Editorial comments:

L55: Move definition of NPS to here (first usage)

"NPS" was removed from here and several other locations as it is not frequently used in the text and not needed to distinguish the photogrammetry DEM presented.

L67: Add degrees to coordinates

Changed

L72: Better: 'typically tens of cm (but <50cm) thick'?

Made this suggested change.

L95: 'existence-slopes': add spaces and change hyphen to em-dash

Changed

L118: Change hyphen in 4 - 8 to en-dash and remove spaces.

Changed.

L127: vise > vice

Changed.

L220: inferring > 'implying', or 'for which we infer'

Changed to implying.

L251: In-situ > In situ

Changed.

Fig 1b: The stream gauge symbols aren't legible in panel B

Fig 1 has been re-rendered with larger stream gauge symbols in panel B.

Fig 3: State in caption what black point and arrow represent in Inset (location of a-d?)

Indicated now in caption.

Fig 6: Change superscript zeroes to degree symbol throughout figure

Changed "$^{0}$" to "°"

Marin Kneib Comments:

Dear authors,
Thank you for the revision of your manuscript and your answer to my comments. I find your article to be of high quality and scientific relevance. I still had a few minor points (see below) that I feel should be addressed before publication. Line numbers correspond to the document with tracked changes.
Best regards,
Marin

L32: 'exposed ice in ice cliffs' sounds a bit weird – 'exposed ice at the surface of ice cliffs' would be a better term.

Thank you for the suggestion. Changed to "Ice exposed at the surface of ice cliffs."

L38: No dash between 'Changri' and 'Nup', just a space.

Fixed

L34-39: Nice inclusion, but as it is it's all a bit jumbled together with no logical order. It would make sense to distinguish modeling from purely observational studies.

True, the writing needed some cleaning up here. I reworked these lines into the following:

"Ice exposed at the surface of ice cliffs contributes significantly to melt on debris covered glacier surfaces \citep{sakai_distribution_2002,buri_supraglacial_2021,anderson_debris_2021}. \cite{miles_controls_2022} determined that melt rates on ice cliffs are consistently 2-3$\times$ melt rates on clean glacier ice under similar conditions. As a result, ice cliffs which cover typically $\sim$10\% of the area of debris-covered glacier tongues contribute typically $\sim$20-25\% of the melt rates over that same area, for glaciers in Alaska (\cite{anderson_debris_2021}: 12\% coverage, 26\% melt rates on Kennicott Glacier) and Nepal (\cite{brun_ice_2018}, 7-8\% area coverage, 24$\pm$5\% melt rates on Changri Nup Glacier). Ice cliff melt is also significant on glacier and catchment-wide scales; \cite{buri_supraglacial_2021} found in a modeling study that ice cliffs account for 17$\pm$4\% of total glacier melt across the Langtang Glacier catchment."

Which makes a distinction between the scales at which each study is assessing the importance of ice cliffs. I also added a paragraph break here to make for easier reading.

L46: Well I guess then it cannot be attributed to ice dynamics only but rather to a mixture of both contributions.

I agree. In this sentence, however, I'm referring to the conclusions drawn by the authors of that study. I have clarified this in the text.

Figure 1: Missing parenthesis at the end of the caption.

Fixed.

L68: there seems to be a missing connector between 'erode' and 'transport'

Yes – inserted "and."

L88: it is not clear here if these melt rates are for the whole glacier or just for the debris-covered part.

Just for the debris covered part (lowermost 8.5 km by length). I've rephrased the sentence based on the confusion: "Anderson et al. (2021a) estimated that 20% of Kennicott Glacier's surface is debris-covered; on the lowermost 8.5 km by length of Kennicott Glacier they determined that 26% of melt was attributed to ice cliffs covering 12% of the surface in summer 2011."

L119 & 122: if 'a threshold minimum steepest slope value was tested…' (l118), then it is more a mapping 'calibration' than 'validation'. As it is, it would be appropriate to split the manual outlines between a calibration and validation dataset to account for potential biases/overfitting issues, and to adequately quantify uncertainties of the mapping.

I do not think that there is a risk of overfitting due to the fact that the only parameter which is varied is the threshold minimum steepest slope value. Given that ice cliffs are by definition steep sections of glacier surface it makes sense that we are starting from a position of over-prediction when selecting slope angles > 30 degrees. All ice cliffs are steeper than 30 degrees (Figure 3f, "True Positive Loss"/"False Negative"), but not all pixels of the glacier surface greater than 30 degrees are ice cliff (but the probability that they are increases for higher slope angles). You make a good point that "calibration" may be more accurate than "validation." However I contend that the results of this calibration give us a good handle on mapping uncertainties through the quantification of False Positive and False Negative identifications.

L123-124: What metric did you use for the calibration?

See response to L202-207 comment. In addition to False Negative-False Positive balance and True Positive maximum, we are now quantifying F1 Score as suggested.

L152: ‘himalayan’ can be removed as ice sails can be found elsewhere. If needed, you can specify ‘ice sails in the Karakorum’ outside of the parenthesis. Note that the Himalayas do not encompass the Karakorum.

Removed “Himalayan”

L178-181: Thanks for your efforts in trying to quantify the uncertainties in the different angles. While 2 cliffs is better than none, it would strengthen the analysis if you could conduct this exercise for ~10 cliffs to be more representative.

We argue that extending this exercise as suggested is not necessary. Our results suggest that the variability between sampled cliffs is in general greater than the variability in results from sampling different locations across individual cliffs (compare Figure 6d-e to Figure S2d). Furthermore, our sampling of a large number of ice cliff-ramp systems is sure to capture much variability in terms of stream maturity, slope aspect, debris remobilization, and other possible confounding factors.

L202-207: it is common for this type of exercise to use a metric that combines TP, FP & FN (as there will in any case be an overwhelming number of TN) such as the F1- score. It would be useful to indicate this score here, as it will enable a better comparison with other methods.

Thank you for this comment! I was not familiar with the F1 score, and I was labelling what is in fact the “False Negative” with my own name the “True Positive Loss.” I’ve re labelled that in the figure and through the text. I’ve calculated the F1 score (scaled to area), and it is not surprising that it agrees very closely with the results of my analysis, since my analysis was informed using all the inputs used to calculate an F1 score—TP, FP, and FN (previously referred to as “True Positive Loss” in previous drafts of this manuscript). The maximum F1 of 0.96 occurs with a minimum steepest slope angle of 51.7 degrees, which is in agreement with the maximum true positive value (93%) and 0.1 degrees difference from the value of 51.8 degrees that I elected based on matching “False Positive” to “False Negative” (previously “True Positive Loss”). This comment helps me to gain more confidence in our analysis while providing more standardized language to use. We now include the F1 score in the methods, results, and in an updated Figure 3.

Figure S3: Just to be sure, (c) shows the mean aspect of individual cliffs? This needs to be specified somewhere. I actually find it interesting to not see any obvious aspect-bias to north-facing cliffs in this figure. This could be an argument in favour of L446-447, which at the moment is only based on one particular observation (so quite weak argument) – could you perhaps look at the aspect distribution of stream-influenced and non stream-influenced cliffs to check this?

Yes, this is true for the other values as well. For clarity we added “Ice cliff metrics represent median values for ice cliff shapes, not raw pixel values.”
This is an interesting idea to investigate, but goes a bit beyond the effort of this manuscript. The lack of aspect bias could also be related in large part to the crevasse-originated ice cliffs (Figure 13c), which would explain the ENE – WSW symmetry (e.g. a crevasse field oriented NNW – SSE). Again, this larger question of controls on ice cliff slope aspect goes a bit beyond the scope of this paper, so we will keep this in the supplementary material.

L227-235: this is a very nice addition!

Thank you!

Figure 10: dot missing in the caption before (b-c)

Fixed.

L388: link missing – maybe ‘Such abandoned stream channel…’? L419: dot missing

Reworked and fixed.