Reply to Reviewer 2 Part C

Interactive comment on "Debris cover and the thinning of Kennicott Glacier, Alaska, Part C: feedbacks between melt, ice dynamics, and surface processes" by Leif S. Anderson et al.
Anonymous Referee #2

Thank you kindly for taking the time to review our manuscripts! We appreciate it.

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Review of Anderson et al., Part C, The Cryosphere, October 2019

In this third paper, Anderson et al. gathered ice velocity data and combine them with rough estimate of the ice thickness to infer ice fluxes and emergence velocities. They also derive the pattern of surface water streams on the glacier and their sinuosity. All the data collected in the three papers are then analysed to discuss feedbacks between ice dynamics and surface melt pattern and how they can explain the evolution of a debris covered glacier tongue.

Thank you kindly for taking the time to review this manuscript. We appreciate your comments and thoughts.

General comments for the three papers (mostly similar to my review of part B).
1/ I am (really!) not convinced by the need to split this study into three parts. It implies lot of repetitions and also mean that the reader as to refer to other parts of the article which is not convenient. Some data are plot several times in the three article (debris thickness, dh/dt for 1957-2009 etc. . .) I think the authors missed here an opportunity to put everything together. It would also help to convey more directly and simply the message.

We appreciate your efforts in reviewing these manuscripts. But we feel that the reviewer does not appreciate the insights provided by each of the three manuscripts that outlines different aspects of a large debris-covered glacier from a region where there are no other studies on debris covered glacier mass balance.

From reviewer 1:

"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."

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. "

From reviewer 4:

"My general comment is that this is a rigorous and well-argued study which shows some

interesting results, different from other papers I am familiar with."

This is an exhausted reader (or reviewer) that finally reaches part C, a paper where very few news results are presented (just velocity data taken elsewhere and a map of the steam network that could have been presented at the time as the lake inventory). I found the discussion confusing and I must admit I did not understand the feedbacks at play. I also did not end up with a clear take home message.

This is an ambition manuscript and the first manuscript to tie a diversity of measurements together with the continuity equation. This in and of itself is a very valuable contribution, especially from a glacier that shows clearly that these components co-vary.

The point is to connect the melt pattern, ice dynamics, and surface processes in a fashion that has not yet been done. We outline a new potential feedback between ice cliffs and thinning that has not yet been identified. We think that that there are quite a few take home messages from this manuscript. For example we show important feedbacks between streams and ice cliffs, new potential processes for the formation of ice cliffs, new links between ice dynamics and ice cliff distribution. These are all new, important contributions just from Part C.

If we were to combine all three parts most of these insights would be lost along the way. Furthermore there would be so many directions explored in a single manuscript that it would be even more overwhelming and less legible.

2/ One strong limitation (that needs to be emphasized more) is that field measurements over a short period of time in July 2011 are used to interpret a map of elevation change over a multidecadal time period. Authors need to recall to their readers that their results apply to a short period of time. The whole discussion would have been much more meaningful if the elevation changes were also measured for the same time period where surface melt features are studied.

We have dh/dt data that spans the in situ measurement period and we will include them in Part B. The zone of maximum thinning is in the same location as the dh/dt map we show in Part B and C.

The extensive discussion at the end of Part B outlines how extreme the changes in ice cliff coverage (increase in ice cliff coverage of 70%) and debris thickness (90% reduction in mean debris thickness) needed to create maximum melt in the ZMT. We feel that this is a compelling argument. We are surprised that this argument was not clear.

Then, authors could have attempted to verify closure of the mass budget (continuity equation) between flux gates separating different parts of the glacier. It would have been a convincing verification of their surface melt estimates, involving some spatial extrapolation.

We realize that past work took the approach you outline here, but we want to emphasize that different ways of argument we use are also viable. The line of argument we use is an alternative, viable approach to flux gates and DEM differencing over the same time span.

General comments for part C.

3/ I found a lot of speculation in the discussion. Just an example: that surface flow field has become more "S-shaped" through time. Authors do not present any velocity observation that can back up this. It seems to be just a good guess.

We would rather posit that some of the paper is based off of field observations, while we do not model or provide quantitative constraints on the hypotheses/observations they are still new, important contributions.

The medial moraines tend to follow flow lines. But yes we are including new surface velocity estimates in the next version of the paper.

4/ A said above, the whole discussion is based on a zonation (the ZMT = zone of maximum thinning) of the glacier tongue from the long term dh/dt, over 5 decades. But to what extent this dh/dt rate is representative of the 2 month changes of the glacier? This is never addressed and it severely undermines the conclusions.

Thank you for bringing up this discrepancy. We addressed this discrepancy in length in Part B in the discussion by placing extreme bounds on our melt estimates. We are including a more recent dh/dt that spans the field measurement period (the ZMT is in basically the same location).

Specific comments.

Abstract does not really read like an abstract. More like an introduction. Authors should aim at ~250 words to keep it concise and to the point. There is no implication or general statements at the end.

We will improve the abstract, but the holistic perspective we take on this work is new and we provide a way forward for the research of debris-covered glaciers. As debris-covered glaciers researchers it is an important next step for us to start looking at interactions between the components of the debris-covered glacier system.

L44. It was not demonstrated in part B that "ice dynamics control the location of the ZMT". This assertion comes from nowhere.

We will justify this more clearly. From a simple process of elimination from the continuity equation and our extreme uncertainty analysis in Part B we thought this was apparent.

L77. "significant" is not quantitative. Can a percentage or a range of percentage be provided?

Yes we will add the percentage here.

L88. "based off of" (?)

We will change this to ‘derived from’

L110. How uncertain is this ice thickness data? Did this paper (or later studies by D. Farinotti) provide constrain on the (likely) large uncertainties for a single profile on glacier which is thinning rapidly.

Yes, We are happy to discuss those uncertainties. But it will not change the pattern of ice emergence rates that we estimate, unless the glacier is thicker below than ZMT than above it.

(when I see the nearly 0 emergence velocity in Figure 5 and the difference to the “flat bed” I think these uncertainties need to be discussed)

But ice emergence rates are derived from dQ/dx and Q = $\bar{u} * H$ If $\bar{u}$ is very low then it doesn't really matter what *H* values are Q will still be low.

L114. At this stage in the paper, the reader wonders why streams need to be mapped.
And why this is done in this third paper? Should ideally be grouped with lake mapping.

We will make the justification for presenting streams in Part C in the intro. We will also add a back of the envelope estimate of the effect of streams on mass balance on Kennicott Glacier in Part B.

This is included in the third paper because streams up to this point have not been included as significant contributors to surface mass balance. How would we quantify their effect on surface mass balance? We intend to include a back-of-the-envelope calculation of the potential effect of streams on the Kennicott Glacier in Part B.

But we also prefer to have the stream digitization here in Part C where we discuss what we see as the primary role of streams: maintaining ice cliffs and focusing debris. And stream sinuosity plays a potentially important role in those effects.

L115. Date of the image? digitization made for the entire glacier? Or the debris covered part only?

We will clarify this.

L120 the very limited amount of new result in this part C reinforces my opinion that this paper could be merged with other parts.

We disagree here. Where has a similar study been published? It would be helpful if the reviewer provided evidence for this statement.

Velocity, melt and surface features have very clear patterns and some co-vary. This starkness of these patterns is worth in depth discussion because we are unaware of any other studies that so these relationships so clearly and consistently. Additionally we add a number of new process feedbacks that have not yet been detailed.

We provide a first holistic view of the debris-covered glacier system. While yes it has been shown that ice dynamics are important for debris-covered glacier thinning, in this manuscript we want to move beyond these items viewed as isolated entities but rather as pieces of a whole that interact with one another.

The stack of data (Fig. 5) is rather unique and not where we tie the different components together. The discussion about ice cliffs and streams and feedbacks is completely new.

Much of the new insight comes from field observations. But these are important observations that need to be published.

From reviewer 1:

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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."

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. "

From reviewer 4:

"My general comment is that this is a rigorous and well-argued study which shows some interesting results, different from other papers I am familiar with."

L125 can the authors confirm that this systematic offsets were not corrected ? and thus may result in biased emergence velocity? This is a significant proportion of the total velocity.

Sure we can correct for this error but it is a systematic error. But it is not clear how a systematic offset would change the **pattern** of ice emergence. We are not looking for a perfect ice emergence velocity, that is not possible, but we give the best, defensible estimate of emergence velocity. That pattern is consistent with the rest of the analysis.

L129. I do not think these two cases of bed were described earlier in the text. Why the need for the Flat bed?

We will clarify this. We include a flat bad to show the effect of changing the bed map. And show that the ice emergence rate pattern we highlight is not necessarily dependent on the assumptions of the bed maps from Farinotti.

L155. The fact that debris thicken downglacier is probably repeated close to 10 times in the three papers (and also plot many times). This is irritating. It illustrates why the artificial separation in three papers does not work.

Thank you for pointing out this repetition we are happy to state ' thickening debris downglacier' it less often. But we are not clear how this is the result of artificial separation of the papers. The reviewer could be more clear. We feel that this is just the result of us needing to smooth the writing for this manuscript.

Whole Section 4.1.2. I am not sure I get the point here and I do not really understand what is the actual finding: thick debris are found on almost all stagnating glacier tongues where melt rates are low, emergency velocity and dh/dt also. There is nothing really new here.

Thank you for pointing this out, we are trying to show how the very pattern you describe is then reflected in many other properties and how that is related to Ostrem's curve. We are just laying the foundation for further analysis lower down in the manuscript.

Also I do not understand why the authors consider a steady state to interpret the evolution a glacier that is actually far from equilibrium. How debris are distributed nowaday is probably inherited from decades of imbalance.

We are just saying that a glacier is always trying to balance the effect of low melt (under thick debris) with reduced ice flow even if the glacier is not in steady state. This is a very difficult concept to communicate but it is important for laying the foundation for what is below.

L202-210. I find this part of the text poorly connected to the data/results obtained. Such a discussion would be relevant for a study examining time series of images and able to observe those debris mass wasting events related to the heterogeneity of the melt rate. Right now, no data in the study allow elaborating or confirming such a theory as a one-shot debris thickness and cliff distribution map was produced.

We are outlining a holistic perspective. These are arguments that are outlining the range of controls on ice cliff distribution. We are laying out the process basis for further analyses to follow later. We are surprised at the resistance to us presenting ideas that are based on physical arguments. Not all contributions need to be quantitative. We are crossing over into areas that are closer to hypotheses and we aren't sure how that is not a valid contribution.

Reviewer 4 has a good perspective here:

"I don't think it [this manuscript] provides definitive answers to the problem of quantifying the feedbacks in these complex systems, but it does point to a way forwards." As such we think this is manuscript an important contribution to the rock glacier literature.

L310. Glacier

L311. "an ice cliff-glacier thinning feedback is evident on Kennicott Glacier". This was not evident at all for me, I do not think it was demonstrated or I did not get it.

We are proposing that

Figures 1, 2 are good examples of redundant figures, already shown almost identically in part A and B.

As the writers of these manuscripts we had to make decisions that from our perspective can be viewed as arbitrary from the reader (like where to add streams or which figures to repeat). We suspect that if we didn't include these figures then the reviewer would take issue with having to look back at the other parts to see these keep results. From our view this is a minor amount of repeat. If we published these three manuscripts independently there would be no complaints about repeating a context figure.

Figure 3. Do the authors have evidences of reduced ice fluxes with time? This is probably an important part of the story, it is indicated on this figure but not really in the paper. Are the surface velocities changing with time? Or only the reduction in ice fluxes is due to surface lowering? These changes ice fluxes are probably key to understand the present-day distribution of dh/dt and debris on the tongue.

Thank you for highlighting this. We have extracted additional annual surface velocity patterns from and earlier time period and we will include them with flux estimates from earlier in the next iteration of the manuscript. As we should all expect though (we know ice has thinned, velocity has lowered) but we can show quantitatively that ice flux is reduced.

Figure 5d. The difference between "Flat and Variable" bed needs to be discussed more. It is worrisome that the "Flat Bed" curve show nearly 0 emergence velocity in

region of high melt, in the active ice zone.

The reviewer has misread the plot here, the variable bed plot shows nearly 0 emergence velocity. But the surface velocity pattern in this region is nearly uniform in this region. If the ice thickness is also nearly uniform in this region then ice emergence velocity can also be 0. This is totally physically viable.

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.