Review of 'Debris cover and the thinning of Kennicott Glacier, Alaska: in situ measurements, automated ice cliff delineation and distributed melt estimates' by Anderson et al, submitted to The Cryosphere

The revised manuscript by Anderson et al has combined parts A and B to focus on distributed melt estimates for most of the debris-covered area of Kennicott Glacier, Alaska. The scope is much clearer and the study is greatly improved in this formulation, which I appreciate has been a major undertaking for the authors. The presentation and analysis of observations from debris covered glaciers in Alaska are a welcome contribution to The Cryosphere, and this study uses empirical models to complement the remote sensing observations and inferences of Brun et al (2018) regarding the importance of ice cliffs and sub-debris melt, and the resulting patterns of glacier-wide melt rates.

At this point I have few substantive comments, although I think the study would benefit aesthetically from additional careful editing and textual revision. In particular, some content seems misplaced between Methods/Results/Discussion, and the storyline is not entirely clear for the discussion, which comes up just short of stating explicitly that since melt rates are not the key factor driving the zone of maximum thinning, reduced ice fluxes must play an important role.

My comments below are extensive, but primarily relate to the presentation of the study. There are a few more substantive comments, but none that require additional analysis.

Summary comments:

1. A need for careful final editing in terms of grammar and accuracy. At present the writing style is not particularly satisfying to read: many sentences start with 'it,' 'but,' 'and,' 'because,' etc that should be linked to the preceding thought, and the editing/proofreading has not been thorough.
2. When uncertainty values are given, it is not clear how they were derived or what they correspond to. In terms of representativeness, the authors seem to make some key unstated assumptions: 1) that the peak ablation season is a good proxy for annual-average ablation rates (possibly not true for ice cliffs), 2) small-scale debris thickness variability is not important to assess glacier-scale melt rates despite the nonlinear relationship between debris thickness and melt). Although I disagree with both of these assumptions, I do not think they affect the conclusions of the study. I do, however, think those assumptions should be made clear; this study makes good use of empirical methods to ask an important theoretical question, but the reader needs to be reminded of the fundamental assumptions of the framework of analysis.
3. Some content related to the continuity equation in the introduction and methods, but then this isn't really discussed in the results and discussion at the end. I guess these parts of the analysis have been retained for later publication, but it feels like an unfinished thought here; perhaps not entirely necessary (yet) or a bit of discussion would close the gap to simply indicate clearly that based on this analysis it should be tested whether a reduced ice flux can explain the ZMT.

Comments:

L18. 'melt reducing' is a compound modified and should normally be hyphenated

L19. This sentence is a bit complex due to the frequent commas. There should not be a comma after 'thinning'. The 'but' is counterintuitive here and is better replaced with 'and' as Kennicott is an example of the dynamics represented above. However, the 'under insulating debris cover' is only demonstrated by your manuscript later on (it is not yet background knowledge that the debris is thick), so perhaps best to remove 'insulating'.

L22. Although an excellent contribution, this is a semi-automated method as it requires training data.

L23. Why the comma after 'relationships'?

L25. 'which' should be 'and' for the sentence to make sense grammatically: Ice cliffs cover 11.7% of the debris-covered tongue [, … ,] and contribute…'

L26. 'with a' should be 'which has'. As written, the literal meaning is that the ice cliffs have a mean debris thickness of 13.7cm, but I suppose that this should refer to the debris-covered area.

L29-30. This is the first time that the decline in ice discharge is mentioned in the abstract. Introducing at the end of current line 24 with a succinct methodological description would clarify to readers where this comes from. Otherwise, the abstract should also have a line along the lines of 'We find a decline in ice discharge from up-glacier…'

L37. The debris is not itself expanding, but the debris-covered areas are.

L38-39. I suggest joining these sentences as they are directly linked theses. It's rarely a good idea to start a sentence with 'But'. The first clause, however, is a hypothesis that should have a reference. In addition, the only study I know of that has assessed whether debris is actually *thickening* rather than simply expanding in coverage is (Gibson et al., 2017).

L41. (F. Brun et al., 2019) is a better reference for this phenomena, although the 2018 study is also appropriate.

L43. Again, I suggest linking this sentence to the one before: ', and has been documented…' Also, Asia has already been mentioned just two sentences before, so this could be 'and has also been documented in Europe'.

L46. This phenomena being global or not is a bit out of scope for this paper. It's an interesting supposition but the important aspect for the background of your study is that it also occurs in Alaska.

L47. No need for 'from' in 'from within'

L48. 'Compelling' is out of place or a word is missing: 'This is a compelling ___ because …'

L54-55. I suppose that this question is the overarching drive of this analysis, and should be highlighted as such rather than as a rhetorical question in the text.

Eq 1. The equation requires units for each of the quantities to be the same (m/a or m w.e./a) which should be noted somewhere, since b* usually differs in units from dH/dt and the flux divergence. Also, the flux divergence terms are not strictly correct, they should be dQ_x/dx and dQ_y/dy and are partial derivatives.

L61-75. It would be good to include a reference for the continuity equation and its simplified version here; this has been done before and deserves a reference. Cuffey and Paterson is the obvious choice because it contains all these concepts, but the continuity equation has been applied to mountain glaciers extensively in Peru and the Alps since the 1990's, and since the 1970's at least for the ice sheets. I'm not suggesting a comprehensive list, but a reference or two would be value-added.

L72. Benn et al (2017) is one of the few studies that have tried to estimate b*_e, and is highly relevant.

L76. This is a nice way to present the various hypotheses.

L80. I believe you intended to italicize the 'M' in 'Melt' as well.

L84. The comma should be removed from this statement.

L85. This is not necessarily upglacier of the debris, but upglacier of the *thick* debris. Typo: 'thinning *and* reduced ice flow'

L87. (Fanny Brun et al., 2018) summarized this concept very well in their Figure 11.

L89. Typo: 'revealing'

L90. It's definitely marginally acceptable to reference your own thesis for a paper on the same topic.

L92. 'from across Kennicott Glacier' is already clear from this sentence

L93-95. The mention of 2011 appears from nowhere since it hasn't been introduced that this is the period you have data for. Perhaps it is tidier to leave the questions unconstrained temporally, but then in the next paragraph to introduce the summer of 2011 as the period of observations. You then still need to be careful to indicate that the same patterns are expected to be apparent in the summer of 2011 as for long term/annual mass balance and thinning, for example based on Das et al (2014) and possibly the ITS_LIVE velocities?

L98-99. I think it's fine to relegate the analysis of streams and ponds to another paper, but if these are also potential contributors to surface mass balance, doesn't this cut your thesis short? I.e. you are no longer able to make a statement for all melt hotspots combined, but only for cliffs. Now, we certainly know that there are few ponds on Kennicott, and their location is different to the TMZ; why not simply pretend they melt at the same rate as cliffs (somewhat as in (Kraaijenbrink, Bierkens, Lutz, & Immerzeel, 2017)) so that your ablation budget is complete? Streams are even more problematic, since they are prevalent and unconstrained. I make this point not to criticize your work; I am convinced that the hotspots are only part of the explanation and that ice dynamics are vital to explain the debris cover anomaly. I just this your huge amount of work here will be much stronger by representing these somehow (even as a hypothesis test).

L105. I believe this should be WorldView-1.

L117. Is this 20% figure from your own digitization (if so, of what imagery and when?), or from (Scherler, Wulf, & Gorelick, 2018)? Just best to indicate the source (or 'approximately'), as these numbers often get recycled.

L130. There is an extra space before 'duration'. Understanding that the scope of this study is the bulk of the ablation season for this glacier, have you considered the representativeness of this period to the full-year ablation budget? This is not likely to change your outcomes, but it is worth thinking whether ablation hotspots such as cliffs and ponds 'turn off' their melt contributions at the same time as the general debris-covered tongue. This probably depends based on the glacier-specific site and characteristics, but cliffs and ponds could have positive surface energy balance during periods of the year when conduction through debris is already negligible. The assumption here (with regards to the research question) is that the peak ablation season corresponds well to the average annual ablation across the glacier. I don't think this assumption is particularly bad, but it should be made explicit.

L136-142. I completely agree with all of this content, but it belongs in the Introduction.

L147. Perhaps combine this sentence with L148-149 'by digging through…'?

L152. It is not clear what these values correspond to – the uncertainty and variability of the measurements themselves, or (as it appears in Fig 5) of the altitudinal curve-fit. For each of these, a bit of additional information is needed.

How did you get an thickness uncertainty of 0.3cm? This is incredibly precise considering the challenge of the measurement – excavation to the ice surface without disturbing the ice surface, finding the reference height, ensuring a vertical measurement, etc. For a 30cm pit, this implies a measurement within 6 degrees of vertical, ignoring all other uncertainties, or for 60cm, 4 degrees.

What does the standard deviation correspond to – the variation between measurements at the same elevation?

I guess that the 'maximum error' is the maximum standard error of the elevation's mean debris thickness?

L159. Again, the 'how' of the uncertainty is as important as the value. In this case I would suspect that this uncertainty represents the uncertainty in the stake height measurements, spread over time? Similarly for L176.

L165. Typo in 'nonetheless'

L166-171. This is nice content and a very tidy synthesis of key points from past work on cliffs, but shouldn't this be in the Introduction? No methods appear until L171-2

L196-197. The content of the stereoimagery is out of place here, and interrupts the description of the ice cliff mapping method, for which it seems irrelevant. Perhaps it could be part of 2.3, since you use the elevation to prescribe the melt rates across the glacier?

L222. As written, I'm not sure which problems are for 'ABT and SED' or 'SED only'. This becomes clearer later in the paragraph, so maybe simplify and remove the indication of which method suffers from which problems here?

L231-2. Makes sense to reference (Steiner, Buri, Miles, Ragettli, & Pellicciotti, 2019) here. This difference is 3% of total cliff area or 3% area mismatch (non-overlapping areas)? Please make it clear in the text.

L259-270. I am impressed by the effort to represent the debris thickness variability based on flow units, which is a key aspect of spatial heterogeneity in deriving glacier-wide melt estimates based on (necessarily) spatially-biased measurements.

L274. It's nice to see h* explained in very clear terms (the half-melt thickness) and related to physical meaning. Please do add units to the definitions of each variable.

L295. Indicate the unit of degrees in the equation (for 90).

L312. Verb tense issue: '… the curve fits … *are* calculated …'

L317. ‘ice cliffs’ should be singular (ice cliff slopes) or possessive (ice cliffs’ slopes)

L318. Doubled %%

L340. Please do not start this sentence with ‘And’.

L346. A suitable reference for this is (Mertes, Thompson, Booth, Gulley, & Benn, 2016).

L383. Nice simple inference worth testing further. Most likely this relates to the difficulty of mobilizing debris, see e.g. (Moore, 2017).

L392. It’s not immediately clear what the values in parentheses mean. Are these the lower/higher estimates? Just make it clear in the first instance.

L421. Is this in cm? This content is a bit unrelated; the consistent decline in sub-debris melt is not unexpected for many other reasons as well, which h* just indicates in aggregate.

L428. Awkward comma placement. Best to get rid of ‘Becuase’ here, and connect to the next sentence (which begins with ‘But’). Again, see (Moore, 2017) for a review of these processes.

L423-446. This is interesting content but largely conjecture and lacking a synthesis – so there are many possible reasons for heterogeneity in cliff backwasting rates as you measured, but what does it mean for science? What is the point? That we need large N? Or high-quality measurements and models? Or…?

L457. This sentence undermines itself: ‘Our method works if you correct it for bias.’ I think the point here is that your method presents the spatial variability of ice cliff areas very well, even if it underestimates the total cliff area somewhat; this spatial variability is key for you to derive the subdebris melt estimates! It would be nice to see how consistent those biases are between the distinct evaluation patches, possibly as a panel in the SuppMat. A lot of content, time and effort has gone into the cliff mapping method and it would be nice to see the high quality results in more detail there.

L489. Great synthesis. My only suggestion would be to also clearly acknowledge that ice cliffs greatly enhance the overall melt rates for the debris-covered area; as presently written, the implication is that one only has to account for the increasing debris cover to get a reasonable melt estimate; this would underestimate melt by 26% in your results. Clearly the debris dominates the glacier-wide melt rate and wrt elevation, but also it is clear that you need to take cliffs into account somehow.

L515. Convincing hypothesis tests above. For completeness, you may as well perform a back-of-envelope for cliffs and streams to emphasize that, even poorly-constrained, these features cannot source sufficient energy for melt to be the only driver of the ZMT.

L515-517. Agreeing that melt is not maximized in the ZMT, isn’t it also clear that cliff and debris melt is maximized (although still less than for clean ice) leading into the ZMT? I.e. aren’t cliffs and high subdebris melt rates important drivers of the reduced ice flux into the ZMT?

L519. ‘debris-covered glacier termini’ appears to have been accidentally used twice.

L522. (Bisset et al., 2020) examined selected glaciers across HMA, not only in the Everest region

L524. The 42% figure comes from nowhere – do you mean Ngozumpa via Thompson et al (2016)? Anyways I think the suggestion is a good simplified debris thickness representation – if you can figure out how to estimate the apportioning of melt to cliffs!

L530. To my knowledge it is the first time that melt has been quantified for a debris-covered tongue in Alaska, and one of the few times it has been done convincingly, with in situ measurements, for a debris-covered area globally. Nice work.

L531-569. Please rework this long list (12 items!) of bullet points into cohesive paragraphs.

L572. Please just archive the data in a repository. You can still force people to request them or let you know what they intend to use the data for.

Table 1. The values for ice cliff backwasting in the 'min' and 'max' cases do not seem to correspond to Table 2 or Figure 7 (these would be 4.6 and 9.6). Which values did you use?

Table 4. Note that (Thompson, Benn, Mertes, & Luckman, 2016) lumped all cliff-associated influence by necessity, whether due to cliffs/ponds/streams etc.

Figure 1. On panel (c) the legend need some work to show the variation (dH/dt) and units clearly. Please also consider using a proper color ramp – I know it is a pain in QGIS. I don't think there is a need for the statistics from the Das et al (2014) study in the caption, but if you want to include them, it's not clear what you mean by the mean error and 1 std – is the 'mean' the glacier-wide error of the mean, and 1 std (can be sigma) the standard deviation across the glacier? This is easily misunderstood.

Figure 3. Very nice synthesis.

Figure 4. As for figure 1, the legend in panel (a) needs to be tidied.

Figure 5. Correction to caption text: 'The red bars *are* the median…'. For completeness of the manuscript itself, and also as a nice comparison to this relationship, I would prefer to see the relationships of debris thickness with elevation for the other moraines here.

Figure 6 caption. '… is smaller *than* the marker …'. The RMSE units are presumably cm w.e. d^-1?

Figure 7. I suggest to indicate 'ZMT' next to the arrow for clarity. In (b) the axis labels are not terribly clear: 'aspect' is not labelled anywhere (nor the units) and the ice cliff backwasting axis values are a bit confusing due to their location.

Figure 8. In some respects the results without morphological opening look even better, because the opening has also removed thin slivers of true ice cliff area. Did you try a connected-pixels morphological clean? This would eliminate (for example) 'cliffs' with less than 10 pixels, and would generally leave the thin cliffs unmodified. Perhaps you are happy with the current performance, but it's just a suggestion.

Figure 9. In the caption, there is mention of 'thin red lines' but I don't see these anywhere. Perhaps use a different colour for these?

Figure 10. Please indicate the elevation bin size for the hypsometry.

Figure 11. Why not delineate the ZMT here using elevation contours (thus corresponding directly to the rest of the paper)? Otherwise I find myself wanting to flip back several pages to see where the contours are, etc.

Figure 12. Small typo: '84.1% of estimate<u>s</u> are within the grey shaded band.'

Bisset, R. R., Dehecq, A., Goldberg, D. N., Huss, M., Bingham, R. G., & Gourmelen, N. (2020). Reversed Surface - Mass - Balance Gradients on Himalayan Debris - Covered Glaciers Inferred from Remote Sensing. *Remote Sensing*, *12*(1563). https://doi.org/10.3390/rs12101563

Brun, F., Wagnon, P., Berthier, E., Jomelli, V., Maharjan, S. B., Shrestha, F., & Kraaijenbrink, P. D. A. (2019). Heterogeneous Influence of Glacier Morphology on the Mass Balance Variability in High Mountain Asia. *Journal of Geophysical Research: Earth Surface*, 1331–1345. https://doi.org/10.1029/2018JF004838

Brun, Fanny, Wagnon, P., Berthier, E., Shea, J. M., Immerzeel, W. W., Kraaijenbrink, P. D. A. A., … Arnaud, Y. (2018). Ice cliff contribution to the tongue-wide ablation of Changri Nup Glacier, Nepal, central Himalaya. *The Cryosphere*, *12*(11), 3439–3457. https://doi.org/10.5194/tc-12-3439-2018

Gibson, M. J., Glasser, N. F., Quincey, D. J., Mayer, C., Rowan, A. V., & Irvine-Fynn, T. D. L. (2017). Temporal variations in supraglacial debris distribution on Baltoro Glacier, Karakoram between 2001 and 2012. *Geomorphology*, *295*, 572–585. https://doi.org/10.1016/j.geomorph.2017.08.012

Kraaijenbrink, P. D. A., Bierkens, M. F. P., Lutz, A. F., & Immerzeel, W. W. (2017). Impact of a global temperature rise of 1.5 degrees Celsius on Asia’s glaciers. *Nature*, *549*(7671), 257–260. https://doi.org/10.1038/nature23878

Mertes, J. R., Thompson, S. S., Booth, A. D., Gulley, J. D., & Benn, D. I. (2016). A conceptual model of supra-glacial lake formation on debris-covered glaciers based on GPR facies analysis. *Earth Surface Processes and Landforms*. https://doi.org/10.1002/esp.4068

Moore, P. L. (2017). Stability of supraglacial debris. *Earth Surface Processes and Landforms*. https://doi.org/10.1002/esp.4244

Scherler, D., Wulf, H., & Gorelick, N. (2018). Global Assessment of Supraglacial Debris-Cover Extents. *Geophysical Research Letters*, *45*(11), 11,798-11,805. https://doi.org/10.1029/2018GL080158

Steiner, J. F., Buri, P., Miles, E. S., Ragettli, S., & Pellicciotti, F. (2019). Supraglacial ice cliffs and ponds on debris-covered glaciers: Spatio-temporal distribution and characteristics. *Journal of Glaciology*, *65*, 617–632. https://doi.org/10.1017/jog.2019.40

Thompson, S., Benn, D. I., Mertes, J., & Luckman, A. (2016). Stagnation and mass loss on a Himalayan debris-covered glacier: processes, patterns and rates. *Journal of Glaciology*, 1–19. https://doi.org/10.1017/jog.2016.37