Reply to Reviewer 1 Part B

Interactive comment on “Debris cover and the thinning of Kennicott Glacier, Alaska, Part B: ice cliff delineation and distributed melt estimates” by Leif S. Anderson et al.

Anonymous Referee #1

Received and published: 18 October 2019

Review of Anderson et al., part B, The Cryosphere, October 2019

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

In this second opus of their trilogy, Anderson et al. deduced the spatial pattern of melt due to ice cliff and under debris, and consider the distribution of supraglacial lakes to conclude that melt hot spots (cliffs and lakes) are not sufficient to explain the pattern of rapid thinning on Kennicott Glacier.

Overall this is a series of paper that bring a lot of new data and contribute to show that melt hot spots (ice cliff and lakes) only modestly contribute to the overall mass loss of a large debris covered tongue. A clear achievement has been to perform such measurements on a very large glacier in Alaska and proposed methods to extrapolate the point wise measurements to the overall debris-covered glacier tongue.

Thank you for the kind summary.

General comments for the three papers.

1/ I am not convinced by the need to split this paper 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.

We appreciate this perspective on the body of work. A single manuscript would allow the development of the ideas without needing introductions or conclusions to transition through to see the full breadth of the work.

What would happen with one manuscript though is that many of the conclusions from each Part (in situ, ice cliff delineation/distributed melt, and holistic view) would be lost in a single contribution. It would simply be an overwhelming manuscript, with a diverse swath of conclusions. We tried this in a previous submission and the paper was rejected for being too complex.

Some data are plot several times in the three articles (debris thickness, dh/dt for 1957-2009 etc. . .) I think the authors missed here an opportunity to put everything together.

We understand and accept that some repetition can be annoying. Here we tried to walk the line between too much repetition and too much turning back and forth between manuscripts. But we would like the reviewers and editors to consider how overwhelming one manuscript with all of methods and ideas we outline across the full breadth of the 3 parts. That manuscript would be a monster and less intelligible than what is presented here.

Specifically in this part B, the discussion (section 4.2.1) whether ice cliff or debris can explain the zone of maximum thinning would be much more straightforward if Part B and C were merged. Right now this discussion is a lot of speculation to finally justify the need for a part C.

We can see how the body of work could be split in different ways and certain parts would be easier to digest.

We feel that Part B presents a different perspective on thinning than Part C. First we look solely at melt and then we add in dynamics for additional evidence.

We don't feel that section 4.2.1 is much about speculation but rather addressing a new perspective not often discussed in debris-covered glacier literature. We find the arguments we present there are valuable for considering other debris-covered glaciers. Taking an end member approach allows us to consider these processes from a broader perspective than they often are.

Part C presents, additional evidence about whether ice cliffs can explain the zone of maximum thinning but there are additional new items in Part C. We try to bring together all of the observations in a holistic framework in Part C. We feel that is an important contribution that justifies more space than what a single manuscript would allow.

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 measured over a multidecadal time period. Authors need to recall to their reader that their results apply to 2-month period in summer. 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 (but the DEM data are probably not available. . .).

We actually have dh/dt data that spans 2011 and will include it during revisions. The zone of maximum thinning is in the same location as the dh/dt maps from 1957 to 2009.

General comments for part B.

3/ I miss a more thorough description of and comparison to earlier studies mapping ice cliff automatically. In particular Kraaijenbrink et al., RSE, 2016.

We agree that a further analysis of this nature would be fruitful and interesting. But it seems that this comment contradicts this reviewer's idea that all 3 parts be combined into one. We cannot add more content and have these 3 parts combined into one. Content would need to be trimmed down substantially to fit all three parts into one.

4/ I feel it would have been very interesting to see an evaluation of the ice cliff mapping algorithms using independent dataset, for example the Ragletti/Steiner cliff dataset on Lantang Glacier. Maybe this ice cliff automatic mapping part would have deserved a dedicated article, and all the rest of the results would then fit a single contribution?

We agree that the ice cliff mapping algorithm could be a separate paper in and of itself. This comment directly contradict's Evan's comment though that this ice cliff mapping approach is a minor part of this manuscript. We feel that this new approach does deserve more attention in this Part B and we will bring a bit more discussion of this method compared to others during the revisions.

5/ Uncertainties could be treated in a more systematic way so that results in the end should all be quoted together with their range of uncertainties. This applies to all three parts.

Please note that Figure 10 already includes a generous, extreme range of uncertainties.

6/ When authors provide % of melt, they should always make it clear that this is a percentage of the debris-covered tongue (and not the whole glacier!)

We will clarify this during revisions.

Specific comments.

L16 What does "enhancing" the mass balance mean. A mass balance can be increased or reduced. Is this formally demonstrated? I thought it was debated.

We will change ‘enhancing’ as suggested. We need to be more clear about what we mean. Because melt rates of ice cliffs are most certainly higher than sub-debris melt rates (where h_debris > 5 cm), the ice cliffs tend to move the surface mass balance more towards what melt rates would be if there was no debris at all. This is the ‘enhancing’ effect we refer too. But we will clarify this in revisions.

L21 “Total” is ambiguous. Tongue-wide or glacier-wide?

We will clarify this.

L41 One does not expect results in the introduction.

We can rephrase this sentence so it does not read like results. But the statement we make is based off of previous work from the area so it is more an observation to set the stage for the rest of the article.

L51 "surface mass balance" would be a more appropriate way to refer to it

Eq 1. x,y are not defined.

L58. So do the authors neglect them? It should be stated unambiguously.

We will state that we neglect them.

L88ff. Splitting the article into three parts leads to many repetitions such as this section. Problematic in my view.

It is true that some repeating is necessary, but at the same time combining all three parts would make a manuscript with 27 unique figures that combines, diverse in situ field measurements, MF correction, a new ice cliff delineation method, extrapolation of melt across a large glacier, discussion of lakes, streams, surface velocities, ice emergence, discussion of the uncertainty of each approach, a plethora of field measurements, and the presentation of what we feel is important and new theory in Part C.

We feel that a little repetition in each part is fine considering that a combined manuscript would require that we fundamentally change in the conclusions we draw. We would have to drop the discussion of surface processes presented in Part C as well as much of the very important field data from Part A.

It is important to also consider the drawbacks of combining the parts into one as well.

L160. Unclear (understated) what meteorological data would have brought, if they had been available.

This is more pointing to differences in our approaches from many debris-cover studies today. We can clarify this.

L169-170. This statement that 20% of ice cliff area need to be added is enigmatic at this stage in the paper.

We will clarify this.

Eq (3). How the type of fitting curve was chosen? It seems to come from nowhere. Can it be justified?

We will provide a proper method of justification for the curve fitting based on error metrics. Ultimately, the details of the shape of the curve are secondary (linear or non-linear) to the melt suppression effects of debris.

L183. I see in part A that your cliff backwasting rate neglect emergence velocity. This needs to be justified.

It is not clear what the reviewer is referring to here. The emergence velocity is not relevant to our backwasting rage here because we measured the rate in situ, on the glacier.

L191. A statement such as (here) "based on an analysis of 2-m ArcticDEMs" is too vague.eq (7). Ice ciff area. Is it planar or real area? I think "i" must be added as superscript with bˆdot debris and bˆdot icecliff

We will clarify and expand on this.

L204. I do not understand why fitting a curve through 25% or 75% of the data points leads to "extreme" cases. Not clear. Why not a curve containing 67% of the data (to have 1-sigma uncertainties). See my general comment about treatment of error bars.

We just need to add to line 204: ‘for each dataset.’When every dataset is given 25 or 75 % uncertainties and then we use the relevant extreme curve fit for our estimation of melt across the study area an ‘extreme’ case results. This needs to be more clearly stated and emphasized in the manuscript. There already is a good uncertainty analysis here, but for some reason that was missed. For example these ‘extreme’ cases where something like 5% of possible cases are outside of the red band in Figure 10a and 10b.

We could use 1-sigma bounds but the ultimate results will not change.

Here, while we aren’t using 1-sigma uncertainties we do provide a justifiable error analysis it just needs to be emphasized more.

L226. "error checks" is a strange terminology. Why not "validation dataset"

We are happy to change the terminology.

L240. Percentage should be 21% and 31%, right?

Yes we can fix this.

L244. Where does “11.6%” come from? I read 11.4% and 11.7% above.

L245. This raise the question of whether all studies defined the "debris-covered tongue" the same way. Did the authors check carefully previous studies for this aspect?

We used the same study area, but will check the numbers in codes again.

L247. “This implies that ice cliff coverage varies with debris thickness”. This seems like a hasty conclusion. . . other example from the literature to support the statement?

There are not many studies that quantify ice cliff distribution and we don’t see anything wrong with highlighting what ‘could’ be an interesting trend. Perhaps we could change the sentence to:

“This implies that ice cliff coverage could vary with mean debris thickness”.

Since this is the first study

L257. One expect an error quantification for each term (81% and 19%).

The next sentence does just this for the 19% number, which also does the same for the 81% number so the error quantification is already present though it might not be in the expected format.

L268. “Across all of the elevation bands, the ice cliffs between 500 and 520 m generate a maximum of 40% of the total mass loss due to ice cliffs and sub-debris melt.” I am not sure I got the meaning here. Maybe reformulate for clarification. (it is clear from the figure, just a text improvement)

We will fix this.

L273 "within" rather than "with" (I think)

L289. 19%. Lack error bars and also authors need to remind that this applies to a short period of time during summer 2011. So they cannot draw such broad conclusion.

We do not want to overwhelm the reader with uncertainties in every sentence. But the uncertainties are presented in the manuscript. We will highlight them more in revisions.

We will add in additional dh/dt data that spans the 2011 summer. We also make, what we think is a compelling argument in section 4.2.1 for why we do not expect the mass balance pattern to not primarily follow Ostrem’s curve.

I would be curious to see a comparison of this number to the total glacier-wide ablation during this period if available. Is not it just a few percents? Do we need to really worry so much about ice cliffs for glacier-wide or region-wide application (and future projections)?

We could add in this analysis but we aren't sure how it would improve this study and the aims we outlined in the introduction. As we framed the study, we are interested in explaining the thinning patterns of debris-covered glaciers, and Kennicott Glacier specifically. Ice cliffs are an important, proposed contributor to this. Thinning patterns hold implications for longer term thinning patterns and hazards.

L307. the SMB cannot be "suppressed". It can be increase or decrease. (SMB is increased here, or less negative)

We will fix these terms.

L321. "This required backwasting rate is well beyond potential biases introduced due to the summer of 2011 having anomalously low air temperatures". Statement not really explained and justified.

What we mean is that the weather during the summer of 2011 was not anomalous in a way that would change the melt rate pattern we present in this study. We will make it more clear with further analyses.

L324. Is this potential overestimation from the sampling strategy (at top of cliffs) included in the error bars, as it should?

The assertion in the text as it stands is logically correct and we feel is a better way of arguing than adding error bars these error bars. We aren't sure how following this suggestion will actually improve the error estimation beyond what we already have presented. We could include error bars for each backwasting but how would that improve the legibility of our figures? How much of an over estimate is it? We do not know, so where do we end the error bar?

Rather if we know from other approaches that these are maximum estimates we can use that fact, as we do already in the discussion to present these ice cliff backwasting rates as generously high.

L334. "mass loss" should be replaced by "melt rate" here.

L344. I did not get the point here.

This lays the foundation for Part C. We will emphasize that here. In Part C we use the melt rate pattern developed in Part B to make further theoretical arguments about the interaction of surface mass balance, ice dynamics, and surface processes.

L349. The wording suggests that 11.7 % of the glacier is covered with cliffs. No. This is the % of the debris-covered tongue.

Table2. For the ice cliff backwasting parameter f, the most likely value is not contained by the min/max interval. A typo? Or a real error? For the ice cliff area the most likely and max values are equal. This is also not really expected neither.

Thank you for the comment but this is not an error. We point the reviewer to parameter 'g' immediately below parameter 'f' which is the y-intercept for the curve-fit. If you also look at Figure 4 you will see that these parameters are correct.

Table 3. Ice cliff fractional area, a percentage of what total area?

% of debris-covered glacier area. We will clarify this.

L415. I do not understand this note.

This note should be removed and will be in revisions.

Figure 2. Are these data from Das et al? Did they use GDEM V2? This would be problematic because it has no defined time stamp. Explain the 1957 and 2015 grey boxes also.

We will look into the time stamp issue and make sure that the profile we show has a time stamp. Ah the boxes are the extent of ‘continuous’ debris cover upglacier from the terminus. We will add this note.

Figure 4. Multiple reference to part A complicate the reading.

There is only one reference to Part A. I am not sure where the reviewer got this from. I am not convinced that referring to another paper to read about the in situ collection methods is really a big issue. It actually makes Part B more clear and less muddled. We find that when too many methods are presented in a single paper it becomes exceptionally hard to follow the main thread of the manuscripts. This is a major negative for combining these manuscripts.

But we will clarify what the reader can look for in Part A so it is more clear that it is the in situ data collection methods. Emphasizing again that all the reader needs to do is look in another manuscript that goes into detail about the in situ methods.

25 and 50% or 25 and 75?

This is a typo, sorry.

Is it "elevation bins"?

We will clarify this.

Panel B. Why the order of values in both axis are reversed. Why not showing the Ostrem way?

As we state below in the caption: ‘The axes are flipped to be consistent with other figures.’

Authors could refer (in the article, not here) to a
compilation by Kraaijenbrink et al., 2017 in their Nature study.

Figure 5. Can the authors show the location of this small area of the glacier?

Yes, we will add this.

Figure 6. Impressive maps.

Thank you, we feel that this figure shows the viability of our ice cliff detection method on a very complex example case.

Figure 8. Showing percentage for panel c (instead of fractional area) would facilitate correspondence with the text.

We agree and will change this.

Figure 9 The sign only make sense if this is referred to as “surface mass balance rate”.
If the word “melt” is preferred then positive values should be shown.

We will fix this.

Figure 10. See comment on Figure 9 for "melt"

We will fix this.

Figure 11. Recall the period of dh/dt. In panel b rather than repeating dh/dt authors could show a map with the density / m 2 of ice cliff.

We will show a shorter dh/dt period that covers the 2011 span. The pattern is similar. The cliff density suggestion is helpful, thank you.

Thank you again for your efforts reviewing this manuscript.

In addition to the changes above we suggest that these changes are enacted:

Part B: proposed changes

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 B:

- We will add additional text supporting the usefulness of our new ice cliff detection method. In the supplemental we will include additional satellite photos showing how ice cliffs tend to be darker than the surrounding debris so this method can therefore be applied on other glaciers. We will also compare our method with other approaches from other glaciers.

- We will present new DEM differences from 2007 to 2013. These dh/dt data show that the zone of maximum thinning remains in the same spot as for the period from 1957 to 2007. We will also include additional laser altymetry data from 2007 that shows a similar thinning pattern.
    -This will address one of the main criticisms from multiple reviewers.

- We will introduce back-of-the-envelope calculations of the possible effect of englacial melt, sub-glacial melt, melt under pond surfaces, and melt by streams. This will clear up any issues related to this manuscript not being comprehensive with regards to melt hotspots.
    - We will not include stream digitizations in this manuscript because we cannot possibly digitize all streams on the glacier surface (imagery is too coarse). The streams play more into the feedbacks in Part C. We will instead make arguments about the surface area coverage of streams and their plausible effect on surface melt.

- We will use a uniform curve fit through the ice cliff backwasting data. And also explore the effect of other curve fits, producing different figure 10a and 10bs which we will put in the supplemental and discuss in the main text.

- Add in the paragraph description that links each of the three papers and helps guide the reader through each manuscript.
- Make sure it is clear how generous the uncertainty estimates already are in this paper. One of the reviewers missed these error estimates completely.
- Emphasize the increasing importance of ice cliffs under thicker and thicker debris.