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.

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). 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.

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. 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), it does provide an excellent holistic view of how the various feedbacks are connected for Kennicott Glacier and attempts to make universal statements concerning all debris-covered glaciers at times. 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. 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.

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.

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. 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.

Accounting for streams that undercut cliffs: can the authors comment on how they handled mapping streams that are undercutting ice cliffs? 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. 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. If the cliffs can survive longer, then they may be prone to have more streams 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.

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.

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

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"? 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.

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

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

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.

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.

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

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?).

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

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?

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

L100 – define *w* in the text.

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

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?

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

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.

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. Unclear what "swatch profile" refers to. 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. Figure 5g - Is it necessary to abbreviate length to save two letters? This seems unnecessary. 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.

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

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.

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.

L216 – delete the comma.

L229 – "potentially *lead* to …"

L251 – Process links? Or Processes linked?

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.

L304 – should this be “debris thickness”?