The reviewer's comments are in black and our answers in blue and italics.

In this study, the authors monitored the evolution of parts of the debris covered tongue of two glaciers (Langtang Glacier and 24K Glacier) during the monsoon season of the year 2019. They developed a very innovative setup to track the 3D surface changes of the surface at a weekly resolution. They use these weekly digital elevation models (DEMs), and some additional knowledge on the ice dynamics, to calculate the melt occurring from ice cliffs at very high spatial and temporal resolution. They compare their observed cliff melt rates to model simulations to decipher the controls on ice cliff melt and evolution.

I commend the authors for the impressive amount of work behind this manuscript. I think it is a very good addition to the literature, but I have some semi-major comments, and a number of specific ones. Addressing them might help clarifying some aspects of the manuscript.

We would like to thank Reviewer 1 for their very relevant and constructive comments. We agree that some clarification may be necessary for the points raised and we will try our best to address these concerns.

Specifically, we will:

1) *Reformulate paragraph 3.5 to make the uncertainty calculation clearer and focus on the systematic uncertainties. We will also add a table summarizing the final uncertainty values.*
2) *Add explanations to the text as to why we did not use the dynamic ice cliff model and prepare additional model runs to show the comparison with the current implementation.*
3) *Streamline the manuscript by shifting some of the methods and results sections to the supplementary material.*

Major comments:

The uncertainty assessment (section 3.5) is generally done in a careful way. The authors did their best to evaluate honestly the uncertainty associated with their newly developed technique. However, I have some concerns with equation 4, which basically assumes that the errors are uncorrelated (independent) for each pixel. In this study, the errors should be largely correlated because they can originate from e.g. a non-perfect adjustment of the camera, or from the ice flow correction. I therefore suggest to revise this equation, and the text afterwards (L344-346). It is also not so clear, whether these uncertainties are re-used after in the text and figures, because for the figures, they refer to the standard deviation, and not to the uncertainty analysis.

Our uncertainty analysis of the DEM differencing has shown that it could be decomposed into 1) a systematic (or correlated) error, given by the maximum absolute mean values of elevation change over stable terrain and 2) a random (or uncorrelated) error, given by the standard deviation of elevation change over stable terrain (Fig. 5). We could actually show that the bias could be considered negligible relative to the random error.

As pointed out by the Reviewer, Equation 4 only applies to the random error, and we agree that the current formulation of paragraph 3.5 was confusing in this sense. We will therefore reformulate it entirely to make these different points clearer and remove equation 4, as it is not particularly useful for everything that follows.

For simplicity, in the figures we indeed showed only the standard deviation of melt across the transects, which accounts for 1) the random error from the DEMs and 2) the melt variability at the surface of the cliff, and did not include the systematic uncertainty, which is much smaller, to avoid overloading the figures. We will make this clear in the text and will add a table showing the different components of the uncertainties.

Section 3.5 will therefore be rewritten as:

'To estimate the uncertainty in melt rate, we combined the uncertainties from the flow correction σ_{flow} with an estimation of the uncertainty of the calculation of melt distance from the DEMs σ_{DEM}.

We conservatively assumed the melt distance uncertainty σ_{DEM} to be equal to the uncertainty in elevation change as this removes the dependence on the terrain aspect and slope. Indeed, in the case of two DEMs with the same slope parallel to one another, which we considered to be the most short-term change due to ice melt, the elevation difference should be larger than the melt distances (e.g. Mishra et al., 2021), and the same should be true for their uncertainties. In the case of our study areas with complex geometries and viewing angles, we expected these uncertainties to vary with slope and aspect, as well as with the number of overlapping images, the distance from the time-lapse cameras, and the time difference with the reference DEMs (James and Robson, 2014b; Mallalieu et al., 2017; Armstrong et al., 2018; Filhol et al., 2019). We also expected the elevation change uncertainties to increase with time from the reference image set and distance from the time-lapse cameras, except in the very near-field where less overlap of the images should lead to higher uncertainties (Mallalieu et al., 2017).

The uncertainty from the melt distances σ_{DEM} comprises a systematic error $\sigma_{DEM,sys}$ given by the absolute mean elevation change over stable terrain, and a random error $\sigma_{DEM,rand}$ given by the standard deviation of elevation change over stable terrain. Depending on the evolution of the uncertainties in space, this relationship can be scaled by a factor *f*:

$$\sigma_{DEM} = \frac{1}{f} \times \sqrt{{\sigma_{DEM,sys}}^2 + {\sigma_{DEM,rand}}^2} \quad (1)$$

We therefore estimated the melt uncertainties in the cliff domain by analyzing the mean and standard deviation of elevation change over the moraine (Fig. 4). Indeed, the moraine was the closest feature to the survey domain that could be considered relatively 'stable', at least over a period of a few months. Furthermore, it had similar slopes and aspects to those of the cliffs in the survey domain, but was located in the background of the survey area, making it a good but conservative proxy for the features analyzed (Fig. 4). We conducted two different tests to estimate the melt uncertainties in the cliff domain. The first test (1) was to look at the evolution of the mean and standard deviation of the elevation changes relative to the reference DEMs over the moraine with time (Fig, 5a, b). The second test (2) was to look at the evolution of the mean and standard deviation of the elevation changes with distance for time-lapse DEMs taken within a few days from each other (Fig. 5c, d).

The mean value remained between +/-0.2 m for Langtang, where the moraine was ~800 m away from the cameras (Fig. 5a), and between +/-0.05 m for 24K, where the moraine was ~400 m away from the cameras (Fig. 5b). Using a factor 2 to account for positive and negative biases, we obtained $\sigma_{DEM,sys}$ = 0.4 m for Langtang and $\sigma_{DEM,sys}$ = 0.1 m for 24K (Table 2). $\sigma_{DEM,rand}$, given by the standard deviation, increased with time during the first two months of the time-series for Langtang, until it reached a value of ~1 m, while it remained stable around 0.6 m for 24K during the whole period. For (2), we took the DEM the furthest away in time from the reference DEM and processed the image pairs taken within 48 hours of this new reference DEM, only keeping the resulting DEMs with a mean elevation change relative to the reference DEM lower than 0.2 m for Langtang (4 remaining DEMs) and 0.05 m for 24K (7 remaining DEMs) (Fig. 5a, b, dashed lines). The elevation change patterns of these near-contemporaneous DEMs highlighted a factor f = 2 increase in standard deviation with distance between the cliff domain and the moraine for Langtang and f = 1.7 for 24K (Fig. 5c, d). As a result, for Langtang σ_{DEM} = 0.5 m and for 24K σ_{DEM} = 0.4 m (Table 2). These are the same values as if we had calculated them from the random errors only, which means that the systematic errors can be considered negligible.

We also needed to account for the uncertainties related to the flow correction, which we assumed to be equal to the quadratic sum of the 1σ surface velocity uncertainty σ_{xy}, the 1σ emergence velocity uncertainty σ_b estimated following the approach and assumptions described by Miles et al., 2018, and the uncertainty from the slope correction σ_{Slope}:

$$\sigma_{flow} = \sqrt{{\sigma_{xy}}^2 + {\sigma_b}^2 + {\sigma_{Slope}}^2}. \quad (2)$$

For the uncertainty on the slope correction, we assumed a 2° uncertainty in the slope angle. As a result, the 1σ uncertainty from flow correction was equal to 0.007 m.day^{-1} for Langtang and 0.004 m.day^{-1} for 24K.

The 1σ melt uncertainty for each pixel could be expressed as:

$$\sigma_{Melt} = \sqrt{{\sigma_{DEM}}^2 + \left(\sigma_{flow} \times dt\right)^2}, \quad (3)$$

where dt is the number of days over which the melt is calculated. Ultimately, we calculated melt on a tri-weekly basis for Langtang and a bi-weekly basis for 24K to reduce the uncertainties relative to the measured melt rates. This meant that the uncertainty from flow was an order of magnitude lower for these domains and could therefore be neglected: $\sigma_{Melt} \backsimeq \sigma_{DEM}$ = 0.5 m (0.02 m/day) for Langtang and $\sigma_{Melt} \backsimeq \sigma_{DEM}$ = 0.4 m (0.03 m/day) for 24K over their respective tri- and bi-weekly melt periods (Table 2).

Table 2: Uncertainty estimations for Langtang and 24K.

Glacier	Random DEM uncertainty $\sigma_{DEM,rand}$ (m)	Systematic DEM uncertainty $\sigma_{DEM,sys}$ (m)	Scaling factor f (-)	Flow correction uncertainty σ_{flow} (m/day)	Averaging period dt (days)	Final uncertainty σ_{Melt} (m)	Final uncertainty σ_{Melt} (m/day)
Langtang	1	0.4	2	0.007	21	0.5	0.02
24K	0.6	0.1	1.7	0.004	14	0.4	0.03

Based on this, in all that follows used the standard deviation of melt at the cliff location to account for these uncertainties, as it directly accounts for 1) the random error from the DEMs and 2) the melt variability at the surface of the cliffs.'

My second major comment is about the model used in the study. I don't fully understand why the authors used the static version of the model instead of the dynamic one. The dynamic model would be a great way to assess the share of each process (surface energy balance vs. 'geomorphic' processes). As it stands the manuscript is a bit frustrating, because the description of processes related to the redistribution of debris remains extremely descriptive. I am also curious about the model calibration, if there is any, because no details are provided about it.

This is a very good question, which we are happy to clarify. Indeed, the dynamic-geometry cliff model presented in Buri et al., (2016b) was extended from the static-geometry cliff energy-balance model presented in Buri et al., (2016a) to one able to predict cliff evolution (and thus simulating changes in slope, aspect and size) by taking into account both melt and debris redistribution and neighbouring supraglacial lakes. The reviewer is correct that, ideally, a dynamic model would be appropriate for the purpose of our manuscript. However, the dynamic model was formulated and constrained with limited data of cliff geometry (a pre- and a post- ablation season DEM for each of four cliffs on one glacier). Due to the limited data available for the model development and the underlying risk of equifinality, the processes influencing the cliff dynamics (debris redistribution, additional melt from ponds) were represented by rather simple parameterisations lumping together distinct physical processes. For example, debris redistribution was simply constrained by a slope threshold above which debris would be removed, without accounting for the actual debris motion or mass conservation (Buri et al., 2016b; Moore, 2018). Similarly, the pond influence was represented by an additional melt rate at the base of the cliff, constant over the entire cliff-pond interface, and calculated from the modeling of one pond only. As such, we feel that those parameterisations and corresponding empirical parameters are too simple to represent the complexity of changes occurring during one melt season - whereas they were appropriate to provide bulk changes over long periods (the entire season, or monthly intervals, as in Buri et al., 2016b, 2018, 2021).

Our objective for the modelling in this study was to understand the spatiotemporal variability of the energy-balance and melt patterns of the cliffs at very high resolution. For this purpose, leveraging the actual cliff geometry at each time-step is extremely beneficial, as the actual and modelled cliff geometry will diverge even over short time-scales (1 week) due to the cliff process complexity and the lumped process representation in the dynamic model discussed above. As such, the dynamic model would be less reliable for understanding the local energy *balance, even if calibrated to the studied cliffs. Overall, we found that the mixed approach (observations of cliff geometry to drive the energy-balance model, which is the model component for which we have high confidence) allows to leverage the best of the observations (high resolution, frequent DEMs) and the best of the advanced model of Buri et al., 2016a (its EB component) to understand with unprecedented resolution and detail cliff melt patterns. The further development of a dynamically evolving cliff geometry model appropriate for high temporal resolution is of high interest to our group of authors and this line of research, but it would require a substantial investment to collect additional data from more cliffs, and more than two glaciers, to make the model physically representative, and it was thus outside the scope of this work - where our main goal was to understand the complex patterns of short term cliff evolution over the studied cliffs. The key step forward in our study is the ability to constrain cliff geometry changes on a weekly basis and calculate an adjusted energy balance (notably from radiative fluxes) based upon a known cliff geometry. Accordingly, we retain a high confidence that we are modelling the energy balance well at the surface.*

We do feel that the development of a more process-oriented dynamic cliff model would be an important advance for the community. Advances in the past few years may contribute towards this, for instance improvements in the representation of debris motion at the surface of glaciers (Moore, 2018; 2021; Anderson and Anderson, 2018; van Woerkom et al., 2019; Westoby et al., 2020). While there are still some important knowledge gaps (for example related to the sliding of debris on steeper slopes or the debris evacuation by streams or ponds), we are convinced that the way forward for the cliff dynamic model would be to represent these processes in a much more physical way. This was not possible a few years ago, but the multitemporal UAV or time-lapse datasets (DEMs, orthoimages) that have been produced in recent years (Westoby et al., 2020; Sato et al., 2021; this study) should enable this next major step in future work.

Despite all these different elements, we understand the interest of the reviewer regarding the ability of the dynamic model to represent the melt and evolution of ice cliffs at different sites. We will therefore conduct additional tests with the cliff dynamic model to show how it compares with the current model formulation. Also, we will indicate the different points mentioned above directly in the paper by adding 1-2 sentences in Sections 3.7. and 5.3.

For the descriptiveness of the text regarding debris motion, we will streamline the results description in section 4.2 and focus on the main findings and move the more descriptive results to the SI.

Finally, to answer the question related to the calibration of the energy-balance model, we used the exact same parameters as Buri et al., (2016a), no further calibration was conducted and the same parameters were used for both glaciers. We will make sure to specify this point in the first paragraph of section 3.7.

My third comment is about some sentences in the discussion and conclusion that I find slightly misleading, or not well supported by the data. For instance, in l253-254, l400-403 and l631-633, it is written that time and space integrated methods lead to an underestimation of melt rates of 50%. This is not really correct, it is just that the methods are looking are measuring different targets: previous methods measured integrated losses that include reburial and expansion, and thus also melt beneath debris, while here the authors focus on shorter time scale and on melt rates along cliff transects. The same comment applies for the comparison between the melt estimates calculated for the ice cliffs vs. the sub-debris melt (e.g. L23-25 and L394-399). The calculation of this ratio also lacks details, because little is said about the uncertainties and the representativeness of each end-member.

Indeed, our study has been focusing on data with very high spatio-temporal resolution, something that had not previously been done for ice cliffs. We believe that the comparison we made with other methods is interesting as it shows that depending on the definition adopted for the calculations, the melt contribution of ice cliffs can change considerably. The point here is to put the different approaches into perspective and show what is missed through the mixing of ice cliffs and sub-debris melt when more simple approaches are taken due to less temporarily resolved data. Similarly, a recent study has also highlighted the differences between extracting melt vertically and normal to the slope (Mishra et al., 2021).

Some of the statements we made may have been misleading in this sense and we will correct them as follows:

L400-403: 'The high temporal resolution of this dataset enables one to precisely estimate the melt contribution of ice cliffs, which is 4 to 129% greater than if the Pléiades DEMs were used for Langtang and up to 27% more than if the UAV DEMs were used for 24K (Table S4), due to the mixing of ice cliff and sub-debris melt contributions for less temporally-resolved data.'

L631-633: 'The time-lapse camera DEMs enabled a precise quantification of the cliff melt by accounting for sub-seasonal cliff geometry changes, which are ignored when extracting melt from pre- and post-monsoon DEMs.'

Regarding the comparison between the melt estimates calculated for the ice cliffs VS sub-debris melt, we feel that it is also a relevant one when thinking about ice cliff enhancement factors relative to sub-debris melt, which is a key metric to assess the contribution of ice cliffs to the melt of debris-covered glaciers (e.g. Brun et al., 2018; Mishra et al., 2021; Rounce et al., 2021). The calculation was not explicitly described in the text and we will update this and take the mean+/-STD of cliff melt and divide it by the mean+/-STD of sub-debris melt to make sure that it is representative (L394-399). L23-25 we will simply remove the comparison with the sub-debris melt rates, as it is not so relevant for the abstract.

I found the paper slightly lengthy in some places. For instance, the introduction could be more concise, or the line 361-370 about the surface energy balance model are not extremely useful. While this is not a major issue, it would streamline the manuscript to check for each sentence whether it is relevant for the general paper scope. Similarly, the results section 4.2 that presents each cliff's evolution is difficult to follow. The authors do not link very well the morphologic and meteorological changes happening to changes in the melt rates, and instead follow a more descriptive approach.

We thank the reviewer for their comments on this. We will make sure to streamline the sections mentioned here and make sure to outline the link between morphologic and meteorological change to changes in melt rates. Specifically we will:

1) *Cut some of introduction sentences and especially condense paragraph L70-82*
2) *Streamline section 3.2 and stick to a shorter summary of the methods*
3) *Condense the text in section 3.5 and make it more to the point (as shown in previous comment)*
4) *Streamline the paragraph L361-370 and remove all the repeated content from the introduction*
5) *Focus on the most important results for each cliff in section 4.2 and make the links between meteorology and dynamics clearer. The remaining and more descriptive results will be moved to the Supplementary Material.*
6) *Condense L618-625 and merge with previous paragraph.*

Specific comments:

We thank the reviewer for their thorough reading of our manuscript and will make sure that all these points are corrected as suggested.

In some places, there are some sentences in bold font. They should be in normal font.

We will remove the bold fonts.

L18: “Tibet” -> should be “China” for consistency with “Nepal”, no?

Agreed.

L20-21: the uncertainty is given at pixel of cliff scale?

This is the systematic error of the DEMs scaled with distance calculated in Section 3.5. This should actually be divided by 2 as this is for the DEM positioning rather than the DEM differencing. This will be clarified in the text: ‘We derive weekly flow-corrected DEMs of the glacier surface with a maximum bias of +/-0.05 m for Langtang Glacier and +/-0.03 m for 24K Glacier…’

L27: I didn’t find elements in the text that supports this statement in a quantitative way.

This refers to L543-548 of the discussion. We will make sure to specify these values there.

L36: it’s the ice and not the “ice cliff” that is directly exposed. The sentence should be re-phrased.

Agreed.

L45: consider adding “for specific locations”.

Agreed. Will add it.

L115: which part of the glacier section is included in the calculation of slope?

This is the slope of the whole glacier, but since the focus is on the characteristics of the debris-covered portions of the glaciers, we will indicate the slope of the debris-covered parts instead (this will not change the rest of the sentence).

L130: is it relevant to quote the price in a scientific publication? I suspect anyone interested by the setup will contact the authors.

We will remove it.

L191: maybe a slightly more quantitative assessment of the quality of the co-registration would be good

We will indicate the improvement in terms of velocity over the off-glacier terrain.

L193-195: the mean elevation difference on stable ground is very large (larger than the absolute value of the emergence for 24K!), and would imply a potential systematic bias. Is the median at zero? The authors should consider putting the mean or median off-glacier elevation at zero.

This is a good point. We will make sure to put the mean to 0 and update the figures as needed.

L292-293: why not calculating all the uncertainties within the normal intersection framework?

We chose the conservative approach of taking the vertical uncertainty as the melt uncertainty as this removes the dependence on the slope of the off-glacier terrain and its (lack of) representativeness for the ice cliffs. We will specify this point better here.

L330: how does the 2° uncertainty in the slope angle translates into the sigma_flow? Also the units of the different components of sigma_flow are never explicitly written.

We will make sure to specify these elements in the text.

L413: missing figure number

Thanks for spotting this, we will correct it.

L423-432: what is the impact on the melt rate?

We will make sure to link the observations with the changes in melt rates in Section 4.2.

L539-554: here I would expect a quantitative analysis, which remains too descriptive

We will specify the corresponding values for these two paragraphs.

L571: a bit in contradiction with L571

Apologies but we do not see this contradiction?

L603-605: then why not testing the dynamical parametrization of the model?

We have provided a detailed answer to this point in the major comments.

Section 5.3: it would be worth acknowledging the limitations of the study (very small area surveyed, only one full ablation season, north facing cliffs only…)

Agreed. We will add a sentence acknowledging these limitations specifically.

L627: “bridged a crucial gap” -> this sounds a bit like overselling the study

We will replace it with ‘considerably improved our understanding of ice cliff evolution’

L627-648: the conclusion is mostly based on very general sentences, and does not do a good job in highlighting the specific findings of the study. It should be re-written to better highlight the novel aspects of this study. I would recommend not to use too many bullet points in the conclusion.

We will remove the main bullet points and instead add details/numbers to highlight the specific findings.

Fig. 7 a and b: if I read the caption well, it says that the shading area represents the standard deviation, but what standard deviation? The spatial one? Why not using the uncertainties calculated from the uncertainty analysis for the observed melt rates?

See detailed response to the major comment.

Fig. 7 c and d: there is a unit problem for the energy fluxes. They are not the right order of magnitude.

Thanks for this. We will double-check these values.

Fig. 8: is there an influence from the large boulder on top of the cliff edge? It is never mentioned in the text I think?

We have not identified any clear effect, but will mention it in the results.

Fig. 9 and all the following ones with the same design:

- I find it very confusing to represent the cumulative precipitation for overlapping periods, it suggests a huge amount of precipitation, because most periods are counted twice. I suggest expressing the precipitation in mm/day instead.

We will change to mm/day.

- On the panel d: at which time resolution are the fluxes plotted? When two model runs overlap, which one is used for the flux plot? Also fluxes should be in W m-2 and not melt contribution, because they can be negative.

We took the central value of each period, we will specify it in the caption. We will however keep the fluxes as ‘melt contribution (m/day)’ to keep the link with the left panels, and specify the equivalence in the caption.

References

Anderson, L.S., Anderson, R.S., 2018. Debris thickness patterns on debris-covered glaciers. Geomorphology 311, 1–12. https://doi.org/10.1016/j.geomorph.2018.03.014

Buri, P., Pellicciotti, F., Steiner, J.F., Miles, E.S., Immerzeel, W.W., 2016. A grid-based model of backwasting of supraglacial ice cliffs on debris-covered glaciers. Ann. Glaciol. 57, 199–211. https://doi.org/10.3189/2016AoG71A059

Buri, P., Miles, E.S., Steiner, J.F., Immerzeel, W.W., Wagnon, P., Pellicciotti, F., 2016. A physically based 3-D model of ice cliff evolution over debris-covered glaciers. J. Geophys. Res. Earth Surf. 121, 2471–2493. https://doi.org/10.1002/2016JF004039

Buri, P., Pellicciotti, F., 2018. Aspect controls the survival of ice cliffs on debris-covered glaciers. Proc. Natl. Acad. Sci. 115, 4369–4374. https://doi.org/10.1073/pnas.1713892115

Buri, P., Miles, E.S., Steiner, J.F., Ragettli, S., Pellicciotti, F., 2021. Supraglacial Ice Cliffs Can Substantially Increase the Mass Loss of Debris-Covered Glaciers. Geophys. Res. Lett. 48. https://doi.org/10.1029/2020GL092150

Brun, F., Wagnon, P., Berthier, E., Shea, J.M., Immerzeel, W.W., Kraaijenbrink, P.D.A., Vincent, C., Reverchon, C., Shrestha, D., Arnaud, Y., 2018. Ice cliff contribution to the tongue-wide ablation of Changri Nup Glacier, Nepal, central Himalaya. Cryosph. 12, 3439–3457. https://doi.org/10.5194/tc-12-3439-2018

Filhol, S., Perret, A., Girod, L., Sutter, G., Schuler, T. V., Burkhart, J.F., 2019. Time-Lapse Photogrammetry of Distributed Snow Depth During Snowmelt. Water Resour. Res. 55, 7916–7926. https://doi.org/10.1029/2018WR024530

James, M.R., Robson, S., 2014. Sequential digital elevation models of active lava flows from ground-based stereo time-lapse imagery. ISPRS J. Photogramm. Remote Sens. 97, 160–170. https://doi.org/10.1016/j.isprsjprs.2014.08.011

Mallalieu, J., Carrivick, J.L., Quincey, D.J., Smith, M.W., James, W.H.M., 2017. An integrated Structure-from-Motion and time-lapse technique for quantifying ice-margin dynamics. J. Glaciol. https://doi.org/10.1017/jog.2017.48

Mishra, N.B., Miles, E.S., Chaudhuri, G., Mainali, K.P., Mal, S., Singh, P.B., Tiruwa, B., 2021. Quantifying heterogeneous monsoonal melt on a debris-covered glacier in Nepal Himalaya using repeat uncrewed aerial system (UAS) photogrammetry. J. Glaciol. 1–17. https://doi.org/10.1017/JOG.2021.96

Moore, P.L., 2018. Stability of supraglacial debris. Earth Surf. Process. Landforms 43, 285–297. https://doi.org/10.1002/esp.4244

Moore, P.L., 2021. Numerical Simulation of Supraglacial Debris Mobility: Implications for Ablation and Landform Genesis. Front. Earth Sci. 9. https://doi.org/10.3389/feart.2021.710131

Rounce, D.R., Hock, R., McNabb, R.W., Millan, R., Sommer, C., Braun, M.H., Malz, P., Maussion, F., Mouginot, J., Seehaus, T.C., Shean, D.E., 2021. Distributed global debris thickness estimates reveal debris significantly impacts glacier mass balance. Geophys. Res. Lett. e2020GL091311. https://doi.org/10.1029/2020GL091311

Sato, Y., Fujita, K., Inoue, H., Sunako, S., Sakai, A., Tsushima, A., Podolskiy, E.A., Kayastha, R., Kayastha, R.B., 2021. Ice Cliff Dynamics of Debris-Covered Trakarding Glacier in the Rolwaling Region, Nepal Himalaya. Front. Earth Sci. 9, 398. https://doi.org/10.3389/FEART.2021.623623/BIBTEX

Van Woerkom, T., Steiner, J.F., Kraaijenbrink, P.D.A., Miles, E.S., Immerzeel, W.W., 2019. Sediment supply from lateral moraines to a debris-covered glacier in the Himalaya. Earth Surf. Dyn. 7, 411–427. https://doi.org/10.5194/esurf-7-411-2019

Westoby, M.J., Rounce, D.R., Shaw, T.E., Fyffe, C.L., Moore, P.L., Stewart, R.L., Brock, B.W., 2020. Geomorphological evolution of a debris-covered glacier surface. Earth Surf. Process. Landforms 45, 3431–3448. https://doi.org/10.1002/esp.4973