Reply to comments on the manuscript

‘Sub-seasonal variability of supraglacial ice cliff melt rates and associated processes from time-lapse photogrammetry’

Marin Kneib, Evan S. Miles, Pascal Buri, Stefan Fugger, Michael McCarthy, Thomas E. Shaw, Zhao Chuanxi, Martin Truffer, Matthew J. Westoby, Wei Yang, and Francesca Pellicciotti

Response to editor and general revision

Dear authors,

Thank you for your comments to the reviewers. The two reviewers provided some suggestions to clarify uncertainty assessment, model selection, and time-varying albedo parameterisation. Additionally, they both pointed out that the paper is lengthy. I agree with these comments and encourage you to take into account these comments in the revised paper.

Best,

Dr. Kang Yang
Editor, The Cryosphere

Dear Prof. Yang,

Many thanks for the consideration of our manuscript. We have received two well-informed reviews which are overall very positive on the quality of the analysis and relevance of the science presented in this manuscript. Both reviewers also raised some valid points related to the uncertainty estimation, model selection and albedo parameterization.

We would like to thank very much all reviewers for their constructive and thoughtful comments, which have surely contributed to improve the manuscript. In response to these comments, we have conducted the following changes:

- ***Uncertainty assessment**: We have entirely reformulated paragraph 3.5 to make the uncertainty calculation clearer. We have also added a table in the main text with the different uncertainty values.*
- ***Model selection**: The choice of the static model updated with the time-lapse DEMs was made because the dynamic cliff model was not meant to represent*

local processes influencing the cliff evolution (debris redistribution, pond influence) at such a fine temporal scale. As the goal of this study was to use the model to isolate the energy-balance from the cliff evolution, using the dynamic model would have led to errors in the energy balance and would have made the melt patterns difficult to interpret. By using the static model with updated geometry, we ensure that the energy fluxes are as accurate as possible. Indeed, our results show that the updated geometry model predicts melt estimates in line with our observations. We have provided a very detailed response to these comments, including figures of the cliff geometry changes when the dynamic model is run at such a high spatio-temporal resolution without recalibration of the geometric change parameters.

- ***Albedo parameterization**: We have now added some details about this topic in the discussion. In short, this would be a worthwhile way forward, but would need more data to come up with a parametrization that is transferable between sites.*
- ***Length**: We have streamlined the text in several sections of the manuscript, including the introduction and discussion. We have also moved parts of the methods to the supplementary material and condensed the description of the cliff evolution in the results.*
- ***Figures**: We found a mistake in the color scale of figures 8, 10, 12, 14, S4, S5, S7, S9, which was supposed to go from 0 to 0.1 m/day instead of 0 to 1 m/day. We have now made the correction.*

We think the manuscript has been strengthened by these revisions, but none of our main results or conclusions have changed.

We have provided detailed answers to each of these comments in this document. The comments are in black and our answers in blue and italics. The line numbers in our answer correspond to the ones in the 'track changes' version of the revision.

Thank you for your consideration of our revised manuscript, which we hope now is acceptable for publication. Please address correspondence to me at marin.kneib@wsl.ch.

Kind regards,

Marin Kneib and Co-authors

Response to reviewer 1

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 was necessary for the points raised and we have addressed these concerns as best as we could.

Specifically, we have:

1) *Reformulated paragraph 3.5 to make the uncertainty calculation clearer. We have also added a table summarising the final uncertainty values.*

2) *Added explanations to the text as to why we did not use the dynamic ice cliff model and prepared additional model runs to show the comparison with the current implementation.*

3) *Streamlined 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 bias 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 are able to show that the bias is 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 have therefore reformulated it entirely to make these different points clearer and removed equation 4, as it was 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 represents 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 have now made this clear in the text and have added a table showing the different components of the uncertainties.

Section 3.5 was 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 common 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 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*:

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

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 $\sigma_{DEM} = 0.5\ m$ and for 24K $\sigma_{DEM} = 0.4\ m$ (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} (all in m.day^{-1}):

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

Where:

$$\sigma_{Slope} = \sigma_{xy}\tan(\alpha) + \frac{d\alpha}{\cos^2\alpha}u_s \approx u_s d\alpha \qquad (3)$$

Where α is the mean glacier slope in the survey domain, and u_s the mean velocity. For the uncertainty on the slope correction, we assumed a $d\alpha = 2° = 0.03\ rad$ uncertainty in the slope angle, which results in $\sigma_{Slope} = 0.03$ cm.day^{-1} for Langtang and 0.06 cm.day^{-1} for 24K. 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:

$$\sigma_{Melt} = \sqrt{{\sigma_{DEM}}^2 + \left(\sigma_{flow} \times dt\right)^2}, \qquad (4)$$

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: $\sigma_{Melt} = \sigma_{DEM} = 0.5\ m\ (0.02\ m/day)$ for Langtang and $\sigma_{Melt} = \sigma_{DEM} = 0.4\ m\ (0.03\ m/day)$ for 24K over their respective tri- and bi-weekly melt periods (Table 2).

Based on this, in all that follows we used the standard deviation of melt at the cliff location to represent 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. We note however that assuming a Gaussian error for independent measurements, the random error from the DEMs becomes negligible (<0.05 m) for the average melt when the number of pixels considered is greater than 100, which is always the case here.

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

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, and at the temporal scale of this study (weekly geometry updates) - 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, the lumped process representation in the dynamic model discussed above, and the very high temporal resolution at which the geometry needs to be updated. 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 the highest confidence based on past validation efforts [Sakai et al., 1998, 2002; Han et al., 2010; Reid and Brock, 2014; Steiner et al., 2015]) 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 (Fyffe et al., *2020; 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 therefore conducted additional tests with the cliff dynamic model to show how it behaved in this particular framework (Fig. R1, R2). We ran the cliff dynamic model at 1m resolution for all four studied cliffs (tests at 1.2m resolution showed that the spatial resolution had a limited influence on the model results), updating the geometry every time there was a new time-lapse DEM (~weekly intervals). We used a slope threshold of 35° below which the cliff could be reburied and used otherwise the same parameters as the ones used in the study by Buri et al. (2016b). In all cases the cliffs shrink rapidly and disappear entirely within one month (Fig. R1, R2). We additionally ran the dynamic model with a monthly temporal resolution for the 24K cliffs, which led to the cliff shrinking, but much slower than with weekly geometry updates, as it survived three months longer (Fig. R3).

This shows that the slope threshold is not the only parameter responsible for this unexpected evolution of the cliffs, and that this is at least partly due to the high temporal frequency of the geometry updates, for which the spatial resolution is much higher than the melt distance. The dynamic model was not designed for such cases, and therefore struggled to maintain the cliffs. It would therefore require to be entirely re-calibrated to be relevant here, which is beyond the scope of this study as it does not contribute particularly to our research questions. Furthermore, based on the availability of this data, we believe that further efforts in this direction should rather aim to represent the geometry updates and debris processes in a more physical way (including updating meshed point-cloud-based surfaces rather than Eulerian grids) to make this model more transferable, rather than re-calibrating it for each specific case.

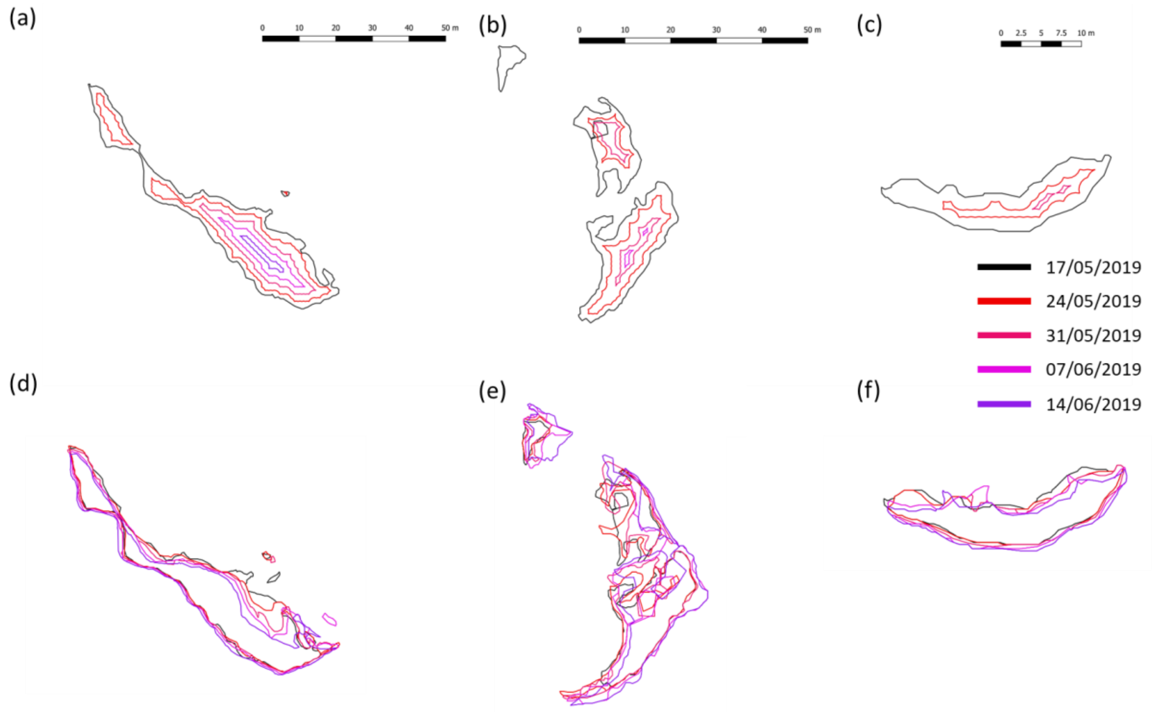

Figure R1: Initial (black) and updated cliff outlines for the Langtang cliffs at weekly time steps until full reburial of the cliffs when modelling changes in cliff dynamics following Buri et al., 2016b (a-c) and comparison with actual outlines (d-f).

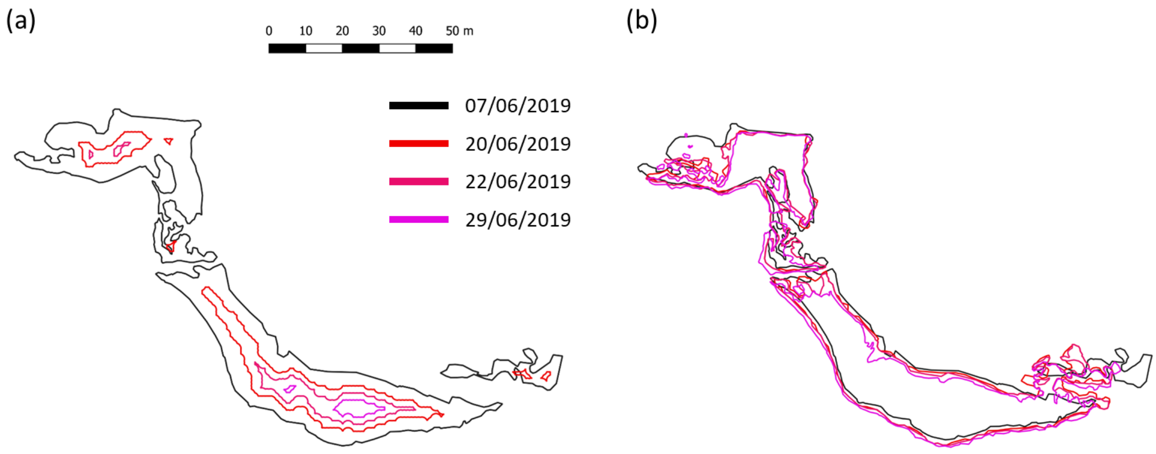

Figure R2: Initial (black) and updated cliff outlines for the 24K cliff at weekly time steps until full reburial of the cliff when modelling changes in cliff dynamics following Buri et al., 2016b (a) and comparison with actual outlines (b).

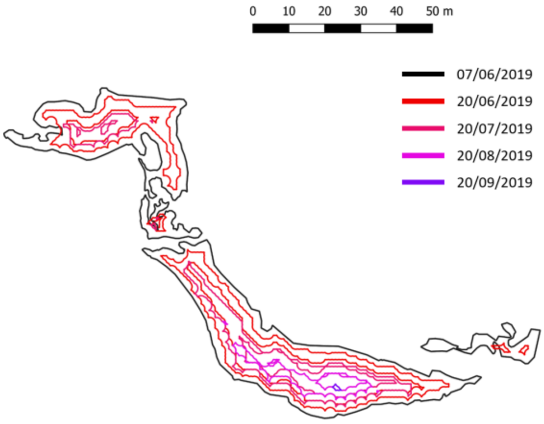

Figure R3: Initial (black) and updated cliff outlines for the 24K cliff at monthly time steps until full reburial of the cliff when modelling changes in cliff dynamics following Buri et al., 2016b.

We have also indicated the different points mentioned above directly in the paper by adding the following sentences in Sections 3.7. and 5.3:

L407-414: 'We used the static version of the model to focus on the contribution of the different energy-fluxes only, thus removing the influence of the modeled geometry updates. Indeed, the cliff dynamic model was designed to represent changes over long periods (entire melt season or monthly intervals), for which the melt rates are high relative to the model's spatial resolution. Due to the limited data available for the model development, the processes influencing the cliff dynamics (debris redistribution, additional melt from ponds) were also represented by rather simple parametrizations lumping together distinct physical processes. While this dynamic model is appropriate to estimate bulk changes over long periods (Buri et al., 2021; Buri and Pellicciotti, 2018; Buri et al., 2016b), we considered it to be too simple to represent all the complexity of changes occurring on a weekly time-scale, and therefore less reliable to understand the local energy-balance.'

L666-669: 'With the growing availability of high-quality multi-temporal observations of debris-covered glacier surfaces (Westoby et al., 2020; Sato et al., 2021), including from time-lapse photogrammetry, future model developments in this direction should attempt to reconcile mechanisms of cliff backwasting that are driven primarily by the cliff energy balance with debris redistribution processes and the influence of supraglacial hydrology. '

For the descriptiveness of the text regarding debris motion, we have streamlined the results description in section 4.2 and focused on the main findings (see detailed response to comment below).

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 have specified this point in the first paragraph of section 3.7:

*L403-404: '*We used the exact same parameters as Buri et al. (2016a) at both sites, and did not conduct any further calibration.*'*

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 have corrected them as follows:

*L442-447: '*The high temporal resolution of this dataset enables one to precisely estimate the total and spatially-averaged melt of ice cliffs, while the estimates from the Pléiades and UAV DEMs are usually 5 to 80% off depending on the method used to extract these values (Table S4, S5), due to the mixing of ice cliff and sub-debris melt contributions for less temporally-resolved data.*'*

*L695-697: '*Notably, 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 or annual DEMs. *'*

In addition we have included a comparison with a fully static model (no geometry updates from the time-lapse DEMs) and with taking only the initial cliff outlines for the melt calculation from the Pléiades and UAV DEMs (Table S4). We also included the results as a percentage relative to the measured melt from the time-lapse DEMs:

*'*Table S4: Average cliff daily melt rate (m w.e.day^{-1}) for each surveyed cliff from the flow-corrected Pléiades (for Langtang), UAV (for 24K), as well as from the measured and modeled melt from the time-lapse time series. The UAV and Pléiades melt was calculated perpendicular to the slope of the initial DEM, as described in Section 5.4. Melt values were then integrated

spatially (and temporally for the melt derived from the time-lapse), accounting for the cliffs' slope, to calculate the total volume losses. For the pre- and post-monsoon DEMs this spatial integration was conducted over 4 different domains: 1) the intersection of the cliff outlines in the pre and post-monsoon, 2) the pre-monsoon outlines only, 3) their union and 4) their union with a 4m buffer. The modeled melt was calculated using a fully static model and using the static model with the geometry update from the time-lapse DEMs. The total volumes were then normalized by the domain area, and by the mean cliff planimetric area for the time-lapse values.

Melt **(m w.e.day^{-1})**	Pre- and post-monsoon DEMs (Langtang: 2m Pléiades, 24K: 0.12m UAV)				Time-lapse DEMs		
	Intersection	Initial outlines	Union	Union + 4m buffer	Modeled (static)	Measured	Modeled
Langtang Cliff 1	0.020 -49%	0.019 -51%	0.017 -56%	0.017 -56%	0.031 -21%	0.039 0%	0.041 +5%
Langtang Cliff 2	0.041 -16%	0.042 -14%	0.037 -24%	0.033 -33%	0.037 -24%	0.049 0%	0.049 0%
Langtang Cliff 3	0.045 -4%	0.044 -6%	0.034 -28%	0.032 -32%	0.031 -34%	0.047 0%	0.046 -2%
24K Cliff	0.053 +4%	0.046 -10%	0.041 -20%	0.037 -27%	0.045 -12%	0.051 0%	0.053 +4%

'

We have also now based our calculations on the total melt over the whole cliffs (not just the area-averaged melt), as we felt that this was a fairer comparison, and more relevant for estimating the cliff contribution to glacier mass balance. We have added these estimates in a new table in the Supplementary Material:

'Table S5: Total cliff daily melt rate (m^3 w.e.day^{-1}) for each surveyed cliff from the flow-corrected Pléiades (for Langtang), UAV (for 24K), as well as from the measured and modeled melt from the time-lapse time series. The UAV and Pléiades melt was calculated perpendicular to the slope of the initial DEM, as described in Section 5.4. Melt values were then integrated spatially (and temporally for the melt derived from the time-lapse), accounting for the cliffs' slope, to calculate the total volume losses. For the pre- and post-monsoon DEMs this spatial integration was conducted over 4 different domains: 1) the intersection of the cliff outlines in the pre and post-monsoon, 2) the pre-monsoon outlines only, 3) their union and 4) their union with a 4m buffer. The modeled melt was calculated using a fully static model and using the static model with the geometry update from the time-lapse DEMs.

Melt **(m^3 w.e.day^{-1})**	Pre- and post-monsoon DEMs (Langtang: 2m Pléiades, 24K: 0.12m UAV)	Time-lapse DEMs

	Intersection	Initial outlines	Union	Union + 4m buffer	Modeled (static)	Measured	Modeled
Langtang Cliff 1	0.4 -90%	2.0 -49%	3.7 -5%	5.3 +36%	5.3 +36%	3.9 0%	4.2 +8%
Langtang Cliff 2	2.5 -91%	23.5 -15%	36.5 +33%	47.0 +71%	20.3 -26%	27.5 0%	27.7 +1%
Langtang Cliff 3	23.3 -39%	51.2 +34%	68.5 +79%	78.7 +106%	35.4 -7%	38.2 0%	36.9 -3%
24K Cliff	51.9 -47%	118.5 -8%	172.2 +78%	225.9 +131%	128.8 +31%	98.0 0%	102.6 +5%

’

As these are particularly relevant findings, we also included the following related statements in the discussion:

L638-641: ‘Ultimately, not accounting for these geometry changes results in 5 to 80% discrepancies in terms of total and area-weighted cliff melt (Table S4, S5), which has important consequences for the estimation of cliff contribution at the glacier scale, in case the overall cliff area would consistently increase or decrease. ‘

L661-663: ‘We could also show that using a fully static version of the model generally resulted in better melt estimates than when deriving these from pre- and post-monsoon flow corrected Pléiades or UAV DEMs (Table S4, S5). ‘

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 initial manuscript and we have now corrected this. We had initially taken the mean+/-STD of cliff melt and divided it by the mean+/-STD of sub-debris melt. However, for clarity we now just compared directly the average sub-debris melt and the average cliff melt. We clarified this and updated the text accordingly:

*L23-26: ‘*We find that ice cliff melt varies considerably throughout the melt season, with maximum melt rates of 5 to 8 cm.day^{-1}, and their average melt rates are 11-14 (Langtang) and 4.5 (24K) times higher than the surrounding debris-covered ice. *’*

*L435-440: ‘*The mean and standard deviation of the sub-debris melt calculated from the flow-corrected Pléiades and UAV DEMs and for snow- and cliff-free (including a 5 m buffer around the initial and final cliff outlines) zones were -0.3 +/- 0.4 cm.day^{-1} for Langtang and -1.1 +/- 0.5 cm.day^{-1} for 24K, 11-14 (resp. 4.5) times less than the average cliff melt measured from the time-lapse DEMs (Fig. 6). ’

These values were also updated in the discussion:

*L567-568: '*The studied cliffs displayed melt rates at 4.5 times higher than the surrounding debris-covered ice on 24K and 11-14 times higher on Langtang*'*

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 have now streamlined the sections mentioned and shortened the text where needed. Specifically we have:

1) *Cut some of introduction sentences and especially condensed the 4th and 5th paragraphs*
2) *Moved a large part of section 3.2 to Supplementary Material*
3) *Removed and reformulated large parts of Section 3.5*
4) *Streamlined the first paragraph of Section 3.7 and removed all the repeated content from the introduction*
5) *Focused on the most important results for each cliff in section 4.2 and made the links between meteorology and dynamics clearer in sections 4.2.1, 4.2.2 and 4.2.3.*
6) *Condensed and reformulated Section 5.3.*

Specific comments:

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

We have removed the bold fonts, except for the overall objective of the study and the titles of the bullet points in section 5.2.

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 calculated in Section 3.5. We have updated the values based on the updates in Section 3.5 and changed the wording in the text to make it clearer:

*L20-21: '*We derive weekly flow-corrected DEMs of the glacier surface with a maximum bias of +/- 0.2 m for Langtang Glacier and +/- 0.05 m for 24K Glacier*'*

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

This refers to L596-602 of the discussion. We have now specified it in the text:

L601-602: 'This melt reduction effect was generally comprised between 1-3 cm.day^{-1} relative to the locations on the cliffs that remained debris-free.'

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

Agreed. Changed to:

L38-39: 'Similarly to supraglacial ponds, the surface of ice cliffs is directly exposed to energy fluxes from the atmosphere, these cliffs therefore act as 'melt hotspots' relative to the surrounding debris-covered ice'

L45: consider adding "for specific locations".

Agreed. We have added it (L47-48)

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

This was the slope of the whole glacier, but since the focus is on the characteristics of the debris-covered portions of the glaciers, we have now indicated the slope of the debris-covered parts instead:

L120-121: 'The debris-covered area of 24K is much steeper (9.8°) than for Langtang (3.4°)'

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

We have removed it (L135).

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

We detailed the results of the vertical accuracy in the following sentence:

L198-200: 'For both sites we estimated the vertical uncertainty as the standard deviation of the DEM difference over off-glacier stable terrain (Mishra et al., 2021), 0.53 m for Langtang and 0.50 m for 24K. '

And we now put a reference to section 3.3 where we give the velocity uncertainty obtained from the normalized median absolute deviation of the surface displacement over the off-glacier terrain:

L194-196:'After the initial co-registration of the UAV DEMs, there remained some non-linear distortions (tilts) that were removed using additional natural off-glacier control points (boulders) on both sides of the glacier identified in the June flight to rerun the bundle adjustment of the October flight, which improved the co-registration (Section 3.3).'

L281-282: 'We estimated the surface velocity uncertainty as the normalized median absolute deviation of its x and y components over off-glacier terrain, equal to 0.84 m (0.6 cm.day^{-1}) for Langtang and 0.35 m (0.3 cm.day^{-1}) for 24K over the full study period. '

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 now put the mean to 0, and accounted for the change in the corresponding calculations. The difference was small enough that it was not visible in the corresponding figures. We updated the text as follows:

L198-200: 'For both sites we estimated the vertical uncertainty as the standard deviation of the DEM difference over off-glacier stable terrain (Mishra et al., 2021), 0.53 m for Langtang and 0.50 m for 24K. '

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 specified this point in the text:

L300-302: 'We conservatively assumed the melt distance uncertainty to be equal to the uncertainty in elevation change as this removes the dependence on the terrain aspect and slope. '

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 have now specified these elements in the text:

L343-354: '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} (all in m.day^{-1}):

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

Where:

$$\sigma_{Slope} = \sigma_{xy}\tan(\alpha) + \frac{d\alpha}{\cos^2\alpha}u_s \approx u_s d\alpha \quad (3)$$

Where α is the mean glacier slope in the survey domain, and u_s the mean velocity. For the uncertainty on the slope correction, we assumed a $d\alpha = 2° = 0.03\ rad$ uncertainty in the slope angle, which results in $\sigma_{Slope} = 0.03$ cm.day^{-1} for Langtang and 0.06 cm.day^{-1} for 24K. 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.'

L413: missing figure number

Thanks for spotting this, we corrected it.

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

This resulted in increased melt rates at the location of the expansion. We have now specified this:

L474-475 'Langtang Cliff 1 was a relatively small (5-10 m tall, 30-40 m wide) north-facing cliff (Fig. 8, 9). In July, it expanded a few meters to the east, resulting in enhanced melt rates at this location, but the new section got re-buried relatively quickly in August.'

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

We have now specified the melt values relative to the melt for portions of the cliffs that remained debris free, and updated the values accordingly in the abstract:

L596-607: '

- **Melt reduction from patchy debris**: 40-80% lower measured melt values evident at the foot of the Langtang Cliffs 2 and 3 (Fig. 10, 12, S5 d-f, S7 a-c) were likely caused by the active reburial of these sections of the cliffs during shorter time intervals than the 2-3 week period over which melt was integrated. This influence of debris was also visible on the 24K cliff where the two transects which had the higher proportion of 'dirty' ice, and where it was most difficult to outline the ice cliff relative to the patchy debris, experienced reduced melt. At this location, the debris on the ice cliff was thick enough to reduce melt (Fig. 14-15, S9). This melt reduction effect generally accounted for 1-3 cm.day^{-1} relative to the locations on the cliffs that remained debris-free.
- **Melt enhancement from thin dust layers**: The 10-60% higher melt values on the upper and shallower cliff slopes that had recently become free of debris of Langtang Cliffs 2-3 (Fig. S5 d-f, S7 a-f) were likely caused by lower albedo values due a higher concentration of dust particles at the surface (Fyffe et al., 2020). Similarly, transect 3 of the 24K cliff was affected by small debris clasts and thin debris (Fig. 14), but these did not reduce melt and more likely led to higher melt rates due to lower albedo values.'

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 have added a sentence acknowledging these limitations specifically:

L656-657: 'Despite the small number of cliffs covered, their similarity in aspect and the relatively short duration of the observations (one melt season), this study has highlighted the variability in ice cliff characteristics and behaviors. '

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

We have replaced it:

L693: 'This study considerably improved our understanding of ice cliff evolution '

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.

Thanks. We have now removed most of the bullet points and rearranged the introduction to outline these aspects:

L693-721: 'This study considerably improved our understanding of ice cliff evolution by using terrestrial time-lapse photogrammetry to quantify the weekly evolution of four ice cliffs on two climatically contrasting Himalayan debris-covered glaciers. Notably, 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 or annual DEMs. Prior to our work, cliffs had been observed only at the beginning and end of the melt season (because of logistical and field challenges), but never during this period, when most of the ablation occurs.

We found that the sub-seasonal variability in cliff melt was high, and was driven mainly by shortwave radiation, while air temperature was the determining factor for the sign of the net longwave contribution. Overall, the modeled melt agreed with the observations. On the other hand, the interaction of the cliffs with surrounding debris cover was found to be particularly crucial, and increased the spatial variability of the cliff melt by causing very strong changes in the cliff geometry. At the cliff surface, it had two main effects:

- The presence of small clasts or thin layers of dust reduced the cliff albedo (resulting in increased melt). Liquid precipitation events were effective at 'washing' this thin debris cover from the cliff surface and increasing its albedo, whilst snow events had a similar effect.
- The presence of slightly thicker, often patchy debris at the cliff surface and the active reburial of parts of the cliffs reduced melt via the debris insulating effect.

Ultimately, our results confirmed that the connectivity between ice cliffs and supraglacial hydrology (streams, ponds) exerts an important control on rates and patterns of cliff expansion and reburial, and that the relevant processes and feedbacks need to be accounted for in contemporary ice cliff energy balance models to better constrain cliff melt and the long-term surface evolution of debris-covered glaciers.'

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.

We have checked these values with those from Buri et al. (2016a) and Buri et al. (2018) and have found the values of the fluxes to be consistent - the mean daily incoming shortwave is higher on Langtang than on Lirung, but not by an order of magnitude. We are therefore confident that these values correspond to the caption 'Average daily incoming shortwave and longwave radiations', in ($W.m^{-2}.day^{-1}$).

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:

L486-487: 'The large boulder standing on top of the cliff did not seem to influence the melt (Fig. 8d-f). '

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 have changed this 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 have now specified it in the captions:

L967: '(d) Modeled net energy fluxes represented by the central value of each period. '

We however kept the fluxes as 'melt contribution (m/day)' to keep the link with the left panels.

Response to reviewer 2

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

This submission presents measured and modelled melt from four ice cliffs located on two Himalayan glaciers with contrasting geomorphological characteristics. Measured melt is derived from terrestrial timelapse photogrammetry and modelled melt is derived from a surface energy balance model partly developed by the co-authors. The methods are extensively described and largely based on previous work and are sound. There is careful attention given to quantifying uncertainty and I can find no fault in that regard. The results show some interesting spatial and temporal variability in melt patterns and provide a window into the dynamics of the ablation season, which is otherwise all too often obscured by cloud and inaccessible for field observations. Interpretation of the key controls of this variability is convincing.

We would like to thank Reviewer 2 for the thorough assessment of our manuscript and for their very constructive comments. Specifically, we have:

1) Added explanations to the text as to why we did not use the dynamic ice cliff model and prepared additional model runs to show the comparison with the current implementation.

2) Streamlined the manuscript by shifting some of the methods and results sections to the supplementary material.

3) Added some considerations about using the time-lapse images to parametrize cliff albedo variability.

It is notable that the premise of the work is to be able to quantify and characterise the evolution of these ice cliffs during their most dynamic period, yet the modelling that is to shed insight into the energy balance is static. There is one line of justification for this (373-374) but it could do with much better justification, especially given the most recently published work of the co-authors describes and uses the dynamic model in a similar vein and some of the future work suggested in later sections has already been realised.

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 debris 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, and at the temporal scale of this study (weekly geometry updates!) - 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, the lumped process representation in the dynamic model discussed above, and the very high temporal resolution at which the geometry needs to be updated. 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 therefore conducted additional tests with the cliff dynamic model to show how it behaved in this particular framework (Fig. R1, R2). We ran the cliff dynamic model at 1m resolution for all four studied cliffs (tests at 1.2m resolution showed that the spatial resolution had a limited influence on the model results), updating the geometry every time there was a new time-lapse DEM (~weekly intervals). We used a slope threshold of 35° below which the cliff could be reburied and used otherwise the same parameters as the ones used in the study by Buri et al. (2016b). In all cases the cliffs shrink rapidly and disappear entirely within one month (Fig. R1, R2). We additionally ran the dynamic model with a monthly temporal resolution for the 24K cliffs, which led to the cliff shrinking, but much slower than with weekly geometry updates, as it survived three months longer (Fig. R3).

This shows that the slope threshold is not the only parameter responsible for this unexpected evolution of the cliffs, and that this is at least partly due to the high temporal frequency of the geometry updates, for which the spatial resolution is much higher than the melt distance. The dynamic model was not designed for such cases, and therefore struggled to maintain the cliffs. It would therefore require to be entirely re-calibrated to be relevant here, which is beyond the scope of this study as it does not contribute particularly to our research questions. Furthermore, based on the availability of this data, we believe that further efforts in this direction should rather aim to represent the geometry updates and debris processes in a more physical way to make this model more transferable, rather than re-calibrating it for each specific case.

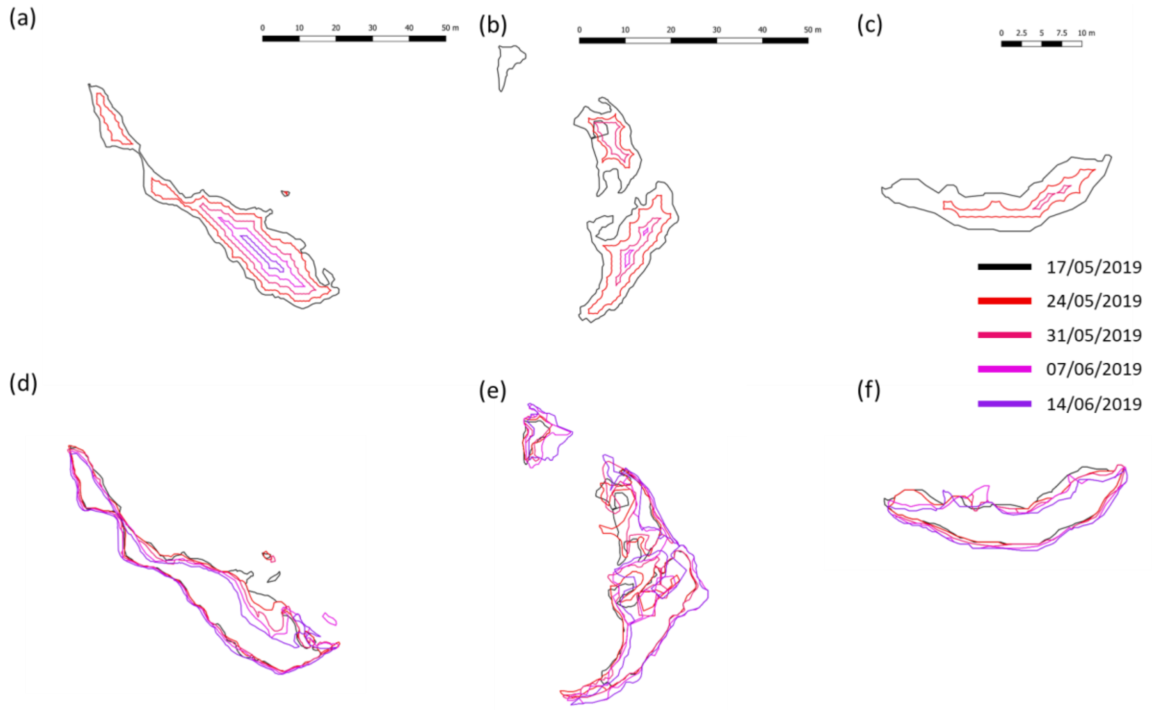

Figure R1: Initial (black) and updated cliff outlines for the Langtang cliffs at weekly time steps until full reburial of the cliffs when modelling changes in cliff dynamics following Buri et al., 2016b (a-c) and comparison with actual outlines (d-f).

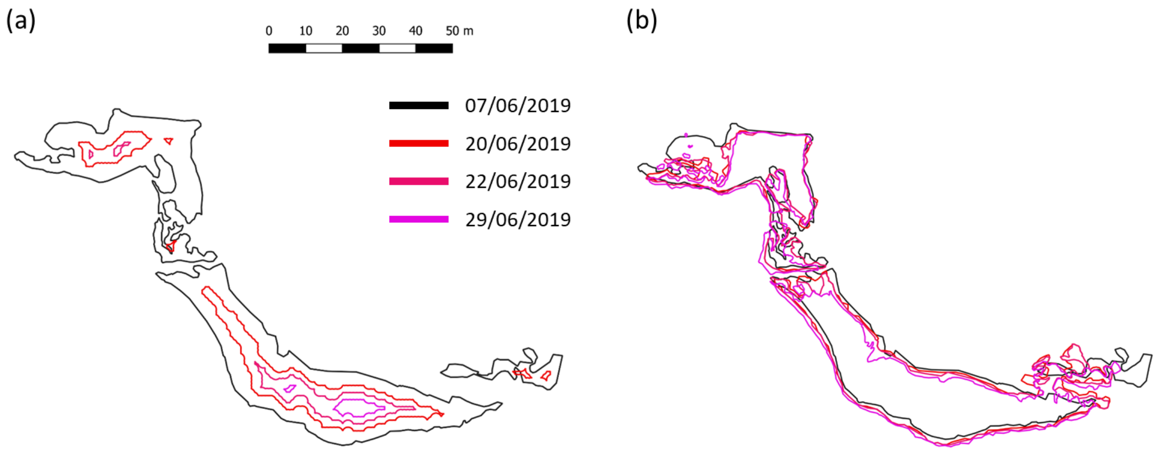

Figure R2: Initial (black) and updated cliff outlines for the 24K cliff at weekly time steps until full reburial of the cliff when modelling changes in cliff dynamics following Buri et al., 2016b (a) and comparison with actual outlines (b).

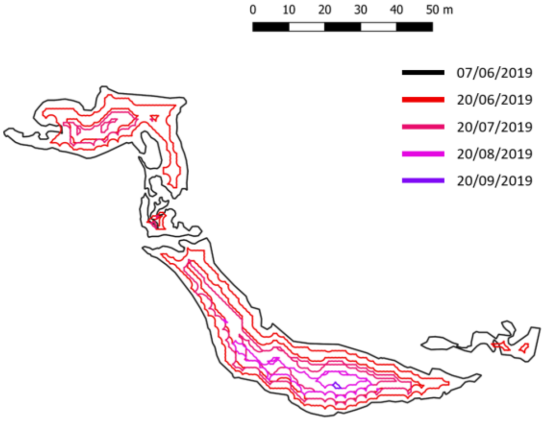

Figure R3: Initial (black) and updated cliff outlines for the 24K cliff at monthly time steps until full reburial of the cliff when modelling changes in cliff dynamics following Buri et al., 2016b.

We have also indicated the different points mentioned above directly in the paper by adding the following sentences in Sections 3.7. and 5.3:

L407-414: 'We used the static version of the model to focus on the contribution of the different energy-fluxes only, thus removing the influence of the modeled geometry updates. Indeed, the cliff dynamic model was designed to represent changes over long periods (entire melt season or monthly intervals), for which the melt rates are high relative to the model's spatial resolution. Due to the limited data available for the model development, the processes influencing the cliff dynamics (debris redistribution, additional melt from ponds) were also represented by rather simple parametrizations lumping together distinct physical processes. While this dynamic model was appropriate to provide bulk changes over long periods (Buri et al., 2021; Buri and Pellicciotti, 2018; Buri et al., 2016b), we considered it to be too simple to represent all the complexity of changes occurring on a weekly time-scale, and therefore less reliable to understand the local energy-balance.'

L666-669: 'With the growing availability of high-quality multi-temporal observations of debris-covered glacier surfaces (Westoby et al., 2020; Sato et al., 2021), including from time-lapse photogrammetry, future model developments in this direction should attempt to reconcile mechanisms of cliff backwasting that are driven primarily by the cliff energy balance with debris redistribution processes and the influence of supraglacial hydrology. '

There is also a great deal of attention given to the important role that albedo plays in controlling melt, and the fact that it is dealt with as a constant in the modelling, but on the other hand the timelapse images are normalised to account for illumination variability and cliff brightness is derived. It strikes me it would be a small step to use these data more explicitly to drive a time-varying albedo parameterisation that would bring the modelled and measured data more closely together. I don't suggest that the authors should implement that as a revision, but maybe the concept (and the challenges that might lie therein) could be included within the discussion of future work.

This is an excellent point that we have added to Section 5.3 of the discussion as a further avenue to explore. There are indeed already studies that have looked into estimating albedo from RGB images (e.g. Corripio, 2004; Ayala et al., 2016; Burger et al., 2018):

L680-682: 'A step forward in the representation of cliff albedo variability could also be to extract it from the brightness observations of the time-lapse images (Corripio, 2004) and the precipitation patterns, although the difficulty here will be the transferability of such a relationship from glacier to glacier.'

Lastly, the manuscript is very lengthy, and could be streamlined in places. An example is in the description of the SfM setup, which is based on previous published work - a short summary of the departures from this previous work rather than extensive description should suffice. Lines 70-99 could be distilled into a few lines. Lines 618-625 seem to offer little other than some generic thought. A re-read of the manuscript with a critical eye for what is and what is not required, and whether any material (methods mostly) could go into Supplementary, would help to keep the reader's attention and lead them to the take-home message more efficiently.

We thank the reviewer for their comments on this. We have now streamlined the sections mentioned and shortened the text where needed. Specifically we have:

7) *Cut some of introduction sentences and especially condensed the 4th and 5th paragraphs*
8) *Moved a large part of section 3.2 to Supplementary Material*
9) *Removed and reformulated parts of Section 3.5*
10) *Streamlined the first paragraph of Section 3.7 and removed all the repeated content from the introduction*
11) *Focused on the most important results for each cliff in section 4.2 and made the links between meteorology and dynamics clearer in sections 4.2.1, 4.2.2 and 4.2.3.*
12) *Condensed and reformulated Section 5.3.*

The manuscript is otherwise very well written. I have picked up some small ambiguities or points for clarification that follow here:

line20: is that horizontal or vertical uncertainty?

Vertical. We have now specified it and modified the wording to make it more explicit:

L20-21: 'We derive weekly flow-corrected DEMs of the glacier surface with a maximum vertical bias of +/- 0.2 m for Langtang Glacier and +/- 0.05 m for 24K Glacier '

line28: variability across space or through time? On an individual cliff, or between sites?

We have specified:

*L29: '*Ultimately, our observations show a strong spatio-temporal variability in cliff area at each site'

line40: I prefer not to suggest that ice cliffs enhance melt - rather it's the debris that is supressing it (unless thin of course). Maybe exceed is a better choice of word.

This is in line with previous research on ice cliff enhancement factors (Brun et al., 2018; Miles et al., 2022). We have kept 'enhance' but added 'relative to their surrounding debris-covered area' next to it to make sure that there is no misinterpretation of the wording:

*L42-43: '*ice cliffs enhance melt relative to their surrounding debris-covered area by a factor '

line43: what is an 'advanced' energy balance model in this context?

We have removed this adjective.

line77: are ponds a process? Maybe pond filling and drainage? Similarly for streams. Maybe you mean down-cutting?

Yes. We modified as suggested:

*L80-83: '*This high variability can be explained by the strong influence of local processes such as pond undercutting, filling and drainage (Kraaijenbrink et al., 2016; Watson et al., 2017b), stream undercutting (Mölg et al., 2020) and debris redistribution (Moore, 2018; Westoby et al., 2020).'

line121: directions of the compass don't need capitalising

We have removed the capital letters throughout the text.

line138: add 'satellite' for those not familiar with Pleiades

Agreed (L143).

line164-165: this is presumably important for the modelling? Maybe state that if so?

The model used does not account for lake effects. We kept this section as it is.

line195: 'identified in the June flight'

Changed as suggested (L194)

line223: 'As an initial estimate, we used the values provided by...'

Changed as suggested (Now in Section S1 of the supplementary material)

line225: didn't change by more than five centimetres in which direction? Not sure I follow. Do you mean five degrees?

Here we mean the height of the camera along the mast. We stated this explicitly in the text, now in Section S1 of the supplementary material:

'The vertical position of the camera along the mast did not change by more than five centimeters'

line428 and elsewhere: why the need to put text in bold? The aim being in bold made sense (perhaps) but not the rest…

We have removed the bold parts (except for the aim and the title of the bullet points in section 5.1).

Table 1: caption needs attention (repeats that from the figure directly above)

Thanks for spotting this. We have changed this:

L931: 'Table 1: Pre- and post-monsoon remote sensing observations from UAV and satellite surveys.'

Figure 8: clarify which period the cliff outline relates to (start or end of observation period)?

Good point. We have now specified this in all captions where necessary (they correspond to the start of the period):

L961: 'Cliff outlines corresponding to the start of the period are shown in black'

Figure 16: this is nicely presented, but the integration of it into the text is poor. It also represents one possible pathway of evolution over a discrete (set) period of a month, showing two points in time. This doesn't fit well with the rest of the study that tells the reader there is great spatial and temporal variability in behaviour, and it has been characterised at fine temporal resolution for the first time. The figure either needs better explanation in the text, revising (to really show the new information gleaned from this study), or removing.

The purpose of this figure was to indicate the different mechanisms outlined in the study, which we have now explicitly stated in the first paragraph of the discussion. We have expanded on the explanation of the figure in the text and presented it as one possible pathway of evolution:

L563-565: 'The different mechanisms outlined here are indicated in Figure 16, which represents one possible evolution pathway for a set of idealized cliffs'

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