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The Cryosphere Discussions

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

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

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.

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. 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. 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.

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. . .).

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.

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?

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.

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!)

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.

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

L41 One does not expect results in the introduction.

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.

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

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

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

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

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

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

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.

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

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

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?

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?

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

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)

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. 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)?

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

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.

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

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

L344. I did not get the point here.

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.

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

L415. I do not understand this note.

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.

Figure 4. Multiple reference to part A complicate the reading. 25 and 50% or 25 and 75? Is it "elevation bins"? Panel B. Why the order of values in both axis are reversed. Why not showing the Ostrem way? 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?

Figure 6. Impressive maps.

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

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.

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

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.

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