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 read of our manuscript and for their very constructive comments. Specifically, we will:

1) *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.*
2) *Streamline the manuscript by shifting some of the methods and results sections to the supplementary material.*

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 point, 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 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 this 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; van Woerkom et al., 2019; Anderson and Anderson, 2018; 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.

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 will add 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. Ayala et al., 2016; Burger et al., 2018).

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 will make sure to streamline the sections mentioned and shorten the text where needed. 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.*
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.*

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 will specify it here.

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

We will specify '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 would keep 'enhance' but will put 'relative to their surrounding debris-covered area' next to it to make sure that there is no misinterpretation of the wording.

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

We will remove this adjective.

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

Yes. Will modify as suggested.

line121: directions of the compass don't need capitalising

We will remove the capital letters.

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

Agreed.

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

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

line190: 'identified in the June flight'

Will change as suggested.

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

Will change as suggested.

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 will state this explicitly in the text.

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

We will remove the bold parts (except for the aim).

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

Thanks for spotting this. We will make sure to change this.

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

Good point. We will specify this (they correspond to the start of the period).

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.

We will expand on the explanation of the figure in the text and present it as one possible pathway of evolution. The purpose of this figure was to indicate the different mechanisms outlined in the study, which we will explicitly state in the first paragraph of the discussion.

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