Reply to Reviewer 1 Part C

Review of “Debris cover and the thinning of Kennicott Glacier, Alaska, Part C: feedbacks between melt, ice dynamics, and surface processes” by Anderson et al.

Thank you kindly for taking the time to review our manuscripts.

This study is the third part of three publications that investigate debris cover on Kennicott Glacier in Alaska. The focus of this study is on the feedbacks between the melt, ice dynamics, and surface processes for the debris-covered portion of Kennicott Glacier. After reading all three parts, the introduction feels repetitive of Parts A and B (I recognize this is unavoidable).

We will work to make the introductions distinct between the three contributions.

Unfortunately, the methods and results in this section are highly underwhelming. The new results in this part are surface velocities, emergence velocities based on those surface velocities and ice thickness, and manual delineation of streams for a single WorldView scene. These are all quite straightforward analyses that are fairly easy to perform.

We agree that these are easy analyses, but the strength of this work is in bringing the pieces together and providing a number of new, important process observations. We do propose a number of improvements to enhance this third part of the paper series.

Where this paper excels is in its discussion, which is grounded in the observations and results from Parts A and B with a little support from results and theory in Part C.

We want to also emphasize the number of new process descriptions we add here. While many of these describe processes acting on the glacier, quantifying these behaviors in the future studies may be an important future direction of work on debris-covered glaciers.

While the discussion does not provide any conclusions that are necessarily groundbreaking (the relationship between debris thickness, surface velocities, and surface processes have been detailed for debris-covered glaciers in other parts of the world in other studies, which this manuscript references) … attempts to make universal statements concerning all debris-covered glaciers at times.

We want to emphasize how much new material is actually in this manuscript. These are observations that have never been put together on a single glacier, with the continuity equation kept in mind. Other studies have discussed some feedbacks between some surface processes but here we also provide new process descriptions.

From reviewer 3:

“for me part C works very well as a stand-alone paper and has a very clear own focus on the dynamic feedbacks and interactions and more than enough conclusive results for a stand-alone paper. “

… attempts to make universal statements concerning all debris-covered glaciers at times.

While we appreciate that there is a lot of diversity of debris-covered glaciers we also feel that there are fundamental processes and physics that are acting on all debris-covered glaciers. We try to walk a line between emphasizing these fundamental processes that may occur on other debris-covered glaciers. We will take more care though in the revision process to ensure that we don't overstep with our assertions.

The manuscript is well written, the figures support the text well, and there are sufficient references to the existing literature. Hence, my comments are fairly minor, but I admittedly have mixed feelings concerning the originality of the paper to stand on its own.

Based on

From reviewer 3:

"for me part C works very well as a stand-alone paper and has a very
clear own focus on the dynamic feedbacks and interactions and more than enough
conclusive results for a stand-alone paper. "

If Parts A and B were separate studies by other authors, then I would argue that the originality and methodology would be poor-fair; this paper would come across more as a review paper of how existing studies are connected and likely not warrant publication without major revisions. However, that is not the case, and instead this paper comes across as an extension of Parts A and B, and a place where everything can be discussed in a broader context.

What would truly elevate this paper to stand on its own, would be if the theoretical feedbacks were supported by model results. Given that Part B develops empirical equations for accounting for distributed melting due to debris, ice cliffs and backwasting, this would be a very logical next step. However, I recognize that more information is likely needed concerning ice cliff nucleation and debris redistribution to be able to model these various surface processes over long periods of time and at a high enough level to support the discussion.

We would love to model the feedbacks we describe here and that is the target of further research. Here though our aim is to provide process descriptions beyond the important feedbacks that have been explored extensively in these manuscripts as well as in others.

My recommendation would be to integrate Parts B and C into a single manuscript. Given the minimal additional methods and results, and the major use of results from Parts A and B in the Part C discussion, it seems like the discussion in Part C could be condensed, without losing its purpose, and combined with Part B. Given I am not a reviewer on Part B, I will suggest the manuscript be reconsidered after major revisions. However, I will note that as a whole, Parts A, B, and C are a tremendous advance for our understanding of debris-covered glaciers, especially in Alaska. Therefore, if the editor believes Part C is warranted to provide sufficient space for the authors to discuss their two previous studies, then I would be supportive of accepting this manuscript subject to minor revisions.

Thank you kindly for your time and effort reviewing these manuscripts. We appreciate it and hope to return the favor soon. While we understand the desire for consolidation we also feel that that this reviewer actually missed the new feedback we reveal on Kennicott Glacier.

We also want to highlight that this is the first study we know of that rather clearly links ice dynamics to ice cliff distribution.

Please find specific comments below.

Main Comments
Surface processes description: I disagree with the semantics used to describe surface processes as a separate term not explicitly referenced in the continuity equation, since they are explicitly in the continuity equation as the specific ablation. This description suggests that there is another term that needs to be accounted for. What the authors are trying to state is that surface processes are important since they control the distribution of ice cliffs, lakes, and streams, which feedback into the specific ablation and the ice dynamics. However, this feedback is nothing new and has already been described in L41-44.

The feedback we are highlighting is actually different than what the reviewer has just quoted and is outlined in Vincent et al., 2016; and Brun et al., 2018. This may be because we did not clearly describe the feedback.

On Kennicott Glacier we find that we find that ice dynamics appears to correlate with ice cliff dinsity. The process links we describe must not be clear enough though in the manuscript as it stands now. We will clarify this in our writing. The high strain rate at the upper part of the (zone of maximum thinning) *ZMT* correlates with high ice cliff density. Low strain rate we see low ice cliff density. We provide a physical mechanism where high strain rates can lead to increased ice cliff density. Increased ice cliff density leads to increased melt rates, which then contribute to increased glacier thinning locally at the upper end of the *ZMT*. Active ice dynamics and increased emergence rates which tend to locally thicken the glacier are compensated with increased ice cliff coverage which tends to thin the glacier.

This is an absolutely new feedback that we have identified. Clearly we need to describe this effect more clearly.

Furthermore, I would argue that “debris cover” should be included as a “surface process” because it differs from the typical clean ice and by itself would impact these relative feedbacks. I would recommend that the authors simply state that the specific ablation for debris-covered glaciers is affected by the distribution of debris thickness, ice cliffs, lakes, and streams, which will control the melt rate and feedback into the ice dynamics.

We appreciate this perspective. But we are taking a view that is more from geomorphology (earth surface processes in general). From a landscape evolution perspective the erosion of the earth’s surface is the glaciological equivalent of melt. The actors causing the erosion of the earth’s surface would be rivers, mass wasting, and hillslope processes. But on debris-covered glaciers melt is the result of heat from the atmosphere, and solar radiation and also other features, like ice cliffs, streams, and ponds. We will think about this differentiation going into revisions, though.

Accounting for streams that undercut cliffs: can the authors comment on how they handled mapping streams that are undercutting ice cliffs?

Thank you for highlighting this, we can be more clear. We will show WV photos from the glacier surface that show the processes of extrapolation under ice cliffs, this can be included in the supplemental. We have field photos to show the extreme sinuosity of many of these streams. That guided our digitization.

Given the area of thick debris is more stagnant, this area has less ice cliffs. The ice cliffs that do exist are undercutting thicker debris which depending on the slope, may cause the ice cliff to be covered in a layer of debris (whether this suppresses or enhances melt is unknown), which is shown in Figure 8a and 9c.

Yes we agree with these statements.

The key is that this region likely has thicker debris and fewer ice cliffs. The thicker debris means there is likely less backwasting at the top of the cliff compared to cliffs further upglacier that have thinner debris. This means that the cliffs may be able to survive longer.

Thanks for highlighting this processes we will try to work it in as a possibility.

If the cliffs can survive longer, then they may be prone to have more steams that are undercut. I assume (the authors may confirm or deny) that these cliffs are unable to be mapped from high-resolution optical images. This could provide another explanation for the drop in the number of streams in the area of thick debris.

We walked over most of this lower tongue and there are very few streams present at the base of these cliffs where debris is thick. We will provide photo evidence to support this.

There just aren't streams in this region with thick debris and the ice cliffs are often in closed depressions and have small drainage basins, which we will more clearly highlight with analysis of a high resolution surface DEM.

Specific Comments
Italics indicate suggested grammatical changes

L15 – "enhancing" the mass balance does not make sense. Consider changing mass balance to mass loss or enhanced to something like affected.

We will change this term.

Abstract – a four paragraph abstract seems unnecessary. Consider condensing to one to two paragraphs.

Yes, we can do this.

L24 – "melt gradient" should be "melt rate gradients" to be consistent with the text.

L24-27 – the abstract should clearly reflect the main findings in the conclusion. I assume that the "high" in "high melt, melt gradients, and ice dynamics" means that all three of those elements are "high"? This is not particularly clear. Furthermore, what is a "high melt gradient" or "high ice dynamics"?

We will make this more clear.

Consider rephrasing these sentences, making them more descriptive and easier to understand. In its present form both the upper-limb and lower-limb have a high ice cliff and stream occurrence, which is inconsistent with the text. The conclusion states these feedbacks well. The abstract should do the same.

We will clarify as suggested.

L28 – can you just state "The zone of maximum thinning occurs..." since the boundary between these two process domains is not well-defined anyways?

We can more definitively define the boundary between these zones.

L34 – “insulates” surface melt does not make sense. Consider “insulates the glacier and strong reduces melt”.

We can clarify this.

L44 – I would strongly encourage only using acronyms when they are absolutely necessary and common. I would recommend removing the acronym ZMT throughout the text to make it more readable for a broader audience.

We will consider removing the acronym.

L44 - Is Figure 1C a result of the present study or a result of Part B? If it is Part B, then it should be cited. If it’s a result of this study, then the zone of maximum thinning should not be presented in the introduction.

We will cite Part B.

Figure 1 – “with the opposite sign in the same pixel”. State in the caption that the zone of maximum thinning is referenced by the double arrow. You can delete the ZMT as this is simply confusing in its present form and will be clear from the text.

We will do this.

What does “Swatch profiles presented lower are 1000 m wide” mean? Where are these profiles? They do not appear to be shown in the figure.

The swath profile is 500 m on either side of the line in Figure 1c.

Also, the dH (dt) -1 label looks very out of place. Consider positioning above the legend.

We will move this.

L45 – stating surface melt and ice dynamics are fundamental to thinning is repetitive of the prior paragraph and can be deleted.

L59 – somewhere in the introduction, whether this be the first sentence that uses “thick debris”, or elsewhere, please define what is meant by “thick” debris (> 0.5 m? > 0.2m? >0.02 m?).

We will define this.

L66 and elsewhere – when referring to elevation make sure to be consistent. I would also recommend using “m a.s.l.”.

We will use this.

L94 – what does “New analyses were required to estimate the annual velocity pattern” mean? Is this referring to Armstrong et al. (2016) and Armstrong et al. (2017)? Or the velocity maps produced in this study, which clearly was a new analysis?

We refer to the mean annual velocity derived from the Part C study.

L96 – based on what observation? This is really an assumption and should be stated as such.

We will rephrase this but we aren't sure it is really an assumption as the equation is simply from fluid mechanics.

L100 – define w in the text.

We will do this.

L110 – were the ice thickness "derived" or simply was ice thickness estimated by Huss and Farinotti (2012)?

It is the ice thickness from Huss and Farinotti. We will clarify this.

L111 – Is this estimate of emergence rates assuming a uniform bed a second estimate of emergence rates? Or is this simply another assumption behind the emergence rate calculations? What does a uniform bed under the glacier fixed at the terminus mean?

We will clarify this in the text. For the uniform bed case, we assume that the bed of Kennicott Glacier is uniform at the elevation of the terminus mean. We include this just to show an end member case.

Figure 4 is referenced before Figure 2 and 3. These should be placed in the order in which they are mentioned in the text.

Figures 2 and 3 are references on line 62. Figure 4 is referenced on line 125. No change is needed.

Figure 2, Figure 5, and elsewhere – melt rate should always be positive. If the values are reported as negative then this should be the mass balance or surface lowering rate.

Thank you for pointing this out. We will correct this throughout the manuscripts.

Figure 5 – why are the values placed on the right y-axis? This implies a secondary axis, but the only plot that has a true secondary axis is g. Change the labels to the left axis so that this plot is easier to read.

Thanks for this comment. We put the tick labels on the right because most of the data is 'high' in the plot on the right and low on the left. So it isn't clear that putting the labels on the left will improve the readability of the plot.

Unclear what "swatch profile" refers to.

The center of the swath profile is shown in Figure 1C.

The description of the flat bed case in this caption should be moved to the text (L111). Change the following: Where surface velocities and emergence rates are low. I suggest explicitly pointing out the topographic bulge in panel e, so that this is clear for readers.

Yes we will make these changes.

Figure 5g - Is it necessary to abbreviate length to save two letters? This seems unnecessary.

Maybe not we will take a look, but we did this so the label was not fixed to three lines.

Also, confusing that the lakes are in a legend while the ice cliffs and streams are not. At a minimum the ice cliffs should be added to the legend, so that it is clear that they refer to the fractional area as well.

We will work on this, but there just isn't enough space in the panel, without reducing the legend text. We will try.

L125 – consider stating that the surface velocities decrease downglacier to near stagnation.

Yes, thank you.

L129 – the range of emergence rates for both cases should be specified in the results.

L170 – "In the ablation zone" should be a new sentence.

L171 – rephrase this to be clearer. The key point here, which is explained well below, is that the feedback between the debris thickness controlling the melt rate, which affects the ice dynamics, which feedbacks to control the debris thickness.

We will make sure this is clear.

L177 – close the parentheses.

L179 – should be a comma before "ice flow should also be high" and the same for the next sentence.

L182 – "melt rates are high, and surface slope..."

L187 – consider deleting the ":" and replacing with "as" or "since" to make it more readable.

L209 – this appears to be a universal statement. Is this meant for all debris-covered glaciers? Alaskan debris-covered glaciers? Are the authors confident with the 20 cm characterization despite the fact that they state the cutoff for these two process domains could be anywhere in the 10-20 cm range (L149)? A better preface could be that this mechanism is expected to occur on other debris covered glaciers where the debris transitions between the two process domains. Given the theory behind the discussion, this would seem to be more universal.

We were not clear enough in writing. What we mean is that ice cliffs are more likely to be buried the thicker the debris is around the ice cliff. The debris climbs up the ice cliff. We will provide videos to support his inference. We have two supplementary videos to support this.

L216 – delete the comma.

L229 – "potentially lead to ..."

L251 – Process links? Or Processes linked?

We will clarify this.

Figure 10 – Cause Ice Dynamics and Effect Debris have the same for the upper and lower limb. The text should be centered like the ice cliffs, lakes, etc. below it. Delete second “that” in caption.

We will correct this.
L304 – should this be “debris thickness”?

Thank you we will correct this.

Part C: proposed changes

We want to emphasize here that we do outline new feedbacks in this paper.

From Reviewer 3 from Part C:

“P 2 line 62-63: importantly in part C you not just present data on ice dynamics and supraglacial streams but crucially in part C these data and all components of the mass conservation equation (thinning, flux divergence. . .) are analysed for relation and feed-backs between them. Also say this here, as it is the backbone and most exiting part of this part C.”

On Kennicott Glacier there is a strong correspondence between ice cliffs and active ice flow. While weak relationships have been suggested here on Kennicott the correlation is more clear than anywhere else.

The highest concentration of ice cliffs occurs at the upper end of the zone of maximum thinning. The high concentration of ice cliffs also corresponds to where we expect ice emergence rates to be high. These ice emergence rates uplift the glacier surface, working to counter glacier thinning. But ice dynamics, which produce this surface uplift also seems to produce more ice cliffs (see the physical descriptions within the main article). These ice cliffs counter the effect of surface uplift, they are essentially a negative feedback on the effect of ice dynamics.

In addition to this new feedback we also present a number of new hypotheses for the interaction of surface processes with melt and ice dynamics with a new, holistic perspective.

We feel that there is more than enough new material here for a stand alone paper, but in order to improve the manuscript we propose that we add these additional datasets/ideas to Part C:

- New annual surface velocities from 2000-2010
  - These velocities allow us to calculate changes in ice emergence rate and ice flux over the in situ measurement period
  -More detailed discussion of the reduction of ice emergence rate through time.

- Delineation of drainage basins on the glacier surface (new figure) to support the stream story already within the manuscript.

- Tie in a discussion about glacier surface topography. Ice cliff maximum heights (from in situ measurements), the number of individual ice cliffs with elevation band, and calculated glacier surface relief down glacier.

- New processes drawings to show the important new observations that we are highlighting in this paper. This will greatly improve the reader's ability to see the new process links we are describing.

- Additional photo evidence from the field outlining these new processes links. Many will go into the supplemental but they will support and clarify the process links we are highlighting.

- Description of a new ice cliff burial mechanism. Timelapse movies from the Kennicott and Ngozumpa glaciers (in the supplemental) showing a new mechanism for the burial of ice cliffs. The actual process is not yet described in detail in the text.

- A paragraph that is the same for each of the 3 parts that outlines how they build off of one another.