

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

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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.

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
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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. 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.

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. 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.

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.

4/ As 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.

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.

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

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

L88. "based off of" (?)

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. (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)

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.

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

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

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.

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

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

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artificial separation in three papers does not work.

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, emergence velocity and dh/dt also. There is nothing really new here. 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 nowadays is probably inherited from decades of imbalance.

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.

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.

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

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.

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.

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