Author's response Revision 3

Understanding monsoon controls on the energy and mass balance of glaciers in the Central and Eastern Himalaya

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Response to the editor and general revision

Dear Editor,

We are pleased to resubmit our revised manuscript [tc-2021-97] "*Understanding monsoon controls on the energy and mass balance of glaciers in the Central and Eastern Himalaya"*, which we revised after receiving this second round of reviews.

Reviewer 3 had no major comments, but a number of specific comments, of which some overlapped with Reviewer 4's main comments, which were: (i) That the implications of our results under future climate were overstated while not supported by the analysis, and that those implications should be presented as speculations or hypotheses. (ii) That the role of post-monsoon and winter mass-fluxes should be included in the discussion and more generally (iii) that a discussion on the limitations of our study is required, in particular: the single-season perspective of our analysis, and that all results were derived for the glacier ablation areas only. (iv) That the Appendix needed restructuring and that some content could be re-introduced into the main text of the manuscript. (v) That some elements, such as the regression models used or the calculation of aerodynamic resistances, needed a better description.

We would like to thank Reviewers 3 and 4 for their thorough and constructive comments, which further contributed to improving the manuscript, and which we have addressed below. We summarise here the most important modifications, and then provide a point-by-point answer to the two reviewers comments.

In summary, we have

- revised the text parts around the implications of our results for Himalayan glaciers in a changing climate, in order to not overstate them, and in order to avoid creating the impression that those were our main conclusions.
- added a section on the limitations of our study, where we discuss our focus on individual ablation seasons and explain why we could not expand our perspective to the post-monsoon and winter seasons. In addition to that, we identified key knowledge gaps and gave recommendations for future research.
- replaced the Appendix with a better structured Supplementary material and moved some of the text and figures from the Supplementary into the main text
- revised and chosen carefully our wording when presenting changes in energy fluxes in order to avoid confusion around the direction of fluxes and their changes.
- revisited the regression analysis performed in order to understand the controls on the turbulent fluxes and added to the Supplementary Material more detailed information and figures around this analysis.

- Highlighted the importance of turbulent fluxes on clean-ice glaciers, including their changes in the seasonal transition, while they were mostly discussed for the debris-covered glaciers in the previous version of the manuscript.
- added to the Supplementary Material more detailed information on aerodynamic resistances and aerodynamic roughnesses.
- added to the Supplementary Material a more detailed description of the definition of monsoon onset and cessation.
- recalculated all uncertainties with wider uncertainty ranges on the radiometer measurements.
- added all missing numbers and variable definitions requested.
- implemented minor reformulations throughout the text and adjustments to numbers and figures following the two reviewers comments.

As a result of these changes, we have added to the Results a subsection on the *Sensitivity of seasonal flux changes to elevation and debris thickness* and to the Discussion a new subsection on *Limitations*. In order to reduce the manuscript in volume, we have replaced the former Appendix with a new Supplementary Material, provided in a separate document, which we have structured into 6 thematic sections. We have moved two figures from the Supplementary Material (former Appendix) to the main manuscript and added two new figures and one new table to the Supplementary Material.

We very much hope that the revised manuscript is now appropriate for publication in The Cryosphere.

In response to Reviewer 3

General comments

The authors present a study of surface energy and mass balances during different climatic regimes, the pre-monsoon and the core monsoon season, at different glaciers in the Himalaya. Interestingly, the glaciers represent sites with thick, thin or without debris cover. From observations and modelling at the point scale the authors derive the energy and associated mass fluxes and show, that depending on the surface type, the monsoon influence is either minimal or enhancing melt compared to the pre-monsoon season.

The manuscript is long and dense but the structure is well built and makes it easy to read. The hypotheses are clearly laid out and the methods are well explained. The discussion section brings up the questions asked in the hypotheses and gives concrete answers with a generalized figure (Fig 8) as overview of the regime changes and impacts on different glacier sites. Figures are coherent and help the reader understanding the key points. The substantial supplementary material supports data and results.

Overall, I rate the manuscript ready for publication after clarifying some minor comments.

We would like to thank the reviewer for their detailed review and positive evaluation of our manuscript. We provided reference to line numbers of the revised manuscript, unless otherwise stated. In order to make easier links between related comments within this document, we numbered the comments in the form R3.x (comments of reviewer 3) and R4.x (comments of reviewer 4).

Specific comments referring to manuscript version 3:

R3.1: L 131, Equations 1-3: I'd suggest to generally write the energy balances as addition, i.e. without any minus symbol. All fluxes (except SWnet) can switch sign and the sign they receive in the equation depends on the sign convention used (and explained in L 142). According to this, M must receive a negative sign as it denotes the available energy flux for melt, which removes energy from the surface by phase change. This has implications on the sign of M in several figures and tables.

We agree with this, and have changed the three equations accordingly! *(Eq. 1-3)* We also adjusted the signs of M to negative in all equations, figures and tables.

R3.2: L 140: I suggest a rephrasing. In the given form of Eq 1, M is the available energy for melt, not the net energy input into the snow or ice. We just make it the net "input" by solving the equation for M, but note, the sign must be negative according to convention in L 142.

We agree and rephrased this part according to your suggestion, since M is the net input minus the energy needed to heat the snow and ice pack. *(L 136-137)* For the sign of M, please see above (R3.1).

R3.3: L 177, section 3.1.4: All equations and parameters are well defined. I just miss an explicit explanation of the aerodynamic resistances (rah and raw) and their control of atmospheric stability. This could go into the supplementary material.

In response to both reviewers on this issue (see also R4.12), we have added a Section *S3 Aerodynamic resistance and aerodynamic roughness* to the Supplementary Material, where we describe our implementation of a simplified solution of the Monin-Obukhov similarity theory. The derivations of rah and raw are expressed in the formulae. In addition we have added references to the new supplementary section in Methods section *3.1.4 Turbulent energy fluxes* as follows: *(L 178)*

"The aerodynamic resistance r_ah [s m^-1], (Note: which in our model is equal to r_aw) *is calculated*

using the simplified solution of the Monin-Obukhov similarity theory proposed by (Mascart et al., 1995) and implemented in (Noilhan and Mahfouf, 1996), for details see also supplementary Section S3."

R3.4: L 198: Controls on turbulent fluxes. I'd remove this paragraph here and put it into the supplementary material as well with a bit more information on the regression. For now, this information is a bit lost and becomes relevant in Fig 7a only, where a reference to the supplementary material could be made.

We thank the reviewer for this input. Both reviewers requested more information on the regressions (Reviewer 4's comment R4.29), so we introduced new supplementary figures, visualising the individual scatter plots and regression models, including *adjusted* R^2 values and model formulas in the plots (see Figures R1-3 below). We removed the text from the Methods and added it to the Supplementary Material (Section *S5. Controls on turbulent fluxes).* There, we also added the formulae for the three predictors, the general formulation of the regression models and the timescale used. We added a reference to this section to the caption of Figure 8a. We improved the regression by removing values of $LE = 0\ W\ m^{-2}$, which corresponded to timesteps when the debris water interception storage is empty during evaporation conditions and no latent heat flux is possible. Upon revisiting this part and based on a comment by Reviewer 4 (R4.30), we then also replaced the temperature gradient *delta_T* with the vapour pressure deficit *vpd* as a predictor in the regression analysis for LE.

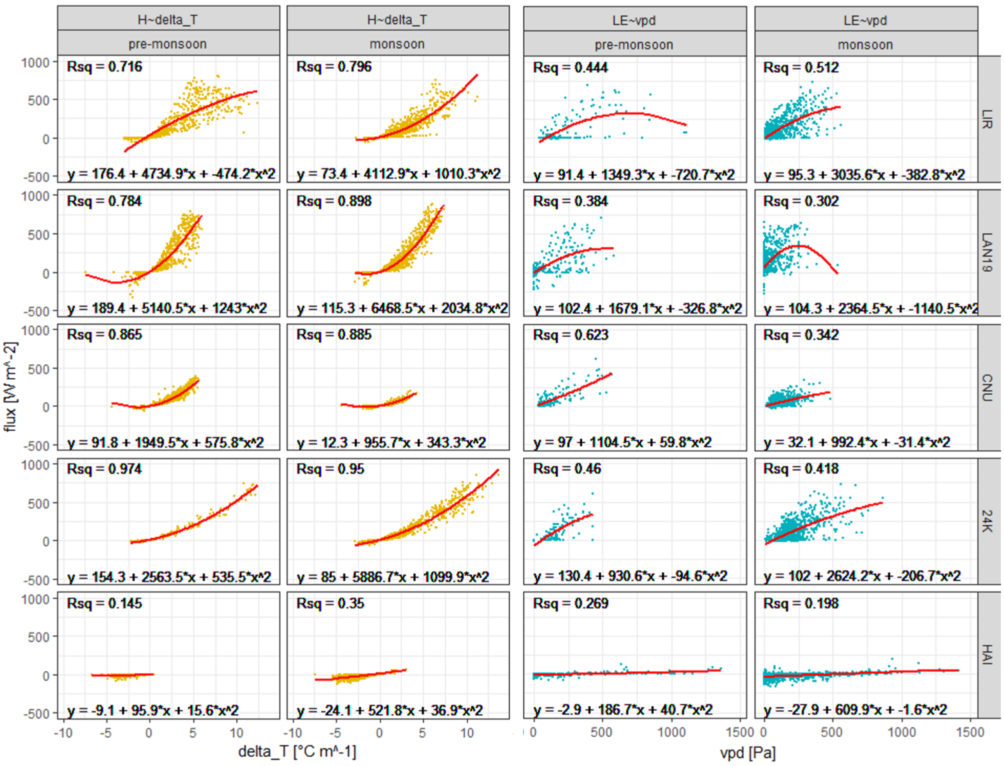


Figure R1: Regression of turbulent fluxes against temperature gradient *delta_T* (H) and vapor pressure deficit *vpd* (LE) for the debris cover sites (Figure S10)

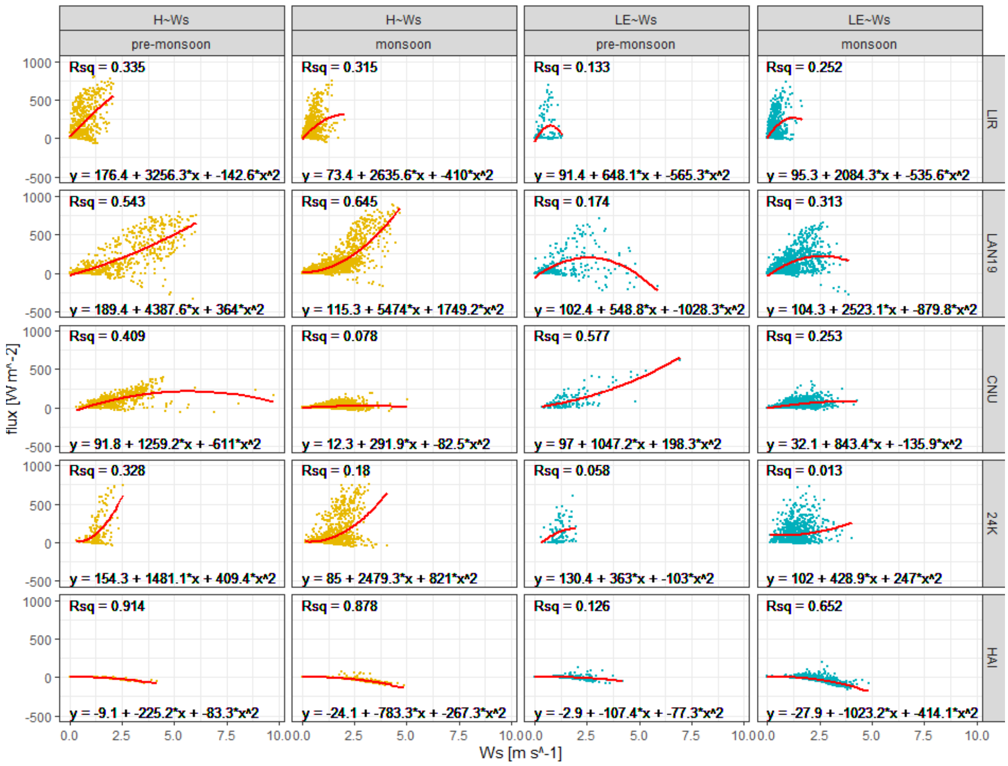


Figure R2: Regression of turbulent fluxes against wind speed *Ws* for the debris cover sites (Figure S11)

R3.5: L 306, section 3.5 Model evaluation: Could you add information of evaluating against surface temperature? A table of RSME would be enough.

We included a table of RMSE in the supplementary. High RMSE at clean-ice glaciers (Yala and Parlung No.4) and snow covered surfaces are a problem of the LW-derived measurements, which give us values higher than 0°C when the ice or snow is at the melting point.

Table S2. RMSE values for modelled vs. measured T_s at all sites. Measured T_s were derived from $LW_\downarrow$ and $LW_\uparrow$ measurements considering the entire modelling period at all sites

		Lirung	Langtang	Yala	Changri Nup	24K	Parlung No.4	Hailuogou
RMSE	[°C]	2.3	2.2	2.99	2.6	1.8	2.89	1.0

We chose not to evaluate model performance against surface temperature, since we used surface temperature derived from incoming and outgoing longwave radiation measurements for optimising roughness lengths on debris-covered glaciers under snow-free conditions. An evaluation against the same measure would create a circularity problem. Instead, performance measures for this optimization were now added (Table 3 below) in response to Reviewer 4 , and clearly labelled as such.

Table 3. Optimum debris parameters k_d and mean absolute error (MAE) from optimisation step 1 (modelled vs. measured melt). z_0m and Nash Sutcliffe Efficiency (NSE) from optimisation step 2 (modelled vs. measured surface temperature)

Glacier		Lirung	Langtang	Changri Nup	24K	Hailuogou
kd	$[W\,m^{-1}\,K^{-1}]$	1.09	1.65	1.77	1.45	0.72
MAE	$[mm\,i.e.d^{-1}]$	5.6	21.6	5.2	1.6	2.2
z0m	[m]	0.7	0.38	0.11	0.15	0.027
NSE	[-]	0.93	0.90	0.64	0.95	0.85

R3.6: L 323: Please correct. In Fig 4 you say the value is 43.4 mmd-1.

We thank the reviewer for spotting this! We have now corrected it. *(L 318)*

R3.7: L 345: Do you mean air or surface temperatures or both?

We have clarified in the text that we were referring to air temperatures.*(L 340)*

R3.8: L 353: There is something missing at the end of the sentence, I think.

Yes, the word 'Figure' was missing, we also added 'despite' to the latter part of the sentence. (L 347-348)

R3.9: L 413: Is it possible to say, whether this wind is katabatic or synoptic?

Katabatic winds are indeed possible and a density driven wind at the base of the steep slope above the Hailuogou AWS is a reasonable assumption. However, we did not add this to our discussion here, since it would require additional observations not currently available and an analysis beyond the scope of our study and not directly relevant for our results.

R3.10: L 542: Does this mean that climate change even enhances the transition from clean ice (or thin debris covered) glaciers to thick debris covered glaciers?

The reviewer poses a very worthwhile, yet complex-to-answer question, since it cannot be answered with energy balance considerations alone, but requires the additional discussion of ice-dynamics and geomorphic effects. This is outside of the scope of our study, but it is partially addressed in other papers (e.g. Herreid and Pellicciotti, 2020; McCarthy et.al, preprint). We expect that decreasing glacier accumulation rates and a resulting slow-down of glaciers worldwide, combined with increased melt out rates of englacial debris and possibly higher debris supply-rates due to enhanced frost-cracking activity and slope failures under climate warming will increase debris-thicknesses of certain glaciers.
Assuming that ice-dynamics remained constant in time, as well as erosion rates and englacial debris concentration and further assuming equality of those properties between the glacier types: Then yes, more melt on clean-ice and thinly debris covered glaciers compared to thickly debris covered glaciers would result in a higher rate of debris melt-out, and thus in a higher rate of debris thickening, for those glaciers in which debris is present.

R3.11: L 602: The definition of the monsoon onset and recession seems not easily reproducible. Could you give more information, e.g. if you have an order of the variables you look at first, or if you define thresholds, etc.

We agree that in some cases where heavy cloud cover and rainy conditions dominate the local weather from spring to autumn (e.g. Hailuogou) the distinction is not easy to make for the human eye (but neither is it for a classification algorithm). We did try automatic classification algorithms (Brunello. et al., 2020; Bombardi 2020), which show good performance overall when dealing with datasets of many years. However, for the analysis of individual seasons, manual classification of the monsoon provided a more reliable distinction.

We have added more detailed information on the procedure we followed to define the monsoon onset and cessation dates and some discussion on the uncertainties emerging from that to the Supplementary Section *S2 Data selection and monsoon definition. (L 835-846)*

"At each site, we define the onset and recession date of monsoon based on visual inspection of the AWS records (Figures S2 to S8) following this procedure:

1. *Inspect SWin to identify a period with cloud overcast sustained for a few weeks and with few interruptions therein, lowering SWin*
2. *Inspect LWin and compare the timing of constantly higher LWin with the above.*
3. *Identify the period of increased rainfall frequency and intensity*
4. *Inspect the relative humidity and precipitation rates to see whether the timing of sustained humid conditions would agree with the above*
5. *Identify a plateau in average air temperature and dampening of the daily air temperature amplitude*
6. *Inspect wind speed to identify a regime change (mean and amplitude)*

This was the general procedure followed, but the order was varied in cases when one or the other variable provided a clearer indication. We note, that in some cases, *where heavy cloud cover and rainy conditions dominate the local weather from spring to autumn (e.g. Hailuogou, 24K) this distinction was less clear than in others, and some uncertainty remains around the exact monsoon onset and cessation dates at those study sites.*

R3.12: Table 1: Please, add the distance between AWS and precipitation measurement site.

We have now added the missing distances to Table 1.

R3.13: Table 2: Glacier elevation: For glaciologists the median elevation would be more interesting than the mean.

Thank you for this suggestion. We changed now to present the median elevation of each glacier, rounded to the nearest 10s of metres.

R3.14: Table 4: Seems like it happened accidentally to be a duplicate of Table 3.

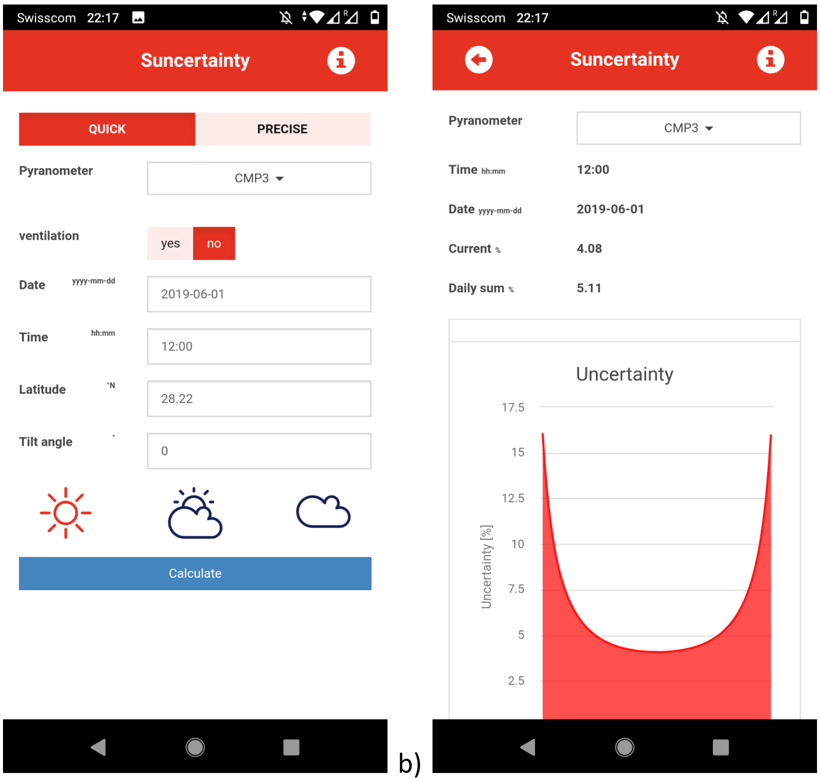
Yes, we removed this duplicate table from the manuscript!

R3.15: Table 5: What is ea? This was not explained before, I think.

We did mention this in the uncertainty estimation part, (L 293) "*...the vapour pressure at reference height ea [Pa]*".

R3.16: What was the reason to choose a 3% range of the radiative fluxes? Wouldn't it make more sense to choose the range from the given sensor error, which might be a bit larger? (I see no need for a repeated analysis, I'm just curious.)

Thank you for noticing this!
The manufacturer of the Kipp and Zonen CNR4 net radiometer, which was used at most of the stations, provided a sensor uncertainty of <5% in the daily totals for the pyranometer (SW in/out) and <10% in the daily totals for the pyrgeometer (LW in/out) at the 95% confidence interval. These values are for a "standard day". Since these values are calculated as the RSS of several sources of uncertainty, which all vary based on the specific installation, latitude, weather and daytime, the manufacturer cannot provide an overall figure for the hourly measurements. Kipp and Zonen however offers a phone app ("Suncertainty") which returns diurnal estimates of uncertainty upon entering data on the above-mentioned installation-criteria. We tested this for the Langtang site, for one day of each sub-period, and three different weather conditions (see Figure R3 (a) input example; (b) output sunny; (c) output cloudy; (d) output heavily clouded). The results show that the daily sum values (<5% and <10%) should be a conservative number on our test days, during the hours with high global radiation. This app unfortunately does not feature the pyrgeometer, but according to a Kipp and Zonen employee, multiplying the pyranometer numbers by 2 should give reasonable estimates for the pyrgeometer.
However, our 3% number, which we seem to have mistakenly read out from the CNR4 manual, happens to be too low. For soundness we decided to recalculate all the uncertainty values with +-5 and +-10% uncertainty ranges for the pyranometer and pyrgeometer, respectively. This left us with somewhat larger uncertainty ranges on our model results, see e.g. Figure 3 and Figure 4. However, these changes to the uncertainty bounds for radiative fluxes do not change the key findings of our work and thus our conclusions are not affected. We also adjusted the uncertainty ranges in Table 4.



a) b)

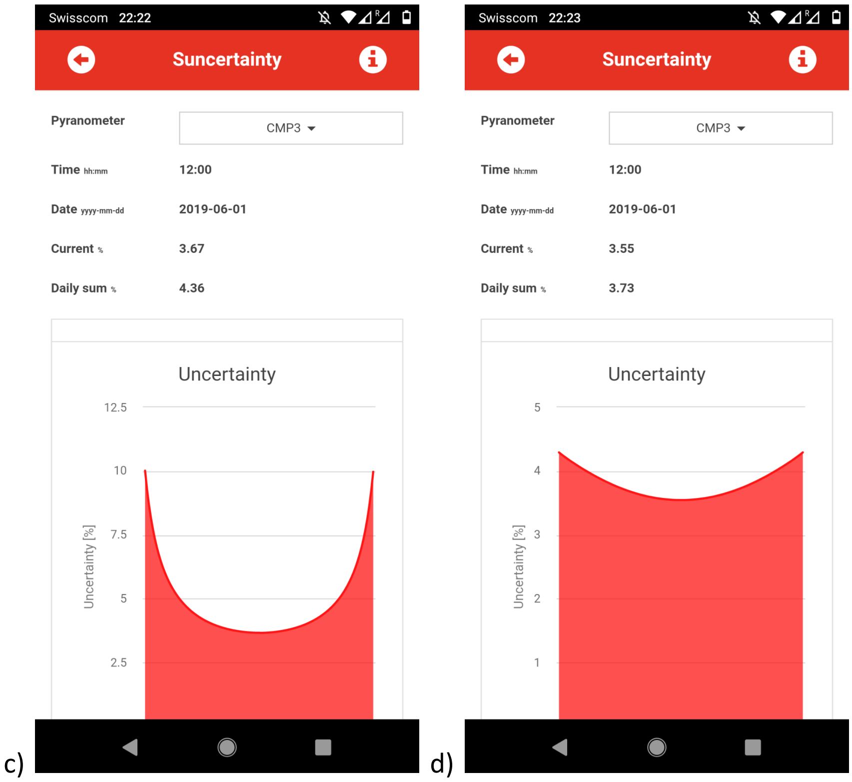


Figure R3: Phone app 'Suncertainty' exemplary outputs for Langtang under three different weather conditions for June 1st, 2019. Unfortunately, the x-axis cannot be properly displayed due to a bug in the app, but it should show the hours of one day (0-24).

R3.17: Figure 7b: Very interesting information. Thank you for showing this.

We are glad that the reviewer liked this figure!

R3.18: Figure 8: Good overview. I just wonder why G and M are not displayed as triangles. They are incoming and outgoing fluxes as well.

The reviewer is right in the case of G, since it represents the energy flux from the surface into the debris- or ice pack. We changed it to triangles. M as the energy residual at the ice surface is only indirectly a result of the surface energy balance on the debris-covered glaciers, via G, so we decided to make its appearance more distinct and maintain it this way (Figure 10).

In response to Reviewer 4:

General comments

This manuscript presents measurements and modelling of surface energy and mass balance at 7 sites throughout the central and eastern Himalaya with a view to assessing how debris cover modifies the effect of the monsoon on glacier fluxes. On-glacier automatic weather station records from the ablation zone of each glacier and a land surface model are used to derive energy and mass fluxes over 4-6 months. The land surface model includes both snow and ice volumes as well as debris cover of various thickness on 5 glaciers where debris cover is observed. The land surface model is optimised and validated against observed melt and surface temperature. The seasonal variation of energy fluxes is presented and differences between pre monsoon and monsoon periods assessed. The commonalities and differences between sites are discussed, especially the role of debris cover in creating differences. The synthesis of 7 sites from a wide geographic area, and analysis in a common framework are a particular strength of the manuscript.

The paper is generally well written and has follows a logical progression. The methods are appropriate and are mostly well described. The results provide new insights into the processes controlling mass loss across these sites. Most statements in the discussion/conclusion/abstract are supported by the results.

We would like to thank the reviewer for their careful review of our work and for this useful perspective! We respond to the specific comments below providing reference to line numbers of the revised manuscript, unless otherwise stated. In order to make easier links between related comments within this document, we numbered the comments in the form R3.x (comments of reviewer 3) and R4.x (comments of reviewer 4).

R4.1: The main conclusion that is not shown by the current results is the assertions about lower climate sensitivity for debris covered compared to clean ice glaciers (line 14, 531-542, 572). These results are not demonstrated and should be presented as speculations or hypotheses. A formal analysis of climate sensitivity could be made with this dataset and model, and perhaps this is an area for future research.

We regret that we conveyed the wrong impression. While excited by the finding that debris-covered ice seems to show a reduced sensitivity, we did not intend to present it as a firm conclusion, but rather, as the reviewer writes, as a hypothesis for future research. We adjusted the manuscript in order to make this clear, and copied the adjusted text into our answers to the more detailed comments.

1. We removed the sentence *"Based on these results, we expect the mass balance of thick debris-covered glaciers to react less sensitively to projected future monsoon conditions than clean-ice and glaciers with very thin debris"* from the Abstract (L14, old version), without replacing it.
2. We adjusted the text in *5.4 Implications for Himalayan glaciers in a changing climate* (see answer to comment R4.36)
3. We rephrased the part on the climate sensitivity in *6 Conclusions* (see answer to comment R4.41)

R4.2: The authors appear to have addressed most of the reviewer comments, however further discussion of the role of post monsoon and winter precipitation (Reviewer #1, general comments) in controlling mass balance should be included in the discussion, especially if the authors wish to discuss the significance of the present results (monsoonal controls) on future mass balance i.e. to what extent do monsoonal, pre/post monsoonal and winter mass fluxes determine annual mass balance?

We agree with the reviewer that the post-monsoonal and winter mass fluxes play a large role in the annual mass balance, even for our spring-and summer accumulation glaciers. With the datasets at hand, it was not possible to present a year-round analysis of satisfying accuracy and detail. Thus, in our discussion we refrained from making any inferences on the year-round mass balance for our study years or for the future, and focused instead on the pre-monsoon and core monsoon seasons, for which we have enough data of high quality to lend us confidence in our findings. We agree however with the reviewer that this is an interesting topic of study, which should be looked at, but which requires appropriate data and analysis.
In response to this and a later comment on the limitations of our ablation-season perspective (R4.5), we included a short discussion on the importance of post-monsoonal and winter precipitation into the new Section *5.5 Limitations*. *(L 562-566)*

"…Our study has also highlighted knowledge gaps which require further study: First, the influence of spring and monsoonal snow cover (its timing and amount) on the seasonal glacier mass balance is currently difficult to discern due to the paucity of multi-annual data sets in High Mountain Asia. Second, the timing and quantity of post-monsoon and winter precipitation influence the annual mass balance, however, even fewer datasets exist for the winter half-year in HMA, preventing a year-round analysis of similar detail. …"

R4.3: While the authors have reordered the manuscript in response to Reviewer #2, too much material has been placed in the appendix and the appendix needs some more organisation.

We agree with the reviewer and replaced the Appendix with Supplementary Material with a clear structure, featuring the following subsections:

S1. Climatic and meteorological conditions
S2. Data selection and monsoon definition
S3. Aerodynamic resistance and aerodynamic roughness
S4. Extended results
S5. Sensitivity of seasonal flux changes to elevation and debris thickness
S6. Controls on turbulent fluxes

R4.4: Some figures and tables could come into the main body of text (e.g. diurnal patterns), and the section (text, table, figure) on sensitivity to elevation / debris cover thickness. Organising the appendix into sections with related figures/tables together and text to give context would greatly help the reader.

We have followed this suggestion extensively, and refer the reviewer to our reply above and to our reply to their more specific comments below (R4.28, R4.34, R4.42)

Further specific comments are given below, but some main points to address are highlighted here.

R4.5: - Some discussion of the limitations of the study is warranted – this should include the fact that all records cover only one season (how significant is interannual variability?) and that the sites are in ablation areas and that surface energy and mass fluxes (along with their response to the monsoon) will change at higher elevations.

We agree with the reviewer on this point, and introduced a new section *5.5 Limitations* to the Discussion *(L 556-571).* We also made a link here to our criteria for choosing our modelling periods (Section *S2 Data selection and monsoon definition)*

"By applying an energy balance model to seven sites across the Central and Eastern Himalaya, we have identified monsoon effects on the ablation season energy and mass balance of glaciers, common for our studied debris-covered and clean-ice glaciers. A list of criteria used for choosing our modelling periods at each site is given in the Supplementary Material Section S2. Applying these criteria, we chose one summer season record for each site, for which all required variables were available at a high level of data quality. As a result of this selection process, our analysis remained limited to one summer season at each site. Our study has also highlighted knowledge gaps which require further study: First, the influence of spring and monsoonal snow cover (its timing and amount) on the seasonal glacier mass balance is currently difficult to discern due to the paucity of multi-annual data sets in High Mountain Asia. Second, the timing and quantity of post-monsoon and winter precipitation influence the annual mass balance, however, even fewer datasets exist for the winter half-year in HMA, preventing a year-round analysis of similar detail. Third, all our sites are located in glacier ablation areas, and surface and energy mass fluxes will change with elevation. While we have tested how representative our point-scale results are for the entire ablation area of the glaciers considered, the response of glacier accumulation areas to monsoon remains to be investigated. Meteorological data from accumulation areas are scarce, however, limiting our current understanding.
Future work should establish new year-round and multi-year records, including datasets from accumulation areas, in order to extend some of our findings. Future work could also target the spatial distribution of forcing data and parameters necessary to run energy-balance models at the glacier-scale."

The reviewer's comment about accumulation areas is a very good one. We were aware of the limitations of our point study, and for this reason conducted the analysis described in Section *4.6 Sensitivity of seasonal flux changes to elevation and debris thickness*, where we assessed how sensitive our findings are across the ablation zones of each glacier.

R4.6: - The sign convention and terms used for changes in fluxes needs clarifying at times

The reviewer is right and we have clarified this as needed. Please see our point-by-point answers below (R4.10, R4.21, R4.22, R4.24, R4.25, R4.26, R4.35)

R4.7: - The methods and results used to regress the turbulent heat fluxes against meteorological forcing needs more description – it looks nice but is hard to interpret meaning of these results.

We addressed this issue in the more detailed comment below (R4.29)

R4.8: - The importance of turbulent fluxes at clean ice sites is downplayed more than needed - the change in latent heat flux is similar at clean ice and debris cover sites.

Also here, we kindly refer the reviewer to their more specific comments later on (R4.20, R4.33)

R4.9: With some reorganisation, clarification to methods and corrections to text this manuscript will make a valuable contribution to the literature.

Thank you!

Specific comments:
R4.10: 131 – the sign convention in Equations 1-3 does not follow that stated in the text - i.e. "The sign convention is such that fluxes are positive when directed towards the surface (line 143)." Thus, all terms should have a + sign in front of them. The exception is M, which is treated as a positive term throughout the text, so should retain a minus sign or appear on the right hand side of equations 1 and 3.

We changed all the signs to positive ones, except for M, which now has a negative sign throughout the manuscript (text, figures, tables). A similar comment was given by Reviewer 3 (R3.1)

R4.11: Table 2 – columns describing the mean air temperature, wind speed, RH, precipitation etc of the sites would be very useful in understanding differences in SEB components between sites, particularly the turbulent fluxes.

Instead of adding this information to Table 2, we added columns with air temperature *Ta* and mean daily precipitation *Pr_d* to Table S1 where we already tabulated relative humidity *RH*, vapour pressure deficit *vpd,* temperature gradient between surface and air *delta_T,* wind speed *Ws and* the percentage of time when water was intercepted at the debris surface *In* (where applicable).

R4.12: 181 – a brief description of rah, particularly how it relates to deltaT, Ws and z0m, z0h is warranted here given the key role the turbulent fluxes play in the analysis and conclusions.

Following this suggestion and a suggestion by Reviewer 3 (R3.4), we have added a section *S3 Aerodynamic resistance and aerodynamic roughness* to the Supplementary Material, where we describe our implementation of a simplified solution of the Monin-Obukhov similarity theory. The calculation of rah and its relationships to the temperature gradient, wind speed and the aerodynamic roughnesses are expressed in the formulae. In addition we have added references to the new supplementary section in Methods section *3.1.4 Turbulent energy fluxes* as follows: *(L 176-178)*

"The aerodynamic resistance r_ah [s m^-1], is calculated using the simplified solution of the Monin-Obukhov similarity theory proposed by (Mascart et al., 1995) and implemented in (Noilhan and Mahfouf, 1996), for details see also supplementary Section S3."

R4.13: 184 – the assumption of z0h = z0v = 0.1 z0m needs supporting, particularly for large z0m over debris cover where this ratio may become smaller.

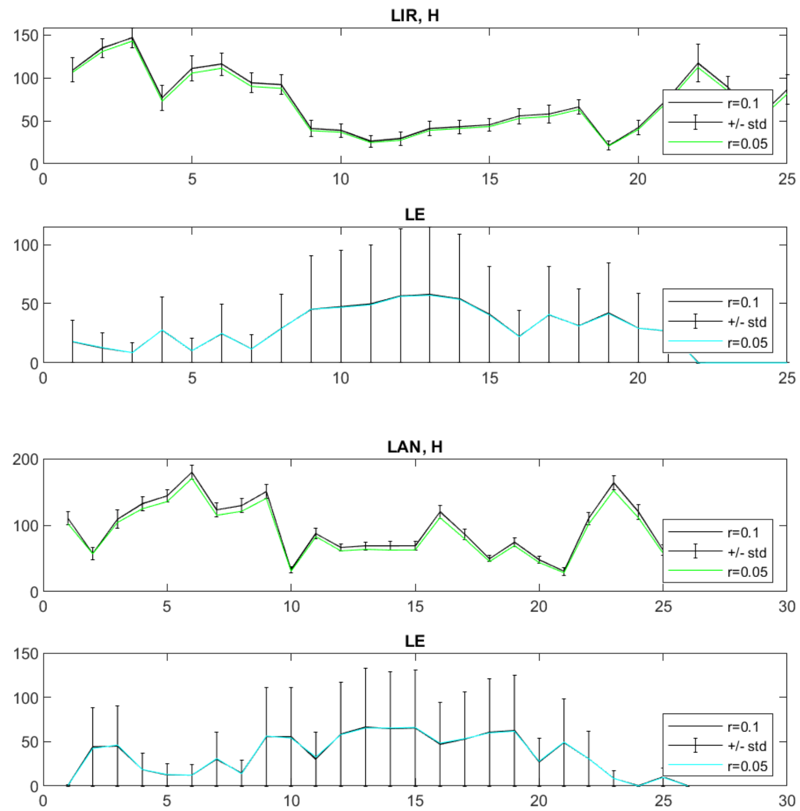
The assumption implemented in our energy balance model (z0h = z0v = 0.1 z0m, i.e. a ratio of r=0.1) is based on Brutsaert (1982) and Mascart et al. (1995) and often used for land surface models (e.g. Noilhan and Mafhouf, 1996). In the literature on energy balance modelling over debris covered glaciers, we found the following three values

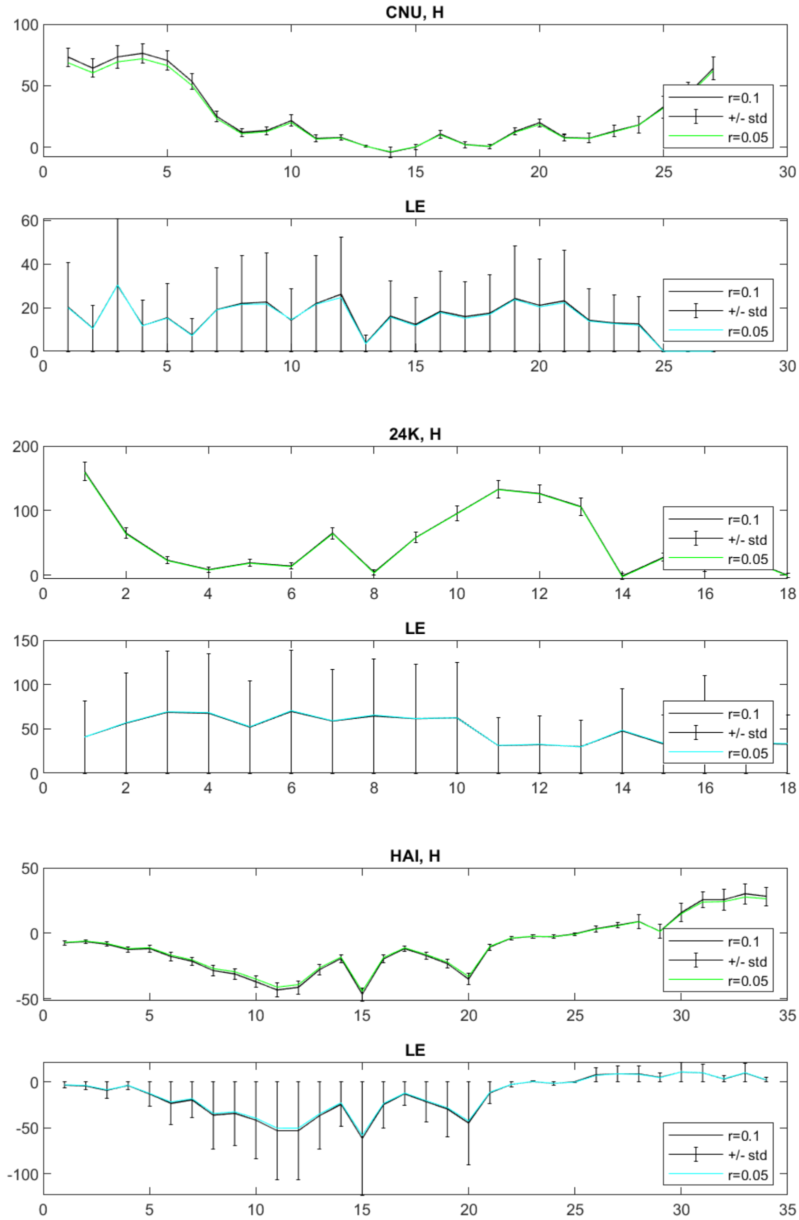
- 1 (e.g. Reid and Brock)
- 0.1 (e.g. Giese et al.,2020), the value we used
- 0.05 (Steiner et.al, 2018), who derived this value from flux tower experiments on Lirung, one of our study sites.

Since we optimised z0m for our sites, it is also dependent on the ratio implemented in our model through its link to z0h and z0v. Hence these three values were effectively optimised together.
To test the sensitivity of the turbulent fluxes to this ratio, we re-ran the optimization procedure with a ratio of 0.05 (Steiner et al., 2018). We then used the new optimum z0m/z0h/z0v to re-calculate the turbulent fluxes. Comparing the resulting H and LE against the ones from the standard run (Figure R4 below), one can see that both fluxes are relatively insensitive to the choice of this ratio.

We added to the supplementary Section *S4 Aerodynamic resistance and aerodynamic roughness: (L 876-884)*

"As a consequence of the assumption explained above (r_ah = r_aw), also the aerodynamic roughnesses of heat and water vapour are used as equal (z_0w = z_0h) and z_0h = z_0w = 0.1 z_0m. For the ratio between the roughness lengths of water vapour, heat and momentum, r=0.1 is a value based on Brutsaert (1982), often implemented in land surface models (e.g. Noilham and Mafhouf, 1996), and is also used in T&C. This ratio remains poorly constrained, not least due to the difficulties in measuring or deriving surface roughnesses (Miles et.al, 2017, Quincey et.al, 2017). Three values have been suggested in the literature: 1 (e.g. Reid and Brock, 2010), 0.1 (Giese et al., 2020) and 0.05 (Steiner et al. 2018), who derived this value for Lirung from flux tower experiments. Since here, z_0h, z_0w and z_0m were effectively optimised together at the debris-covered glaciers, the turbulent fluxes remain insensitive to the choice of this ratio."





R4. Sensible and latent heat flux for debris covered glaciers (weekly averages), optimised and calculated with z0m/z0h/z0v using different ratios (r) between z0w/z0h and z0m. Error bars show the uncertainty (one standard deviation of the updated Monte Carlo runs) associated with the turbulent fluxes from the standard runs.

R4.14: 211 – the discretisation of subsurface layers in snow/debris/ice is ambiguous - please provide a clear description of the number of subsurface layers used to solve the conduction equations.

While we did provide the number of debris layers, we forgot to mention that snow and ice were treated as single-layer systems (thermally active depth of ice set to 2m). We clarified this in the revised manuscript (L 196, L 205-206)

R4.15: 252 – please introduce the term *ln* here (used later but not defined) and provide further details on how the calculation of debris SEB is altered by this term.

We added here a definition of the interception storage *s_In*, explaining how it limits and controls the evaporative flux from debris surfaces and hence *LE*. We also introduce the term *In,* which the reviewer refers to, and which we only use later in *4.5 Controls on the turbulent fluxes.* This is the percentage of time during which *s_In > 0,* i.e. when water is present at the surface. (L 245-249)

"...We assume debris to have a dynamic interception storage s_In, which can hold a maximum of s_In,max = 2 mm water at all debris-covered sites and can be refilled by snowmelt or liquid precipitation. The evaporative flux from the debris is limited by the state of this interception storage and LE can only result from evaporation if s_In > 0. The term In [%] (used in Section 4.5 and Figure 9b) is the percentage of time, during which this condition is met."

R4.16: 290 – please provide NSE (of melt and Ts) for each site for both steps of the optimisation procedure.

We added the Nash Sutcliffe Efficiency (NSE) for the optimization of z0m against surface temperature, as well as mean absolute error (MAE) for the optimization of kd against melt to Table 3.

Table 3. Optimum debris parameters k_d and mean absolute error (MAE) from optimisation step 1 (modelled vs. measured melt). z_0m and Nash Sutcliffe Efficiency (NSE) from optimisation step 2 (modelled vs. measured surface temperature)

Glacier		Lirung	Langtang	Changri Nup	24K	Hailuogou
kd	$[W m^{-1} K^{-1}]$	1.09	1.65	1.77	1.45	0.72
MAE	$[mm i.e. d^{-1}]$	5.6	21.6	5.2	1.6	2.2
z0m	[m]	0.7	0.38	0.11	0.15	0.027
NSE	[-]	0.93	0.90	0.64	0.95	0.85

R4.17: Figure 3 – albedo and precipitation observations indicate the YALA glacier had consistent snow cover in the pre-monsoon period, but this is not shown in the modelled results – was this the case, and if so, how might this discrepancy affect the results for YALA?

Indeed, from the albedo records, we see that Yala has been snow-covered through most of pre-monsoon and repeatedly during monsoon (Figure R5 lowest panel). At this site, the spring-time (May-July) surface-height evolution (as measured by the SR50) was difficult to reproduce with the locally-measured precipitation (Figure R6). Specifically, the albedo measurements suggest snow cover at the weather station but there are few measurements of precipitation. It may be that precipitation is underestimated during this period. However, only by doubling the pre-monsoonal precipitation could we find a satisfying agreement with the snow height and albedo records (Figure R7). However, we did not feel justified making a correction to only part of the record. Given our experience with the Changri Nup station, it is also possible that the disagreement between sensors related primarily to localised snow retention (e.g. a small snowpack near the SR50 and albedometer). For these reasons, and rather than tuning the precipitation inputs, we analysed only the period after June, where the surface height change agrees very well. The slope of the surface height change indicates that mostly ice-melt occurred from June 1 to June 22, which is very well reproduced by our model (Figure R7). This means that the surface must have been intermittently snow-covered at Yala during this period, and that the snow layer must have stayed thin and/or patchy. We always consider the surface energy balance of the actual surface, snow covered or not, hence the effect of snow cover on the energy balance is included in our results. If the measured precipitation is too low, this effect of surface snow might have been underestimated. However, since we trust the agreement between modelled and measured surface height change, we also have confidence in the correctness of the energy balance (with some uncertainty), and we do not think that applying the

above-mentioned precipitation adjustments would have given us more correct estimates for the analysed period.
Indeed, extending the period back into May with the hard-to-justify precipitation corrections would have supported our conclusions better (see comment R4.39), in that it would have resulted in lower melt-rates during pre-monsoon than during monsoon, but our confidence in the energy balance would have been considerably lower. For better visibility of snow cover timing, we removed snow depth from the plots and introduced a binary indicator of snow covered periods to the top of each site's panel (Figure R8). We also slightly adjusted the appearance of other elements in Figure 4 (Figure R8) for better readability.

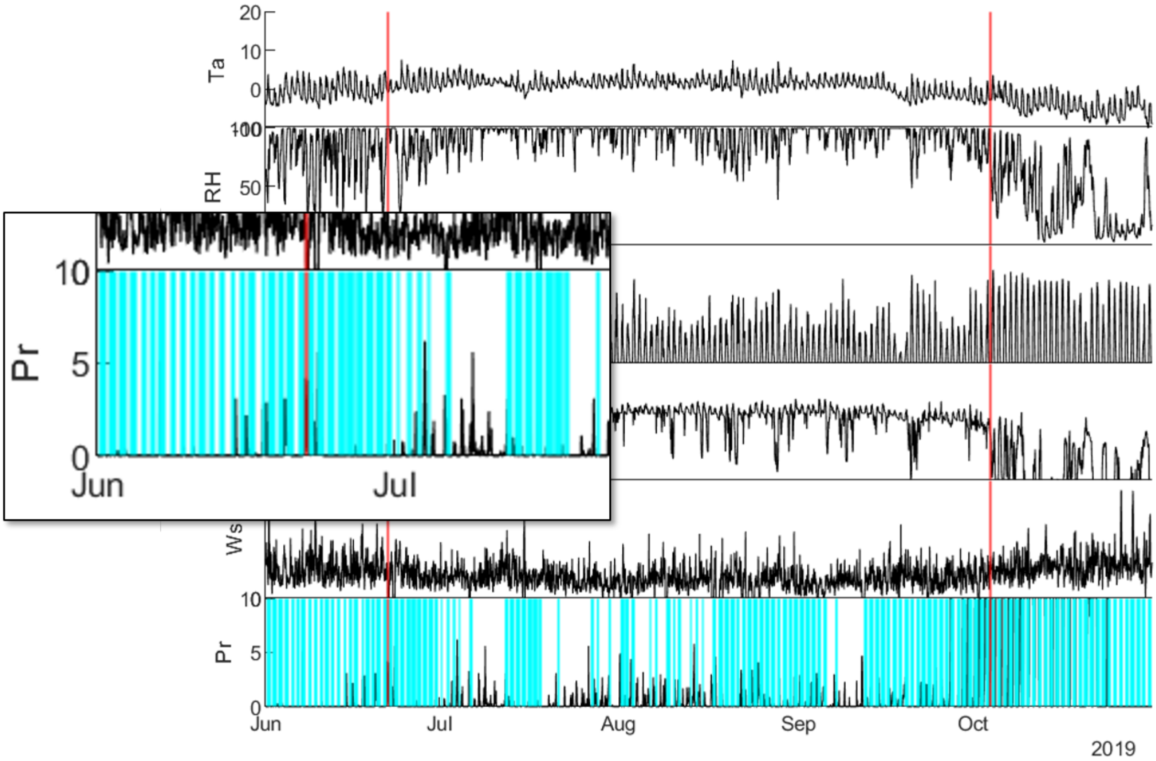


Figure R5. showing the AWS measurements, including precipitation and snow covered periods (cyan bars, where measured albedo exceeds 0.5) in the lowest panel (Figure S8)

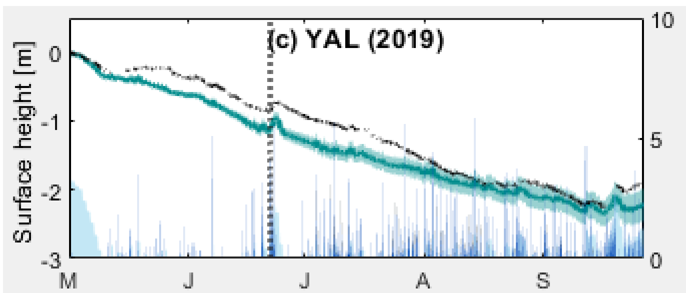


Figure R6, adding the month May to the considered period

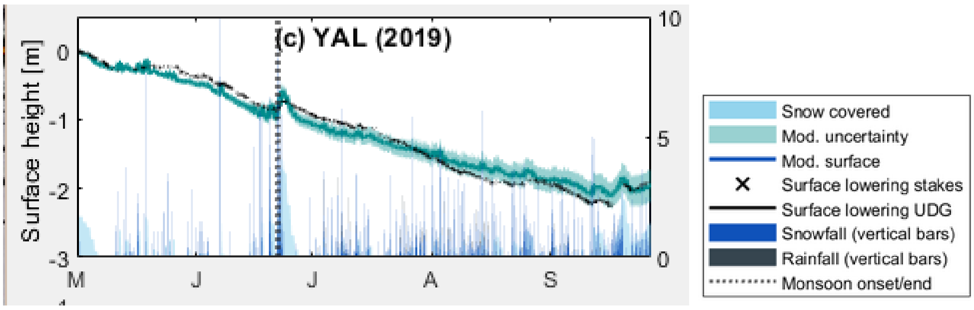


Figure R7, Model test: investigating the model results sensitivity to a doubling of the precipitation inputs in the pre-monsoon

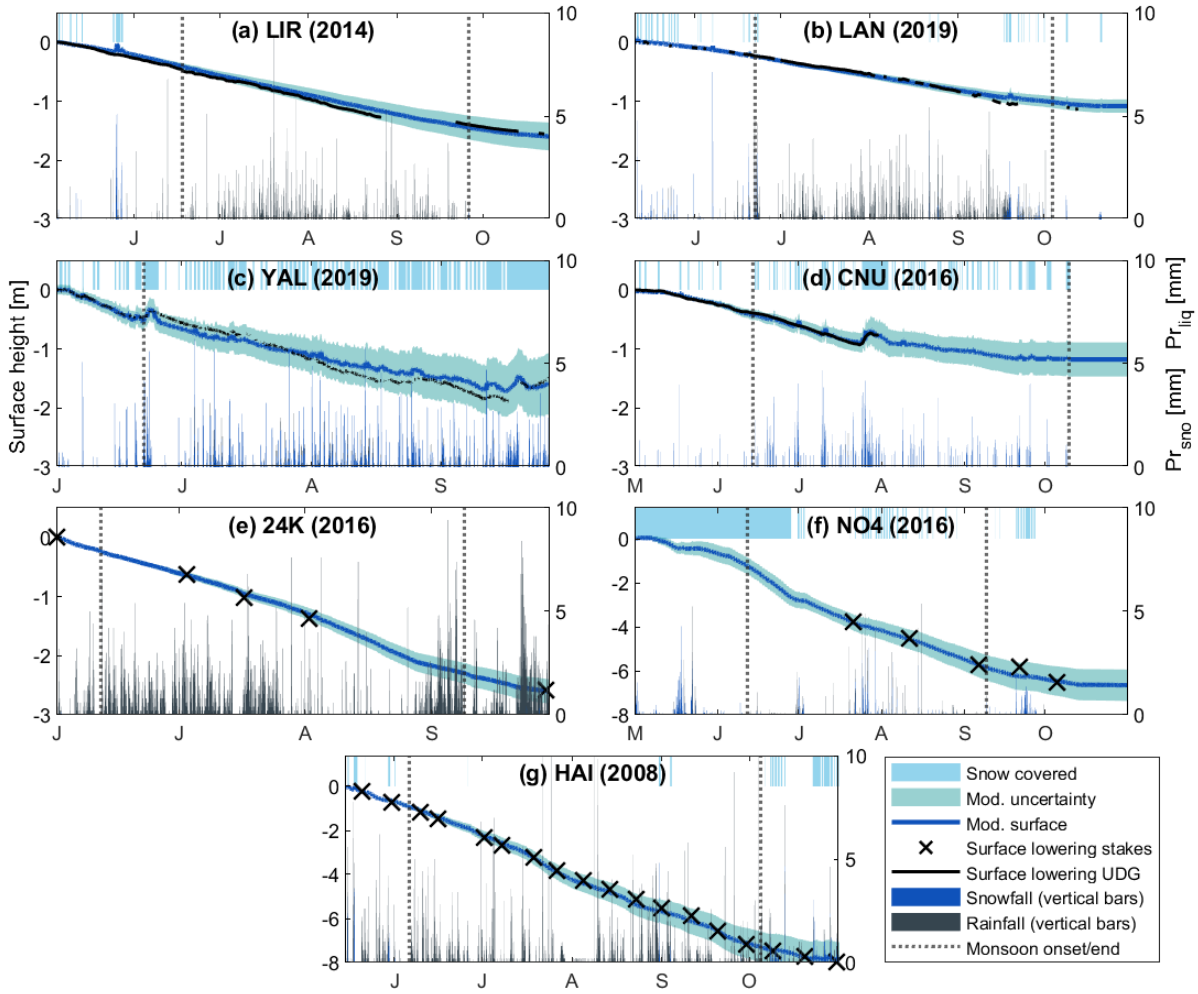


Figure R8, with adjusted representation of snow cover timing; Yala (YAL) model outputs maintained as in previous version (Figure 4)

R4.18: 323 – it is unclear how was the monsoonal period was identified in each record (Figure 3,4). Please add this detail and some discussion of how sensitive the results are to this choice.

We have added detailed information on the procedure we followed to define the monsoon onset and cessation dates, including a discussion on the uncertainties around that choice, in the Supplementary section *S2 Data selection and monsoon definition. (L 835-846)*

"At each site, we define the onset and recession date of monsoon based on visual inspection of the AWS records (Figures S2 to S8) following this procedure:

1. *Inspect SWin to identify a period with cloud overcast sustained for a few weeks and with few interruptions therein, lowering SWin*
2. *Inspect LWin and compare the timing of constantly higher LWin with the above.*
3. *Identify the period of increased rainfall frequency and intensity*
4. *Inspect the relative humidity and precipitation rates to see whether the timing of sustained humid conditions would agree with the above*
5. *Identify a plateau in average air temperature and dampening of the daily air temperature amplitude*
6. *Inspect wind speed to identify a regime change (mean and amplitude)*

This was the general procedure followed, but the order was varied in cases when one or the other variable provided a clearer indication. We note, that in some cases, *where heavy cloud cover and rainy conditions dominate the local weather from spring to autumn (e.g. Hailuogou, 24K) this distinction was less clear than in others, and some uncertainty remains around the exact monsoon onset and cessation dates at those study sites."*

R4.19: 323 – also, how were the individual years chosen from the multi-year records at each site? and how sensitive are the results to the chosen years?

We chose the records under considering the following criteria:

1. Data availability
2. Availability of complete forcing data for modelling, including precipitation records
3. Availability of stake measurements or other recordings of surface lowering (e.g. Ultrasonic Depth Gauge) for model optimisation and evaluation
4. Highest quality and reliability of records (No unrealistic/erroneous/disagreeing records)
5. Possibility to substitute from other stations when criteria 1.-4. were not met

We added this list to the Supplementary *Section S2 Data selection and monsoon definition. (L 828-834)*

R4.20: 355 – some comment on the direction of turbulent fluxes for clean ice glaciers would be useful here

Since this is part of the Section *4.3 Impact of debris cover,* we did not include the discussion on turbulent fluxes over clean-ice glaciers here. But we included this into the subsequent sections in response to a later comment *(*R4.33*)*

R4.21: 361 – "Reflected shortwave radiation SW↓, which removes energy from the surface, and which is controlled by the surface albedo, follows these changes (Figure 6), becoming less negative by +5:4 (24K, pre: -18:5, mon: -13:8) and up to +164:8 (Parlung No.4, pre: -219:6, mon: -54:8) between sites." This statement is ambiguous – do you mean that the changes in outgoing shortwave follow changes in albedo, or in incoming shortwave? If you mean the later, then this statement is not strictly true – the changes in outgoing shortwave radiation are dominated by changes to albedo at Parlung No 4. Please revise.

We removed the ambiguity as follows: *(L 356-358)*

"*Reflected shortwave radiation SW↑, which removes energy from the surface, and which is controlled by the surface albedo, becomes less negative (Figure 6), by between +5.4 (24K, pre: -18.5, mon: -13.8) and +164.8 (Parlung No.4, pre: -219.6, mon: -54.8) between sites.*"

R4.22: 364 – "an increase of the flux" – ambiguous (see comment for Figure 8). Suggest changing to" where SW↓ becomes more negative -12:1W m-2 (pre: -60:6, mon: -72:7), as a consequence of…"

We adjusted this as suggested!

R4.23: 374 – some comment on which term in SWnet is causing the increase melt would be useful here.

We added the following here:
"On Parlung No4 the SWnet changes are dominated by variations in SW↑, whereas on Yala, SW↓ down dominates" (L 371-372)

R4.24: 383 – "glacier-cooling H becomes a smaller flux" – ambiguous (see comment for Figure 8). Suggest "becomes less negative"

We adjusted this as suggested! (L 382)

R4.25: 385 – "change in LE partly offsets the changes in H, with increases in the flux ranging from…" – ambiguous (see comment for Figure 8). Suggest "LE becoming more negative by…"

We also adjusted this as suggested! (L 382)

R4.26: 387 – following your sign convention (energy input to surface is positive), H is increased (i.e. less negative) and LE is decreased (more negative) in monsoon period. Also, as the changes in radiative and turbulent fluxes do not balance separately, it would better to state that that "reduced SWin and more negative LE are balanced by increased LWin and less negative H."

This is a good suggestion, and we introduced the wording as suggested (L 386)

R4.27: Figure 6 – nice figure, but why choose to present only one glacier in the alternate depiction, and not the average of all sites of that type?

This figure is meant to help the reader interpret the changes and their direction, and allow them to follow through, how we worked out the commonalities (same surface type) and differences (between surface types) in the flux changes.
It did not seem reasonable, if not misleading, to average the EB components for a small number of sites, which are quite distinct in characteristics such as elevation, debris thickness, climatic situation, as well as in terms of snow covered periods. We instead chose to provide the numbers for example glaciers of each surface type and refer the reader to Table S2 which contains all other numbers.

R4.28: 397 - Figure A11 – this is an interesting and informative figure and should be described in the main body of the text (Section 4.4) for all surface cover types.

We agree, this figure indeed elucidates some of the processes behind the seasonally average changes by showing the EB shifts in the diurnal cycle, for example, how a balancing between the seasons might come about. We moved the figure to the main body of the manuscript (Figure 7) and included text parts describing it into the revised manuscript as follows in the Results:

in *4.4 Impact of the monsoon: (L 363-366)*

"...This balancing of the two LW components changes LW_net in the same direction at all sites over the diurnal cycle, with greater changes during the sunlit hours and smaller changes during the dawning and nighttime hours (Figure 7)"

in *4.4.1 Impact of the monsoon on clean-ice sites: (L 370-372)*
"...The difference in M is largely caused by the variability in SW_net, which almost entirely controls the melt of the clean-ice glaciers during monsoon. On Parlung No.4 the SW_net changes are dominated by variations in SW↑ whereas on Yala, SW↓ dominates. Hence, the bulk of the changes in the diurnal melt cycle happens during the sunlit hours (Figure 7 b, d).

in *4.4.2. Impact of the monsoon on glaciers with thick debris: (L 387-390)*
"…This balancing is also visible in the diurnal cycle of changes at Lirung, Changri Nup and 24K, where there is an increase in M during the night-time and morning hours, but a decrease in the afternoon hours (Figure 7 a, e, g). At Changri Nup (Figure 7e), the pattern is accompanied by a lag of around four hours between the peak changes of the radiative and turbulent fluxes.""

also where the monsoon interruption at 24K is mentioned *(L 393-395) "...This left a clear imprint in the diurnal cycle of changes, in the form of less pronounced flux changes due to the absence of heavy afternoon overcast in comparison to the other sites (Figure 7g) and resulted in higher melt rates during that period (Figure 4e)."*

and we maintained the part in *4.4.3 Impact of the monsoon a glacier with thin debris: (L 401-403)*
"...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 5g and Table S2), with higher increases during the nighttime than during the daytime (Figure 7f)"

Finally, we added to the Discussion:

in *5.3.1 Glaciers with thick debris: (L 485-488)*
"Trade-offs between the first and second halves of the day are likely to play a role in this balancing: Melt-rates increase between the two seasons due to warmer conditions in the morning hours, but decrease as a result of a strong reduction in energy inputs and enhanced evaporative cooling due to moisture availability during the afternoon hours (Figure 7, Section 4.4.2)."

and adjusted in *5.3.3 Glacier with thin debris (L 505-507)*
"While these increases in the turbulent fluxes are balanced with regards to M during the day by reductions in SW_net, both turbulent fluxes become important sources of additional melt energy during the night (Figure 7f and Section 4.4.3)."

R4.29: 404 – the regression model needs a much better description in its own section of the methods. It is unclear what the timescale of regression is (hourly/daily/weekly/seasonal) and what equations are used to fit the model. Without this it is hard to interpret the meaning of these results. Please revise methods and results.

We thank the reviewer for their comments related to the regression analysis. Revisiting this part greatly helped to improve it!
Both reviewers requested more information on the regressions, so we introduced new supplementary figures, providing the individual scatter plots and regression models, including adjusted R^2 values and model formulas in the plots (see Figures R1-3 below). In response to a comment made by Reviewer 3 (R3.4), we removed the text from the Methods and added it to the

supplementary material (Section S5. *Controls on turbulent fluxes*). There, we also added the formulae for the three predictors, the general formulation of the regression models and the timescale used. We improved the regression by removing values of $LE = 0\ W\ m^{-2}$, which corresponded to timesteps when the debris water interception storage is empty during evaporation conditions and no latent heat flux is possible. Upon revisiting this part and in response to a later comment of the reviewer, we then also replaced the temperature gradient *delta_T* with the vapour pressure deficit *vpd* as a predictor in the *LE* regression .

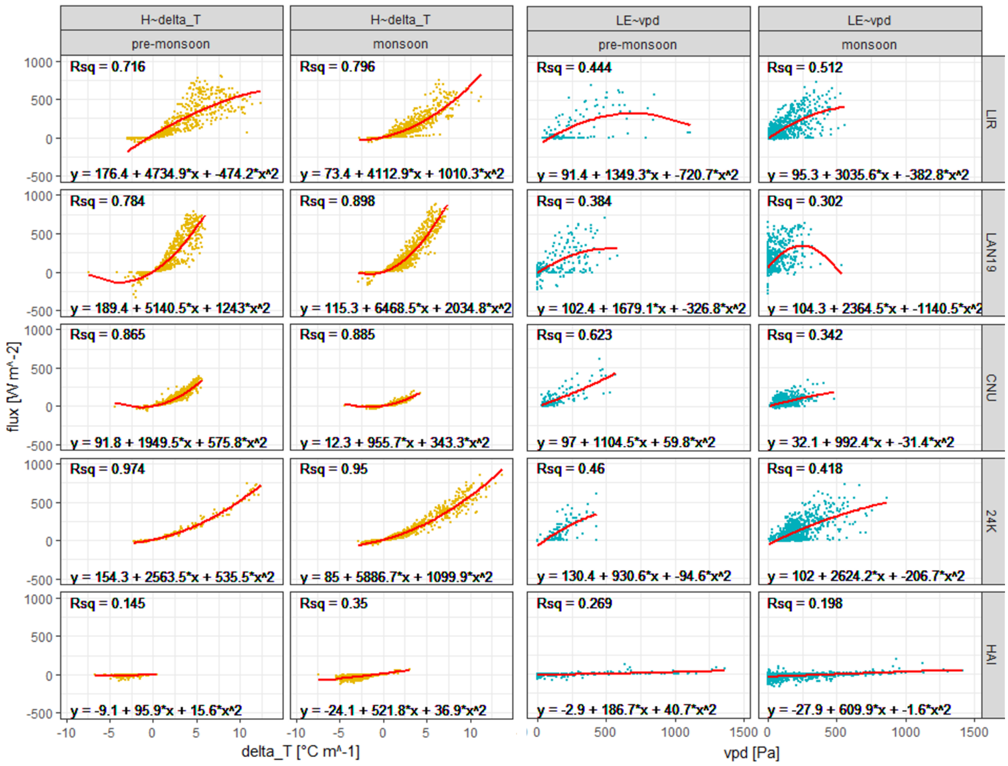


Figure R9: Regression of turbulent fluxes against temperature gradient *delta_T* (*H*) and vapour pressure deficit *vpd* (*LE*) for the debris cover sites (Figure S10)

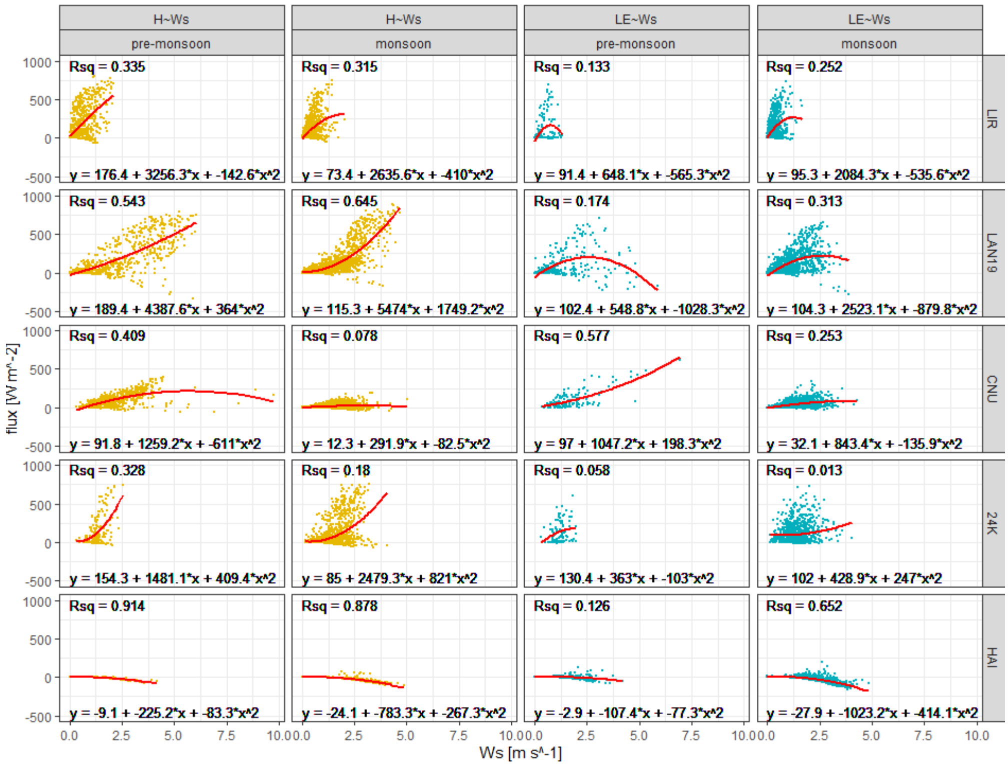


Figure R10: Regression of turbulent fluxes against wind speed *Ws* for the debris cover sites (Figure S11)

R4.30: 413 – "Neither RH, gT , or Ws on their own, nor their combination explain the variability of LE across sites with thick debris" – this analysis is not shown here. Please revise

This piece of text was outdated. Instead of gT, we use a new name delta_T in the updated version. We also dropped *RH* earlier during the manuscript preparation, but after revisiting the regression results now, we additionally introduced the vapour pressure deficit *vpd* (instead of *RH*), since it helps to create a more complete picture of the controls. We thank the reviewer for identifying this mistake! *(L 425-429)*

R4.31: Table A3 – this could be placed in the main body of the manuscript to support these results.

We thank the reviewer for this suggestion. We had moved this table to the former Appendix, following a request by Reviewer 2. We decided to leave it there, since there are already many figures and tables in the main manuscript.

R4.33: 456 – "the turbulent fluxes play a minor role on the clean-ice glaciers" – this statement only applies to sensible heat, as changes in turbulent latent heat fluxes are similar magnitude on clean ice and as debris covered glaciers. Please revise.

We thank the reviewer for raising this point! Here, we did not mean to downplay the importance of the latent heat flux at clean-ice glaciers in general, but meant more the role of it during monsoon and the energy flux as a result of evaporation/condensation. Given this, the sentence you pointed out seemed inaccurate and misplaced in the general part about debris cover, so we removed it.

Instead, we gave more differentiated explanations in the subsequent parts about the changes in the seasonal transition. You are also right that the latent energy flux changes are similar in magnitude to the ones on debris covered-glaciers, even if in the opposite direction, with the surface going from sublimation to condensation conditions.

We revised in the Results *4.4.1 Impact of the monsoon on clean-ice sites as follows: (L 372-375)*

"(all values in W m^2)...Both H and LE remain comparably small energy fluxes at the clean ice sites with highest averages of LE = −17.6 at Parlung No.4 and of H = −13.7 at Yala during the pre-monsoon (Table S2). At Parlung No.4, as much as 12.3 is added to the surface in the form of H during monsoon. Interestingly, LE changes from being a melt-reducing energy flux, emerging from sublimation during the pre-monsoon, to a small melt-contributing energy flux from condensation (< 4) at both clean-ice sites (Table S2).

We also revised the Discussion about the differences in the energy balances between pre-monsoon and monsoon. (L 494-499)
"...Outside of the monsoon, LE removes energy from the surface due to the sublimation of snow and ice. However, when entering the monsoon period, LE tends to switch sign (Figure 8), changing from sublimation/evaporation to condensation, which adds energy to the surface instead of removing it (Section 4.4.1.). This behaviour has not been indicated for the drier conditions on the Tibetan Plateau (Mölg et al., 2012; Sun et al., 2014), but has previously been observed at Himalayan sites with a 'southern influence' (Azam et al., 2014; Yang et al., 2017). Similarly, a small H flux is added to the surface at both sites during monsoon…".

R4.34: 489 – this is the first mention of the elevation and debris cover thickness sensitivity experiment and comes as a surprise. The sensitivity experiment could be worked in the main body of the manuscript (methods and results) or at least should be mentioned in the results section.

We now introduce this sensitivity experiment in its own section in the Results *4.4.4 Sensitivity of seasonal flux changes to elevation and debris thickness.* We left the methodological part in the Supplementary Material in order to not overload the main manuscript (see comments by reviewer 2), but we created clear links in the Results.

R4.35: Figure 8 – the sign convention does not follow the same logic as previous figures: 'increasing value' is ambiguous, as it can mean a more or less positive flux. Better to use 'increasing magnitude' along with the sign of the flux, or be specific and use 'positive change' and 'negative change' to refer to changes that increase and decrease the energy available for melt. Please revise here and throughout section 5.3.

We see the ambiguity that using 'magnitude' instead of 'value', and that using 'more positive' and 'more negative' is more precise. We revised this (Figure 10) and throughout Section 5.3. (see also R4.10, R4.21, R4.22, R4.24, R4.25, R4.26).

R4.36: 535 – "In contrast, the turbulent fluxes 'work for' the glaciers with debris above the critical thickness, and the melt-equalizing effect of debris under monsoon (Section 4.4.2) would likely remain in place". and 540 – "Here we confirm this hypothesis [that mass balance of debris-covered glaciers might be less sensitive to climate warming than clean-ice glaciers]" – This effect remains speculative as the sensitivity has not been tested. While the current study does highlight the different roles of turbulent heat fluxes over debris vs clean ice and how these change in monsoonal conditions, it doesn’t assess how this will change in the future. There may be other interactions that change the role of different fluxes in the future e.g. if RH also increases in the future, then the magnitude of evaporative losses during the monsoon may decrease, thereby further increasing the melt experienced under debris compared to present day. The dataset presents an opportunity to test the sensitivity, but this is not presented here. Please revise.

We thank the reviewer for these detailed considerations! In the revised text segment of *5.4 Implications for Himalayan glaciers in a changing climate*, we chose more careful language in order to not overstate the implications of our results under possible future monsoon changes, and to not present our thoughts as final answers, but as open questions yet to answer in future research. (L 547 - 556)

“Melt rates might increase to a lesser degree on debris-covered glaciers, since the turbulent fluxes ’work for’ the glaciers with debris above the critical thickness, and the melt-equalising effect of debris under monsoon (Section 4.4.2) might remain in place. These components could potentially sum up to have an overall protective effect on glaciers with thick debris, allowing them to resist the projected changes in the monsoonal summer longer into the future. Previous studies hypothesised that the mass balance of debris-covered glaciers might be less sensitive to climate warming than clean-ice glaciers (e.g. Anderson and Mackintosh, 2012; Wijngaard et al., 2019; Mattson, 2000). Here we additionally suggest that this difference in sensitivity could even be stronger in the monsoonal environments of the Central and Eastern Himalaya. Similarly, we suggest that glaciers with debris under the critical thickness might be even more sensitive to future monsoons than clean-ice glaciers. New energy-balance modelling studies incorporating similar datasets and future projections might provide answers to these yet open questions.”

R4.37: 554 – ‘modulated’ -> ‘increased’?

Yes, ‘increased’ is more precise. We changed it! *(L 582)*

R4.38: 556 – the cold surface also favours sensible heat exchange into the glacier surface – please revise.

We included the sensible heat exchange into the sentence.
“The cold surface favours condensation rather than evaporation as well as sensible heat exchange into the glacier surface.” (L 583-584)

R4.39: 557 – “melt-rates increase compared to the pre-monsoon at the clean-ice glaciers” – this is not significant for Yala – please revise.

Yes, we agree. This relates to the earlier discussion around Yala pre-monsoonal snow cover (R4.17). If we were able to model snow correctly from May 1 to June 22, this statement would most probably be true. We could not show this due to problems with snow-modelling in that period. We adjusted the text as follows: *(L 385-387)*

“…: melt-rates tend to increase compared to the pre-monsoon at the clean-ice glaciers and the glacier with thin debris cover (with the exception of Yala), while they stay similar at the glaciers with thick debris cover.”

R4.40: 563 – "The cooling induced by H at the same time decreases, with the result of unchanged available melt-energy M during monsoon." Increased LWin during the monsoon also helps offset the decreased SWin along with H. Please revise.

We agree with this and changed this part as follows: *(L 591-592)*
"The monsoonal decrease in SW↓ is further offset by an increase in LW↓ and a decrease in cooling induced by H, with the result of unchanged available melt-energy M during monsoon."

R4.41: 570 – it is unclear how the results support a reduced climate sensitivity at debris covered sites. Please provide additional material in the results/discussion or revise.

We revised this part of the Conclusions and made clear that these possible implications only hold for the summer season mass balance. We also added that this remains an open research topic: *(L 598-601)*

"Given these findings, under projected future monsoonal conditions, namely warmer and possibly longer and wetter monsoons (Sanjay et al., 2017; Moon and Ha, 2020; Masson-Delmotte, 2021), the summer season mass balance of glaciers with thick debris-cover might react less sensitively than the one of clean-ice glaciers and glaciers with thin debris. We encourage future research to answer this still open question. "

R4.42: Section A1 – this could be presented in the main body of the text (methods/results). Also, some features are ambiguous – i.e. was air temperature the only variable modified for elevation (i.e. not incoming longwave radiation and RH?

We moved one part of this analysis to the main body of the text, which is now Results Section *4.4.4 Sensitivity of seasonal flux changes to elevation and debris thickness*. We however maintained the methodological part in the Supplementary Material, where we reduced the ambiguity around the variables modified ("*...synthetically varying the AWS elevation to represent the range of elevation of each glacier ablation…") (L609, old version)* and adjusted it to: *(L 887-888)*

"We re-run the model varying Ta across the range of elevation of each glacier ablation zone by applying a lapse rate of 0.6°C/100m and, for the debris-covered sites, by varying the debris thickness in the range 10-80 cm (for ranges and steps see Table S3)."

R4.43: Figure A12 –Please modify caption as the fourth sentence infers debris cover was varied for all glaciers - Presumably the debris cover thickness was only varied for debris covered glaciers? Also the first sentence could be more instructive e.g. "Sensitivity of changes in the individual fluxes when moving from premonsoon to monsoon, to elevation and debris cover thickness (for debris covered glaciers only). "

We moved this figure to the main text (Figure 8), clarified the issue raised by the reviewer, simplified the caption and added a reference to Table S3.

R4.44: Table A3 – the derivation of cloud cover fraction should be described in the methods section.

Since nowhere in the text we refer to the cloud covered fraction, we decided to remove it also from this table (updated Table S2). For the reviewer's interest we however added the derivation below.

Following Juszak and Pellicciotti (2013) and Flerchinger et al. (2009), cloud cover fraction ccf is calculated as:

$$ccf = \frac{K+1}{0.84 + \frac{0.84}{e_{cs}}} \tag{5}$$

The cloudiness correction factor K is:

$$K = \frac{LW_{\downarrow}}{e_{cs}\sigma Ta_K^4} \tag{6}$$

where the Stefan Boltzmann constant $\sigma = 5.6704e-8\,[W\,m^{-2}\,K^{-4}]$, Ta_K is the air temperature in Kelvin, and the e_{cs} is the clear sky emissivity:

$$e_{cs} = \frac{59.38 + 113.7(\frac{Ta_K}{273.16})^6 + 96.96\sqrt{\frac{w}{25}}}{\sigma Ta_K^4}; \tag{7}$$

where w is the precipitable water:

$$w = \frac{4.65\,ea}{Ta_K} \tag{8}$$

Text snippet: Derivation of the cloud cover fraction ccf, as implemented in T&C

Editorial comments:

RE.1: 347 – “snow free” -> “snow-free”

We adjusted this here and in other places in the manuscript. *(L 341)*

RE.2: 350 – “re-emitted” it is perhaps better to say ‘lost’ as turbulent fluxes do not ‘emit’ energy, so to speak.

We used “returned” here, instead of ‘re-emitted’ *(L 345)*

RE.3: 385 – missing negative sign from “to -24.4”

We thank the editor for spotting this mistake and have adjusted it. *(L 384)*

RE.4: Figure 7 caption. As HAI is excluded from panel (b), the commas should be removed from the last sentence, so it reads “Only debris-covered glaciers where LE is a glacier-cooling flux are shown”

We removed the commas, as suggested. *(Figure 9)*

RE.5:Figure A10 – appears to be missing or perhaps figures need relabelling.

This was an error in the latex code and this figure did not exist. Thank you for spotting this and for your help with the manuscript!

Further changes:

1. We changed the colour of the snowmelt-shading to have the same colour at all sites for better visibility, since some of the colours we had chosen previously appeared too light.
2. Upon implementing the revisions, we discovered a mistake in the plotting of Figure 6, where bars/uncertainties/labelling of *Qv* and *G* were confused in the previous version. We corrected this now.

3. In Figure 6i, we also found that a wrong number was given for the change in SWdown. We corrected this now.
4. Following adjustments in the regression analysis in response to comments (R3.4 and R4.29), we added and revised some numbers in *4.5 Controls on the turbulent fluxes*

References

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