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 reviewer for acknowledging the comprehensive nature of our study. 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 will change 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 will modify the Results section of the manuscript in the way shown below, to make sure this is clearer to the reader:

The ablation season average melt rates vary considerably across sites, with the highest value of 42.7 $mm\,d^{-1}$ reached at the low-lying thinly debris covered site, Hailuogou, and the lowest value of 6.1 $mm\,d^{-1}$ reached at Langtang, a site at moderate elevation but the thickest debris cover (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), 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%

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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 were unreliable at the two highest sites for the post-monsoon (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. The paper is already dense and contains much information (see e.g. Referee #2 comments) 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 glacier energy balance.

Section wise comments:

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

This is a good suggestion and we will modify 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 will remove it from the abstract, and instead stress the importance of energy balance studies in a shorter sentence. *“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 will switch 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 will remove the use of "dirty ice" everywhere. We will also revise the description of Hailuogou glacier in L120-121 and remove the mention of dirty ice in the *Introduction* section and a related citation in L61.

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

We will remove 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 will correct 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) in L60, who conclude that debris thickness has a stronger control on glacier melt than elevation. We however missed Pratap et al. (2015) and will now also include this study.

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 will keep "glacierised".

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

Thank you for spotting this. We will change 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 1. 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 will make this clear in the caption.

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 of the color. We also changed the color 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.

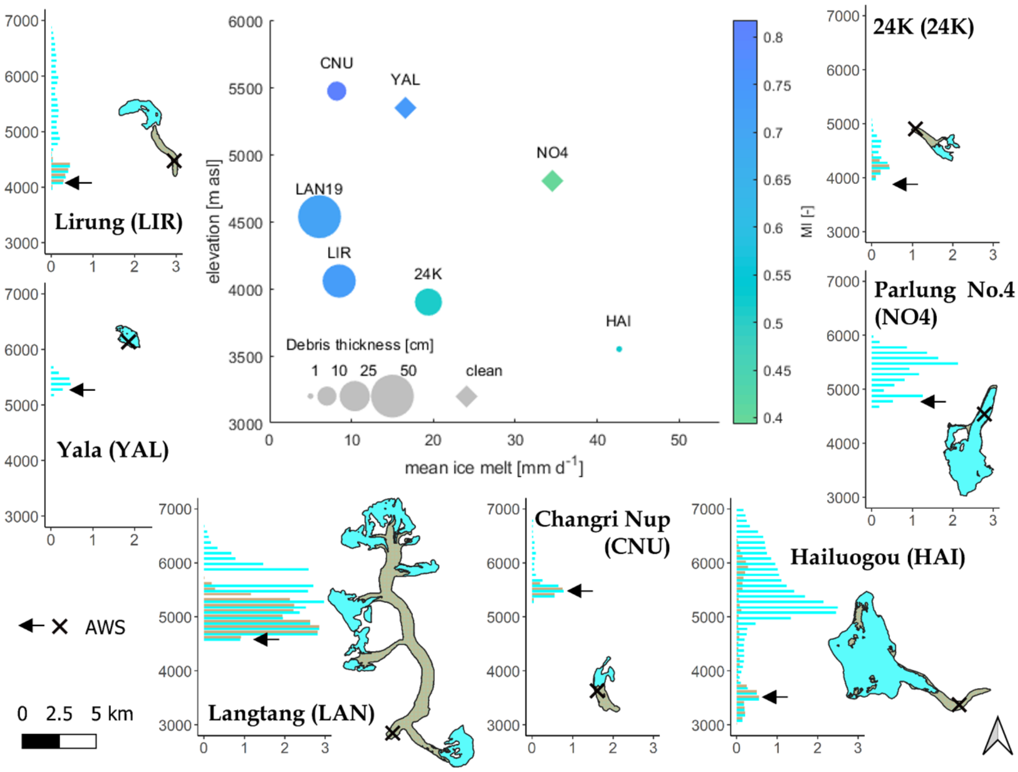

Figure 2 revised

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 will move this part "*Climatic and meteorological conditions*", including Figure 3, 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 will acknowledge this circumstance in L130-133: *"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 your comment, we will add to this sentence "... *and do not represent the conditions at the glacier location*"

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 *"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 will add a sentence clarifying the use of the symbols:

"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 will change 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", we will use the word "perturb" instead.

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 will make 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 will change the color indicating sublimation to allow for an easier differentiation between sublimation and ice melt.

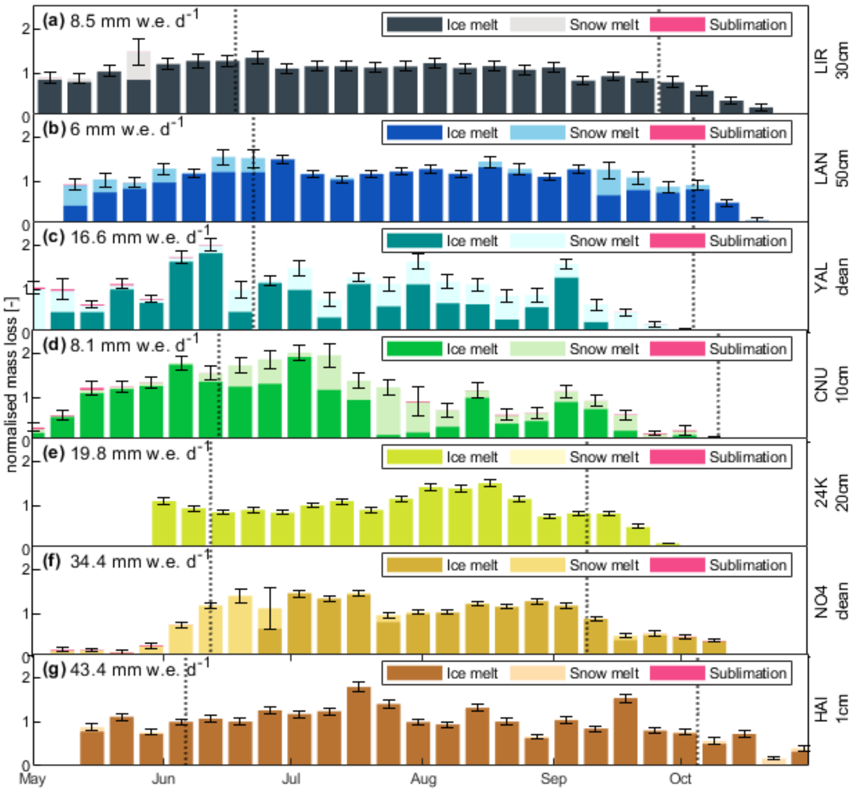

Figure 5 revised

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 will move the *Section 5.2 Sensitivity of seasonal flux changes to elevation and debris thickness* to the supplementary. We will, however, make this justification clearer in the supplementary. 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 will remove this sentence in order to cut down on content. In L579, we already noted that "*the surfaces of glaciers, and especially of debris-covered glaciers, are more complex systems than indicated at the AWS location",* which seems to be sufficient to make this point.

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.

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