The manuscript *Stream hydrology controls on ice cliff generation, evolution, and survival on debris-covered glaciers* by Peterson and co-authors presents a detailed geomorphological study of supraglacial streams on Kennicott Glacier, and their influence on the ice cliff characteristics and distribution at the surface of debris-covered glaciers. It combines detailed field observations, photogrammetric surveys and introduces a model representation of stream incision at the base of ice cliffs.

I found this manuscript particularly well written and interesting, as it presents a detailed analysis of the patterns and controls of ice cliffs on a glacier with a high concentration of supraglacial streams, a dense cliff distribution and relatively thin debris, which contrasts from previous studies which usually focused on glaciers with thicker debris with mostly pond-influenced cliffs. I really liked the idea of the geomorphic model introduced here, which is novel and links nicely cliff backwasting, stream incision and sediment transport. I still had some specific concerns related to some of the methods, and more general comments related to the discussion, that I hope the authors can address.

General comments

<u>Cliff mapping:</u> The approach described L68-73 is a bit vague and uses a lot of parameters that have unjustified values. There is a variety of methods that have been used to map ice cliffs in a (semi-)automated way from DEMs (Herreid and Pellicciotti, 2018) or optical images (Kneib et al., 2020; Kraaijenbrink et al., 2016; Anderson et al., 2021), and the choice of a different approach would therefore need to be well justified. As it is, it is very vague and unclear. Why not simply use the orthoimage? Was there no manual refinement of the outlines? What is the uncertainty of the mapping? Is there no effect from the very high spatial resolution that could lead to influence from large boulders for example? This is especially critical as 1/ it is not clear how much the mapping influences the analysis presented here and 2/ there is no discussion whatsoever of this automated mapping method. At the very least, it would be useful to compare the outlines of this approach with those from Anderson et al. (2021).

<u>Choice of cliff profiles:</u> The location of the profiles perpendicular to the cliffs appears to be pretty random. It would be useful to make a more systematic analysis for at least a few cliffs on how much the choice of the location of these profiles matters. I once tracked ice cliffs with time-lapse photogrammetry on a glacier in SE Tibet and made similar profiles, but realized that depending on the location of the profile along the cliff and its connection to the supraglacial stream, this could really change the surface profile and its evolution (Kneib et al., 2022).

<u>Estimating thermo-erosional undercutting:</u> I found the paragraph on the estimation of debris transport by the streams very interesting. However, it seems to me that the missing link in your conceptual model would be link between stream incision rate and ramp angle/characteristics. Would there not be a way to estimate this stream incision rate relative to the debris-covered ice? This appears to be quite a crucial element as I suspect that if the glacier had been more thickly debris-covered, the incision would have been too important and could have led to a cut-and-closure mechanism (Gulley et al., 2019).

<u>Effects of dynamics on ice cliff behavior:</u> I found the supplementary text S1 very interesting and would recommend including it in the main text. I also looked in a previous study at the influence of glacier speed-up on cliff distribution and characteristics, and there the speed-up led on the contrary to more stream-influenced cliffs, most likely as the changes in glacier dynamics led to more water being rerouted at the surface (Kneib et al., 2023). A link with glacier surges could also be relevant for this discussion (Glasser et al., 2022).

Line-by-line comments

L25: It would be good in this paragraph to add a few numbers on melt enhancement by ice cliffs (e.g. Miles et al., 2022), as well as on ice cliff density at the surface of glaciers. Specific studies looking into the role of ice cliffs on the debris-cover anomaly at the glacier scale could also be mentioned (e.g. Brun et al., 2018; Zhao et al., 2023)

L30: Not sure the term 'models of retreat' is appropriate. Would rather suggest 'debris-covered glacier melt models'.

L31: I would also note the work by Watson et al., 2016; 2017 and Westoby et al., 2020.

L33: another useful reference here would be the work by Bartlett et al. (2021).

L36: Remove 'Scott' Watson's first name in reference. This is a recurring error in the text. A similar problem occurs for the Garner et al. references.

L46: Here it is noteworthy that so far, most studies had so far focused on glaciers with thick debris cover where the main control on ice cliff evolution comes from the supraglacial ponds (Langtang, Khumbu, Lirung, Ngozumpa, Miage…). Thus the need for detailed work on the influence of stream hydrology.

Figure 1: Specify somewhere that the arrow indicates north (or put a N above). Also for the other figures. DEM and NPS acronyms need to be defined. Since the outline has been changed, could you also show the new outline?

L53: I find that a few details are lacking in this description. Especially related to debris thickness and flow velocities, since these are referred to later on.

L54: Could you add the coordinates of the glacier?

L57: specify melt rates 'of the debris-covered area'.

L63: specify if this is above the ground (more relevant here) or sea level. If it is sea level, please specify the altitude above the ground.

L67: same thing, mention m a.s.l.

L74: Why was a downsampling of the DEM necessary? Were some tests conducted to assess the influence of the DEM resolution on the mapped streams?

Figure 2: I believe there is no space at backwastes. Rather than simply stating that the velocity is negligible, can you be a bit more specific?

L105: In doing so, there is still a risk of having of lateral shift between the DEMs. This should be corrected for or at least quantified (Nuth and Kääb, 2011).

Figure 3: please remind the source of the orthophoto used.

Figure 5: these are impressive and very interesting results. However, as mentioned in the general comment, these profiles were selected manually and there be some subjectivity in this choice. A more systematic approach (more profiles, and several profiles per cliffs) would be needed to guarantee the strength of these results.

L153: something seems wrong with the syntax of this sentence.

Figure 6: Are there really no temperature measurements that could be exploited and could give a more quantitative idea compared to the qualitative ‘cloudy/sunny’ difference?

Figure 9: I don’t see any scale in panel C. Could you perhaps add an estimation of the total cliff height in the image?

L200-202: this sounds more like a discussion statement

L234: this would be figure S5. Check numbering of supplementary figures.

L256: could this thermoerosional undercutting not be estimated?

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