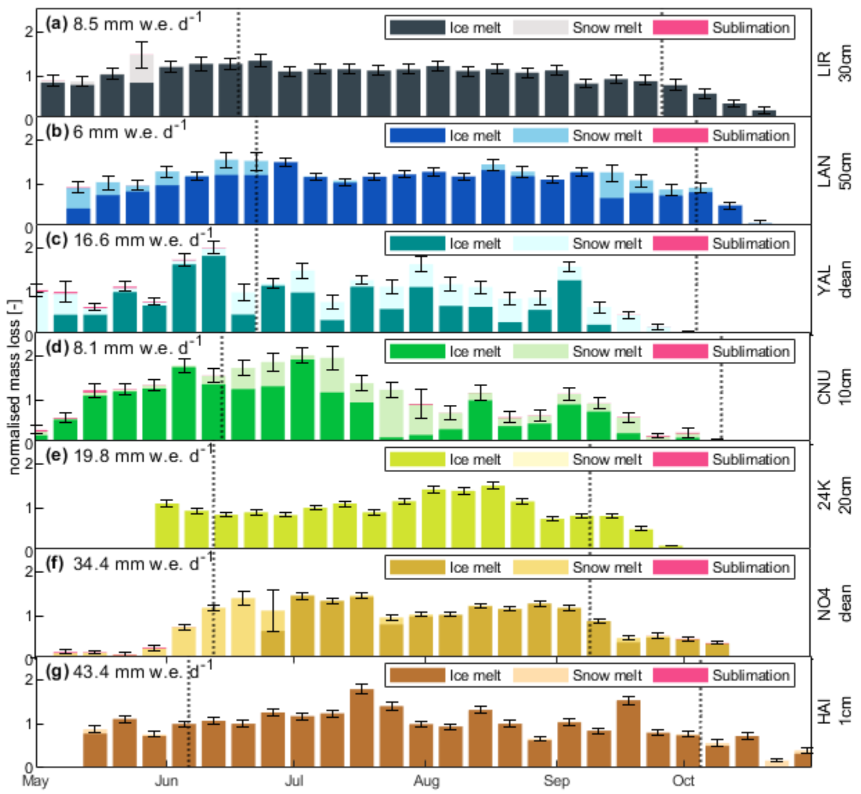


Figure 5 *revised*

L481: "applying a Ta lapse rate of 0.6°C/100m" What about the change of values of other forcing variables with the change in elevation?

This is a very good comment. We considered all possible options for the extrapolations of the meteorological variables. While temperature has a relatively stable elevation lapse rate, which has been investigated and quantified in a number of studies, the other variables are not simple to extrapolate across the glacierised area (as for precipitation or wind speed), or the change over the glacier area was expected to be small (as for incoming shortwave radiation). For the purpose of this sensitivity exercise, we assumed that the strongest changes in meteorological forcing with elevation would be the air temperature, which in turns controls the precipitation partition and the albedo. To reduce the content and complexity of the main manuscript, and in response to comments from Referee#2, we moved the *Section 5.2 Sensitivity of seasonal flux changes to elevation and debris thickness* to the supplementary. We however made this justification clearer in the supplementary (L602-603). We note that this experiment does not affect any of the main paper results, which all derive from simulations forced with unadjusted AWSs data. The experiment goal was to ascertain that the results did not depend on the specific elevation and debris thickness of our AWSs.

L585-89: More things are also to be considered for realistic simulation, for example avalanches, crevasse, blowing snow, water channel, etc.

Yes, we agree with the need for these additional aspects of complexity, many of which are possible in the distributed implementation of T&C. We however removed the section *Future work,* also based on comments by referee #2 in order to cut down on content.

Bolch, T., Kulkarni, A., Kääb, A., Huggel, C., Paul, F., Cogley, J. G., ... & Stoffel, M. (2012). The state and fate of Himalayan glaciers. *Science*, *336*(6079), 310-314.

https://doi.org/10.1126/science.1215828

Answers to Referee #2 on the manuscript *Understanding monsoon controls on the energy and mass balance of Himalayan glaciers.*

We answer all comments point-by-point below each statement in blue font.

This is an interesting concept and topic of research and a number of new analyses are presented in this manuscript. But I find that I am overwhelmed by the lack of synthesis in the analysis and the writing.
It is difficult to tie the in situ weather station and modeling results with the actual conclusions stated. This is partly because the paragraphs seem to jump from one flux to another or from one variable to another from sentence to sentence. I am left wondering if these conclusions are actual supported by the work in the manuscript or are rather just assertions? They might be but the figures, analysis and writing do not clearly support the conclusions in the discussion/ conclusions section.

We thank the referee for their comments. To address the referee's main concern regarding a lack of synthesis, we introduced a number of improvements to both text and figures to explicitly link our results to the interpretations and conclusions. To provide the manuscript a logic thread and focus, we formulated up front (in the revised *Introduction,* L69-71) a clear set of research questions, replacing the more vague "research objectives" of the submitted version.

The new research questions are
1) Which energy and mass fluxes dominate the seasonal mass balance of glaciers in the Central and Eastern Himalaya?
2) How does debris modulate the ablation season energy balance in comparison to clean-ice surfaces?
3) How does the monsoon change the glacier surface energy balance?

We gave the manuscript a new structure and organised both results and discussion around the research questions listed above. The *Discussion*, in particular, is now structured to respond to each of them separately, before opening to a broader Discussion of *Implications for Himalayan glaciers in a changing climate.* The new overall structure of the manuscript is as follows and is explained in details below:

Contents

1 **Introduction** 3

2 **Study sites and data** 5

3 **Methods** 10

3.1 Tethys-Chloris energy balance model . . . 10

3.1.1 Radiative fluxes . . . 11

3.1.2 Incoming energy with precipitation . . . 11

3.1.3 Phase changes in the snow pack . . . 11

3.1.4 Turbulent energy fluxes . . . 12

3.1.5 Ground energy flux . . . 12

3.2 Mass balance in the T&C model . . . 13

3.2.1 Precipitation partition . . . 13

3.2.2 Water content of the snow, ice and debris layers . . . 14

3.2.3 Snow and ice mass balance . . . 14

3.3 Debris parameters . . . 15

3.4 Uncertainty estimation . . . 15

3.5 Model evaluation . . . 16

4 **Results** 17

4.1 Modelled mass balance . . . 17

4.2 Modelled energy balance . . . 17

4.3 Impact of debris cover . . . 18

4.4 Impact of the monsoon . . . 19

4.4.1 Impact of the monsoon on clean-ice sites . . . 20

4.4.2 Impact of the monsoon on thickly debris-covered sites . . . 21

4.4.3 Impact of the monsoon on a thinly debris-covered glacier . . . 22

4.5 Controls on the turbulent fluxes . . . 22

5 **Discussion** 23

5.1 Which mass and energy fluxes determine the seasonal mass balance of glaciers in the Central and Eastern Himalaya? . . . 23

5.2 How does debris cover modulate the energy balance in comparison to clean-ice surfaces? . . . 24

5.3 How does the monsoon change the glacier surface energy balance? . . . 24

5.3.1 Debris-covered glaciers . . . 24

5.3.2 Clean-ice glaciers . . . 25

5.3.3 Glacier with thin debris . . . 25

5.4 Implications for Himalayan glaciers in a changing climate . . . 25

Conclusions 27

Given that this one was the referee's major concern, and to help assess our changes, we provide here a detailed description of the changes introduced per section.

<u>Results:</u>
First, in order to better structure our *Results* and to link them to the revised figures systematically, we adjusted them as follows:

- We maintained the subsections *Modelled mass balance* and *Modelled energy balance.* We shortened these sections to focus them only on results required to answer our research questions, e.g. common energy balance patterns for all sites and the role of snow accumulation
- We moved the model evaluation from the *Results* (it was described originally in the *Modelled mass balance* section) to the *Methods.*
- We introduced two new subsections: *Impact of debris cover* and *Impact of the monsoon,* to separate those aspects, and we split the latter into three subsections, one each for surface type: *Impact of the monsoon on clean-ice sites, Impact of the monsoon on debris covered sites* and *Impact of the monsoon on thin debris covered sites.*
- We also moved some of the content of the section *Turbulent fluxes at debris-covered sites and their controls* to the *Methods* (L198-201), as indeed we described some of the methodology in that section.

We streamlined the text to emphasize the numbers that lead to our interpretations and conclusions. For example, instead of going through each energy flux individually in a systematic but dense manner in the section *Impact of the monsoon*, we now discuss the monsoon impacts in a more integrated way: we start from the change in melt between pre-monsoon and monsoon (Figure 6, Table A2), then present the changes in the radiative budget before addressing the role of the turbulent fluxes and their changes. We link each statement and number to the respective figure and/or table. There might have been some confusion around the direction or sign of the fluxes and their changes. But to improve readability, we switched the sign of the energy residual (melt energy) from negative to positive, which means that a more positive flux increases melt. We also provide a reference for the change values, which were in the previous version presented on their own, in that we now also included the site name and the absolute values for pre-monsoon and monsoon.

To further improve the readability of the *Results*, we adopted a more intuitive language and terminology. For example, instead of using “sources” and “sinks”, we now use “contributing to melt” or “reducing melt”, “glacier-cooling” and similar, in order to reduce confusion around the direction of the fluxes and their changes.

We hope that this new structure and writing style allows us to explicitly draw together the distinct numerical results to answer our research questions in an easily understandable way. As an excerpt from the revised results section *4.4. Impact of the monsoon* (L394-400):

4.4.3 Impact of the monsoon a glacier with thin debris

In contrast to the glaciers with thick debris, during the monsoon, the melt energy M increases considerably at Hailuogou Glacier. Although SW_{net} contributes less energy for melt during the monsoon and LW_{net} remains overall small at this site (Figure 5), M increased by 28.7 (pre: 158.1, mon: 186.8) on average (all values in $W\,m^{-2}$, from Table A2), and mostly during the nights (Figure A11). The increase in melt energy is mostly driven by the turbulent energy fluxes: H increases by 16.6 (pre: 9.1, mon: 25.7) and LE increases by 26.6 (pre: 5.4 mon: 31.6) (Figure 5 and Table A2), with higher increases during the nighttime than during the daytime (Figure A11). While they act to reduce melt at the glaciers with thick debris cover, here the turbulent fluxes drive additional melt during the monsoon.

Discussion:

We restructured the *Discussion* in subsections that answer the new research questions and link the revised sections, figures and tables in the *Results* to the *Discussion* clearly.

As the *Limitations* and *Future work* sections might have distracted from the main outcomes, we removed both sections and moved some key elements of *Limitations (*i.e. on the debris parameters and moisture interception) to the *Methods*. We reduced the *Implications* section in content and renamed it to *Implications for Himalayan glaciers in a changing climate* in order to focus it on the most important messages around this matter. A snippet from the *Discussion* section *5.3. How does the monsoon change the glacier surface energy balance* (L490-501):

5.3.3 Glacier with thin debris

At the site with thin debris, we observe a melt-enhancing effect during monsoon conditions. The dark debris surface absorbs almost 90% of $SW_{\downarrow}$ in the case of Hailuogou (Table A2), and with a short conduction length ($1\,cm$), the energy influx goes almost entirely to melt. Shortwave fluxes reduce during the monsoon, yet melt nonetheless increases, as higher wind speeds enhance turbulence resulting in an increase in H (Section 4.5 and Table A3). Warmer and more humid air increases LE inputs from condensation at the cold surface (Table A3 and Figure A9). Both turbulent fluxes thus become important sources of melt energy (Section 4.4.3). This adds detailed insights to prior observations and modelling inferences that debris around or below the critical thickness causes higher melt rates than at both clean-ice sites and sites with thicker debris cover (Östrem, 1959; Nakawo and Rana, 1999; Reznichenko et al., 2010; Reid and Brock, 2010; Evatt et al., 2015; Fyffe et al., 2020). Artificially applying thick debris to Hailuogou, while acknowledging the limitations of this experiment (Section A1), results in the same change pattern as the one observed on the other debris-covered glaciers: Melt rates remain almost unchanged when going from pre-monsoon to monsoon (Section A1).

Conclusions

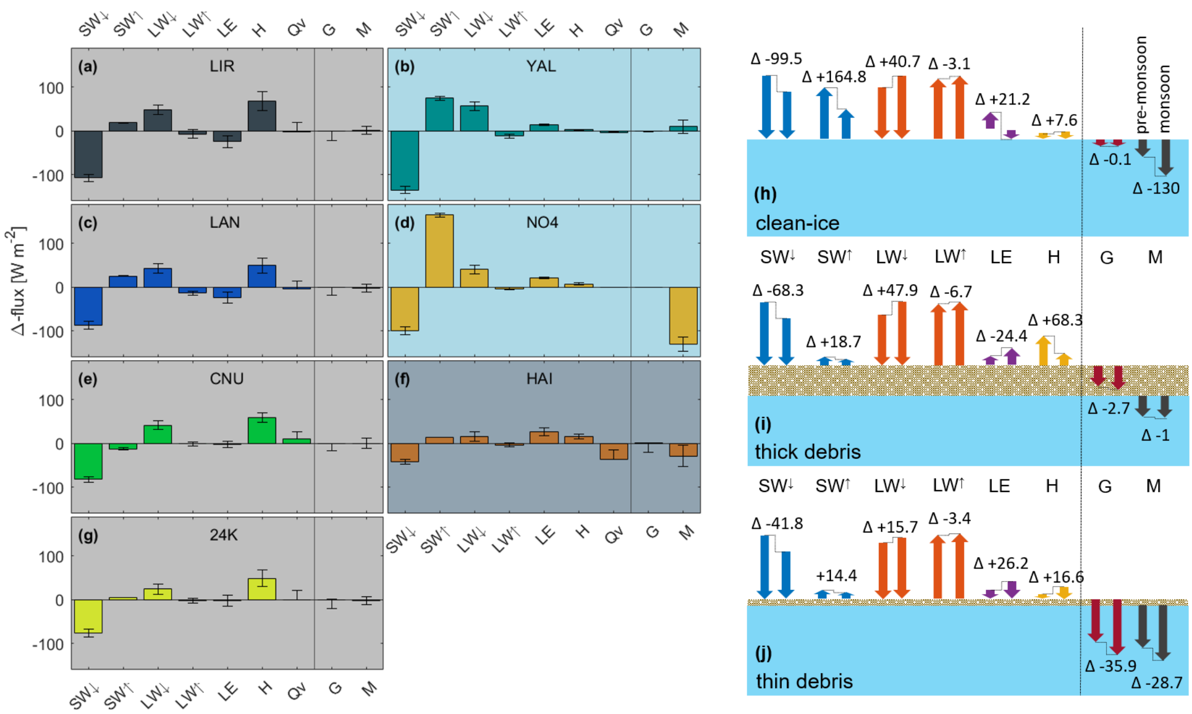
We rewrote the *Conclusion,* which now provides better structured answers to the research questions.

Most of the figures themselves are overwhelmingly complex and the main points are not supported by them. Perhaps the manuscript can be more logically structured and extensive work can be done to give the reader a thread to follow.

To address the referee comment, we have restructured the manuscript in the way described above, revised most of the figures (also based on comments by Referee #1) and introduced important changes to the main figures or removed some of them:

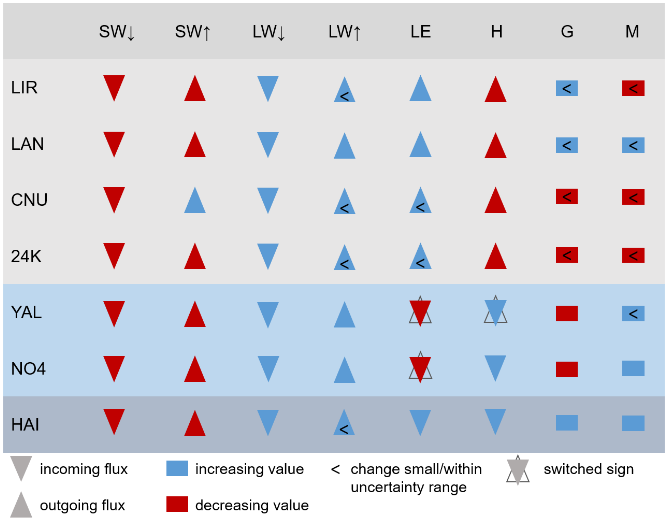
1) After careful consideration of the referee's comments we modified the original Figure 6: we added to the original figure panels (h)-(j) below, which depict the pre-monsoonal and monsoonal fluxes, their actual direction, their magnitude and their

changes from one season to the other with the actual values from one site. The new panels support the interpretation of panels (a)-(g), and should avoid confusion around the direction and magnitude of flux changes. We made a panel for each surface type, and the numbers used are from one site for each of those surface types.



New results figure. (a)-(g) *Differences in energy balance components from pre-monsoon to monsoon at each site including their uncertainties (error bars). The direction of change is to be considered relative to the sign of the original flux (x-axis). For example, a positive change in a negative flux means a reduction in the flux, and can also lead to a change in sign. Background indicates the surface type of the site: grey indicates debris-covered, light blue indicates clean-ice sites, and grey-blue indicates thin-debris.;* ***(h)-(j)*** *Alternative depiction of the changes from (a)-(f), summarizing surface types; Example Δ-flux numbers in [W m^{-2}] refer to (g) Parlung No.4, (h) Lirung and (i) Hailuogou; Numbers for the other glaciers can be looked up in Table 5.*

2) To link the discussion on the *Impacts of the monsoon* to the respective results, we introduced a new figure in the *Discussion.* The idea of this figure is to summarize the flux changes between the different surface types in a visually more straightforward manner.



***New discussion figure**. Triangles pointing down/up indicate a positive/negative flux with regards to our sign-convention, where positive/negative means a flux towards/away from the surface. Red/blue indicate an increasing/decreasing value of the flux when moving from pre-monsoon to monsoon. When signs switch, the underlying, empty triangles indicate the pre-monsoonal direction of the flux, while the overlying, colored ones indicate the monsoonal flux.*

3) We moved Figure 9 with the corresponding text (originally in section 5.2 *Sensitivity of seasonal flux changes to elevation and debris thickness)* to the supplementary material (now Figure A12). These numerical experiments were intended to demonstrate that the seasonal flux changes are robust and do not depend on the actual elevation or debris thickness of the AWSs, but may have interrupted the flow of the *Results* and *Discussion* in the previous version.

To this point I found that the most compelling explanation of the role of differences in local climate came from the ERA-5 output and figure 3. But I must ask: What do the in situ station data tell us that the ERA-5 output do not already inform us about? There is quite a lot of scatter between the in situ site data (the data is from different years, elevations, surfaces, and aspects) unlike the patterns shown in the ERA-5 output.

We thank the reviewer for their perspective. Reanalysis data are extremely useful for many purposes, including catchment-scale hydrological modelling or even for the forcing of glacier-scale energy balance models of large glaciers. Here, we examined the ERA5-Land outputs to put our AWS records (which span only individual years) into their long-term context, as explained later in this answer. In fact, as shown for our study site Langtang, the reanalysis data captures the seasonal cycle of most variables reasonably well (Figure below). However, we are interested in the monsoon impacts on the glacier surface energy balance and in the detailed processes behind them, and we do not think that *the accuracy of the reanalysis data is sufficient to reach our research objectives*.

The figure below makes evident that there are considerable local biases in each meteorological variable at our Langtang glacier site. Indeed, a few °C of air temperature bias (here, 4°C) or different wind speeds (>100% bias) particularly affect, and can even change the direction of the turbulent fluxes, which are key fluxes in the seasonal transition. These biases exist because first, a 9km grid element over high mountain terrain can integrate an altitudinal range of several thousand meters, as well as glaciers, snow cover, vegetation, surface water, and bare rock. Second, there are glacier-atmosphere interactions that create non-average conditions over the glacier surface, e.g. a colder boundary layer and katabatic winds. Those processes cannot be represented in sufficient detail by current climate models and reanalysis products. Third, climate models are known to not perform well in regions with complex topography and where local observations are scarce.

The AWS data, on the other hand, allow us to reproduce the glacier surface energy balance accurately, make inferences about the surface (debris) properties, and evaluate the model performance. The referee makes a valid point that our study site records have different duration and refer to different years, which might complicate their comparison. However, very few on-glacier datasets are available in High Mountain Asia because they are very difficult to collect, and therefore they rarely overlap spatially and temporally. Importantly, the major result of our analyses is that, despite the differences between sites, there are common patterns in the seasonal changes in energy fluxes. To make sure that we do not accidentally compare exceptional years, and draw the wrong conclusions from that, we indeed put our records into the context of average conditions by comparing them to the ERA5-Land data. This showed that the seasonal variability is greater than the interannual variability for all variables and across study sites, and that the years of our records represent typical conditions.

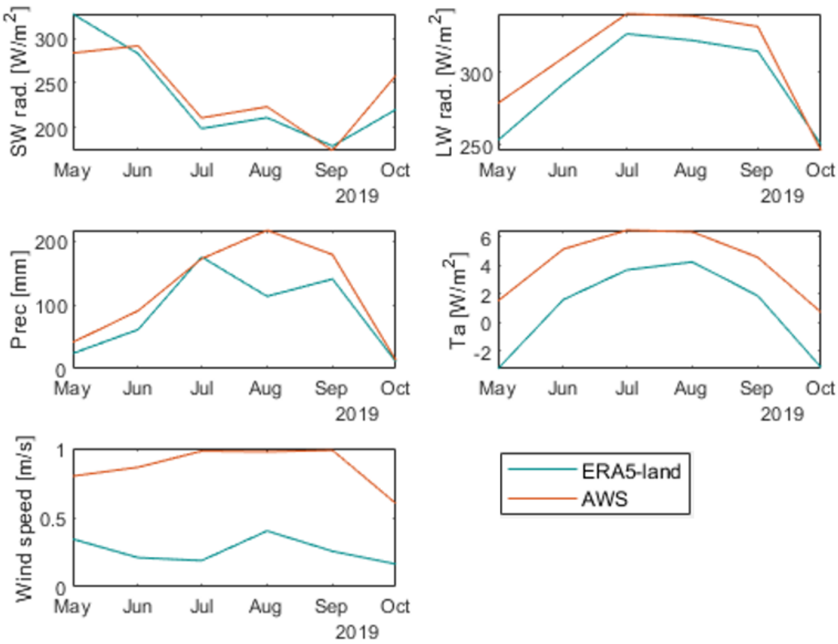


Figure. *Monthly sums (precipitation) and mean (all other variables) of ERA5-Land vs. Langtang on-glacier weather station data;*

In response to the referee's comment, and to avoid ambiguity as to what we use the ERA5-Land data for, we moved most of the description of the reanalysis data, including the corresponding Figure 3, to the supplementary information, and made clear in the main text (L115-119) that we use those data and figure only to show that our selected years are representative of the multi-annual patterns:

We use the monthly averaged ERA5-Land reanalysis data (Muñoz Sabater, 2019) to evaluate the representativeness of the AWS records in terms of seasonal variability (Figures A3 to A9), and to provide an overview of the long term climatic patterns, e.g. the average monsoonal regime from June through September (Figure A1). We thereby focus on the qualitative aspects, given that the absolute values from the reanalysis dataset are not representative for the AWS location at the glacier surfaces. A detailed description is given in the Supplementary.

Perhaps the figures and text can more clearly show the take homes from the station data and support the more general take homes?

To link the key outcomes better to our analysis and figures, we will restructure and modify our manuscript and some of the figures in the ways described above.

My sense is that this could be an interesting, valuable study for TC but as it stands I am not sure if the analyses actually support the conclusions and if using in situ station data is better suited for this question than atmospheric reanalysis output.

We thank the referee again for appreciating the potential value of our study. We tried to respond to the referee's concerns in the best way possible, and will revise the manuscript considerably based on the comments. We will link our conclusions to our results more explicitly in the text, and have made it clear in an answer above why it was necessary to use station data rather than reanalysis data for our study.

More specific comments:
Line 30-32 dates on Mölg should be 2012,2014 and the references should be in order of date in line 33 with the oldest first. Should be corrected throughout.

Thank you for these suggestions, we will revisit this citation and sort citations throughout the manuscript by date.

Line 99. too may uses of 'extensive' in this paragraph.

We fixed this issue and revised a part of this paragraph in order to streamline it (L82-89).

Figure 1. I cannot see the RGI glaciers in panel A. Please change the color of the glaciers. The arrows in panel A seem a bit inaccurate considering that the Indian summer monsoon certainty affects easter Nepal and too the west as well.

We revised this figure based on these suggestions. We gave the RGI glaciers a more visible blue shade. We changed the arrows to represent the influence of the Indian Summer Monsoon more accurately. We also rearranged panels b and c slightly and added a few elements that are missing: North arrow and coordinates with tick marks for panels b and c.

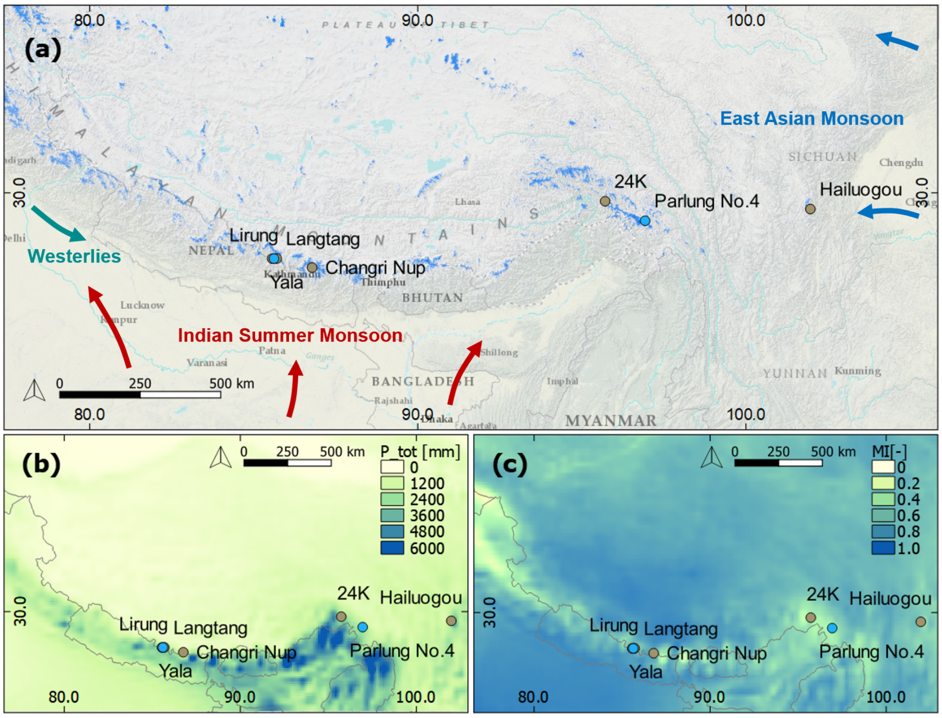


Figure 1 (a) revised glacier color, monsoon influence, guides for glaciers; (b) and (c) scale bars and north arrows;

Tables 5 and 6. Perhaps these should be in the supplement? They are rather overhelming to try to pull anything away from them.

We moved these two tables to the supplementary information

Section 5.1.1 Here many of these points are expected and reproduced by other studies. It seems to me those other studies should be cited here.

This is a good suggestion and we added additional references to L424-439. For example:
The importance of the radiative fluxes and their changes through monsoon were discussed at individual sites in a number of studies (e.g. Kayashta et al., 1999, Aizen et al. 2002, Yang et al., 2011, Mölg et al., 2012). In studies comparing different sites, Zhu et al. (2018) and Bonekamp et al. (2019) identify the timing and quantity of snowfalls as major controls on the glacier mass balance through the albedo effect. Mölg et al. (2012) discuss in particular the role of spring snow accumulation and the importance of monsoon onset timing in controlling the seasonal mass losses. Fujita et al. (2000) highlight the important role of monsoonal summer accumulation, which we called 'ephemeral snow cover from monsoonal precipitation', in protecting the glacier through the albedo effect.

Aizen, V. B., Aizen, E. M., & Nikitin, S. A. (2002). Glacier regime on the northern slope of the Himalaya (Xixibangma glaciers). *Quaternary International*, *97*, 27-39.

Bonekamp, P. N., de Kok, R. J., Collier, E., & Immerzeel, W. W. (2019). Contrasting meteorological drivers of the glacier mass balance between the Karakoram and central Himalaya. *Frontiers in Earth Science*, *7*, 107.

Kayastha, R. B., Ohata, T., & Ageta, Y. (1999). Application of a mass-balance model to a Himalayan glacier. *Journal of Glaciology*, *45*(151), 559-567.

Mölg, T., Maussion, F., Yang, W., & Scherer, D. (2012). The footprint of Asian monsoon dynamics in the mass and energy balance of a Tibetan glacier. *The Cryosphere*, *6*(6), 1445-1461.

Yang, W., Guo, X., Yao, T., Yang, K., Zhao, L., Li, S., & Zhu, M. (2011). Summertime surface energy budget and ablation modeling in the ablation zone of a maritime Tibetan glacier. *Journal of Geophysical Research: Atmospheres*, *116*(D14).

Zhu, M., Yao, T., Yang, W., Xu, B., Wu, G., & Wang, X. (2018). Differences in mass balance behavior for three glaciers from different climatic regions on the Tibetan Plateau. *Climate Dynamics*, *50*(9), 3457-3484.