Reply to Review 2 Part A

Review of 'Debris cover and the thinning of Kennicott Glacier, Alaska, Part A' by Leif Anderson et al., under consideration for The Cryosphere

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

The manuscript by L Anderson, et al., presents a variety of field measurements on debris-covered Kennicott Glacier, and characterises the debris properties and melt rates under debris or at ice cliffs. These data are an extremely useful contribution to understanding of debris covered glaciers in distinct settings. Very few measurements of debris-covered glaciers are available in Alaska, despite the extensive debris coverage of glaciers in the region. The data presented cover an extensive set of topics, and will be useful in calibrating and applying models developed for other regions to Alaskan sites.

Although there are only minor points of criticism relating to the data presented, the manuscript at present lacks cohesion. The results from this manuscript are key in laying the foundation for Parts B and C of the study by Anderson et al, but I can't shake the feeling that this would better fit as (largely) supplementary material for Part B, or as a submission to the EGU journal Earth Systems Science Data; the content is unusual for The Cryosphere. In the latter case or if the manuscript will remain as an independent paper in The Cryosphere, I would recommend expanding the discussion of the varied data collected; some opportunities for expanded discussion are identified in my comments below.

Major Points

As a presentation of diverse field measurements, the manuscript lacks a storyline. I appreciate the effort and value of collecting these measurements, but there is no methodological development, and the results and discussion seem geared towards briefly placing the measurements in the context of observations in High Mountain Asia. The few major outcomes (e.g. aspect dependence of ice cliffs) are not investigated or discussed in much detail, as it is very clear that these measurements are geared towards supporting Part B. Consequently, I feel as though many of the results could be included in Part B without a separate Part A; rather by including these measurements as supplementary material, as they follow more-or-less established methods.

The manuscript organisation is awkward at times. In part this is because measurements and results are presented together, but also because figures are not always associated with the text that pertains to them. More problematic is the lack of an integrating discussion – the individual measurements are discussed but there is not much of a summary characterisation of Kennicott. I appreciate that this is difficult to do from such diverse field measurements. Again, this is in part because the paper is unusual for content in The Cryosphere, and this is another reason why I think this work could be integrated into Part B (or as a manuscript in the EGU journal Earth Systems Science Data, rather than a distinct manuscript.

Data availability. In the modern spirit of open data, I would strongly recommend that these measurements be archived in an open repository.

Off-glacier air temperatures are used to correct short-period met measurements to the full period of record, but these stations have been shown in this manuscript to represent entirely different altitudinal temperature differences compared to on-glacier stations. The use of the off-glacier stations needs to be robustly evaluated at the stations, and the on-glacier stations need to be used to determine melt factors (for the on-glacier air temperature subperiod). Even if this does notchange the pattern of relative melt factors, this represents a (possibly major) uncertainty in all of the analysis.

Uncertainty in measurements or calculations is not considered at all in the manuscript. Since these measurements are used in two linked following studies, and to draw important conclusions about the dynamics of debris-covered glaciers, I think it is important to frame the results in terms of uncertainty from the start.

Minor Points
L34. 'when thick it supresses melt rates' – although common knowledge, it is worthwhile to specify a reference here

We will add a citation here.

L41. Not just explain but also examine; we have evidence of the 'debris-cover anomaly' in High Mountain Asia but not before in Alaska, to my knowledge.

Here, is an opportunity for us to add more to this manuscript and make it more of a stand-alone contribution as this reviewer prefers. We can add in DEM-differencing data after Das, et al., 2015 to show that the debris-cover anomaly is occurring in AK, and is therefore potentially a global effect. If we added this the manuscript would could have a more coherent story line, despite the fact that much of the data lays a foundation for further DC modeling studies (see David's review of Part A) as well as Parts B and C.

L53. Missing 'glacier' – debris-covered glacier mass balance

L55-64. I agree that Kennicott is an interesting case, and a great opportunity to examine the debris-cover anomaly. However, I don't entirely agree with these two justifications in their present form, possibly because a bit more explanation is needed. The presence of thinner debris means that there is less melt enhancement due to cliffs and ponds (ie they may not melt much 'more' than the subdebris ablation), even if their areal coverage is extensive. Your implied point is that the thin debris should lead to less of a melt difference between clean and debris-covered areas, and so the chance of cliffs/ponds/other mechanisms to make up for this is greater. That needs to be made explicit; at present the second rationale is unclear.

Thank you for pointing this out and David's review has a similar comment, and we certainty need to address this. The point is that with thinner debris and more ice cliffs the net melt rate will be closer to the bare ice melt rate not that hot spots will contribute a higher percentage of melt. What really matters for the thinning of the glacier is the absolute melt rate not the relative effect of hotspots as has been emphasized in the literature.

For the third rationale, it would be beneficial to identify the actual density of ice cliffs in the study area (although this is an output from part B).

We would like to add the actual density of ice cliffs in the study area and we will, but David's review commented that we need to not have results from Parts B and C in the introduction of A. But we simply feel that these contributions are components of a whole and that since these works are being reviewed simultaneously that we can provide full justification from the main conclusions from the other parts.

Readers should not have to jump between the manuscripts to understand the rationale.

We agree and would like to include the results you suggest.

L80. The reference to Mount Blackburn does not fit into the text very well – what is the relevance to Kennicott? Debris supply mechanisms? Lithology?

We will correct this and make its relevance more clear.

L83. The multiple clauses with commas are a bit awkward.

L88. For consistency, this should be debris internal temperature and debris surface temperature.

L93. I suggest changing 'vary' to 'differ'. Boundary layer conditions also vary widely for debris-free glaciers, and for debris-covered glaciers; without a doubt there is overlap in this variability, but the distributions of conditions differ, which is your point.

L106. It would be good to include a very brief description of this important transition, or to simply state that this location is at the base of a prominent bulge. It would also be useful to refer to readers to a more specific area of Part C.

Yes we will include a more detailed description where to look in Part C. We would like to apply this concept through all 3 parts. This will make the three parts mesh better together.

L107. These lapse rates are extremely steep, which makes me wonder if the positions themselves are sufficiently representative of the glacier surface. As elevation tends to be a less direct control on air temperatures over debris, I would recommend fitting the regression to all three observations at once (rather than a 2-step regression).

This is a very helpful suggestion thank you we will implement this.

It is highly likely that topographic prominence and proximity to water are both controls on both wind and air temperature over debris (e.g. Shaw and Steiner publications, also Miles et al, 2017 [Frontiers], Supplementary Material).

These are great suggestions, thank you! Here we really highlight the proximity to wide stretches of bare ice, which is the case on the Kennicott, unlike many previously studied DCG. We will discuss these though in the revised manuscript though. We agree that this should be discussed.

L114. 'was' should be 'were' as LRs is plural.L128. It is not clear from Figure 2 which are the 109 locations with debris thickness measurements, as there are more than 109 points when combining sub-debris melt, ice cliff backwasting, and debris temperature.

We will make this more clear in the revisions. Adding in more clear maps.

L130. It would be good to identify these thinner debris positions (especially those with multiple measurements) spatially in Figure 2, rather than just with elevation.

Yes, we can do this if it won't make the figure too complex. We will try.

L136. The presentation of these data seems to occur with Figure 7, which is not mentioned here but is quite a jump through the paper.

True we will fix this issue.

L140-142. Were repeated subdebris melt measurements made at the same positions? Did the debris thickness change when re-exhuming the stakes? What uncertainty is there in your debris thicknesses or melt rates due to the removal and reburial of debris? (Especially if this occurs repeatedly). A key consideration is that supraglacial debris often presents as sorted, but it is extremely difficult to replace debris in the same state which it was found. This of course is not a problem unique to your measurements, but it should be acknowledged and considered.

Yes this is an issue to all sub-debris melt measurements. We only dug a couple of poles more than twice, but we can emphasize this in the manuscript.

L145. This melt factor determination negates SW and LW inputs (and their variability), which may be very important for debris covered glacier surfaces (e.g. Reid, Steiner, Buri ice cliff studies, also Carenzo et al 2016). Although this may not affect your overall results in terms of total melt, it will definitely affect the aspect dependence of subdebris and ice cliff melt. Also, this is clearly determining the mean melt factor for each location; how variable were different melt subperiods for each site?

To us, the melt factor (MF) approach we already use includes these aspect effects. If the melt is higher in a southerly direction then the MF would be higher. If a north facing ice cliff retreats slower than the MF would be lower. The SW and LW effects may be able to produce more accurate estimates of melt but that would play more of a role if cloudiness changed and the relative effect of SW to LW fluxes changes. But a simple MF approach would also include these effects if the relative effect of SW to LW changes as well.

Our approach here is not to use the most sophisticated melt model possible, that requires may more data input (we are in a relatively data poor region for glacier studies and have no access to these fluxes locally) and increased constraint of parameters. The simple approach used here is effective, and please note how small the corrections are in figure 12. It won't matter which melt model we use the change will be small because we are correcting the rates for a difference of a few weeks between individual measurements. But the differences between melt models is a worthy target of research.

L148-150. Please explain this estimation of T* more clearly. Are you using the LR between the two off-glacier stations to estimate T* at each location? If so, this estimation needs to be further evaluated relative to the multi-step on-glacier LRs (for the shorter period of measurements for those stations), which differ considerably for the environmental lapse rate.

Yes we are using two off glacier meteorological stations which has been shown to provide good estimates of melt. As far as we understand off glacier sites provide a better sense of the temperature of the lowest km of the atmosphere which works well for predicting melt. Note too what we use these MFs for: It is just to correct difference in measurement period so we are deriving MF from a couple of weeks to estimate melt for another couple of weeks. The effect on backwasting rates is very small and does not change the story here or in part B, even if we used a more complex model we aren't convinced that anything would change.

At present, the dependence on off-glacier measurements is not very robust, as your on-glacier air temperature measurements indicate a significant deviation from off-glacier air temperature spatial variability. This will have the effect of smoothing your ice cliff MFs with elevation.

We do not see how this really matters. We use the closest, viable meteorological station. The difference in temperatures observed from the on-glacier stations will be included in differences in the MF between sites. See Flowers article on the use of MF from Canada. That is the advantage of the MF approach. The MF includes all these differences in physical variability. Even if an Energy Balance model includes all the Energy transfer pathways the number of parameters skyrockets such that the issue becomes the constraint of these parameters. If we were using an EB approach then yes we need on glacier temperatures, but we are not and we feel that based on a number of studies this approach is justified. We are happy to further discuss this but are not sure that this really effects our study or Parts B or C.

We could do an analysis of MF in space across the glacier though.

L156-7. This is an interesting comparison, and should be explored a bit in the Discussion. Is this due to latitudinal controls on Ta or SWin? Presumably these glaciers have differing lithologies, and they certainly differ in climatic setting, so perhaps this is a coincidence? I note that there is still a factor of 2 difference between the other glaciers.

We agree that this is interesting and will expand on it in the discussion. But it is not clear that we can pull out causality based on available data.

L161. This is not shown in Fig 2.

We will add the locations.

L176. Please justify the use of a linear extrapolation to surface temperature, which differs from interpretation of many debris internal temperature profiles I've seen (often an exponential form is noted when there are sufficient thermistors).

When integrating over more than a week the temperature profile becomes linear when heat is transferred by conduction (Conway and Rassmussen, 2000). We will emphasize this here.

It would also be good to include 1-2 plots of the internal temperatures – diurnal variations and means.

Yes we can include this.

L181. I have some qualms with the 'non-linear' increase, which is only because you have imposed (0,0) as an additional point for your fit. Surely, an infinitesimally small debris thickness (which is of course unrealistic) should converge on the thermal conductivity of the rock material itself (i.e. no longer an effective conductivity, but the true conductivity of the material). If you neglect the (0,0) point, this looks most like a linear trend crossing the x-axis at about 0.4 W (C m) -1 . Also, I think that the non-linearity, if true, needs more consideration and discussion – what are the effects of sorting, for example? Does this imply a bulk density difference between the upper and lower debris layers?Also, what do you expect conductivity to look like for layers thicker than 1 m (e.g. these would exceed the range estimated by Nicholson and Benn (2006).

We will take another look at this and the literature as well. But we agree that we need to be sure here and remove the 0, 0 point. But in the end this does not have major implications for the rest of the work, rather for future melt modelers.

L199. It would be good to show the distinct lithological mixes in Figure 9.

Hmm… we did not do a distinct analysis of lithology but we can extrapolate from geologic maps and our field descriptions.

L205. Please indicate the accuracy of the Fluke Infrared Thermometer.

We will add this.

L204-208. This section does not clearly follow the past sections, and also does not integrate very well with the rest of the study at present.

Thanks for pointing this out. We can add context here to explain why we took these measurements.

L216. Did you classify cliffs based on the presence of streams as well? Part of the results of Brun et al (2016) and others is that any moving water can have the same effect as ponds. In my opinion (not demonstrated) supraglacial streams are even more effective cliff maintenance mechanisms.

We did take notes on the presence of streams at the base and will add that to the manuscript.

L223. It is worth considering these climatological and latitudinal controls in slightly more detail. Is Kennicott really cloudier in the melt season than Lirung (site of Buri and Pellicciott, 2018)? The latitudinal control is not unexpected, but deserves more consideration. Effectively, during the ablation season there should be less diurnal variation in solar zenith angle at high latitude (solar zenith and azimuth are of course correlated seasonally at any latitude).

We will consider it but there are a number of variables to consider here to make conclusive statements. We will try but not dwell on this point.

L233-234. Both instances of 'effected' should be 'affected'.

L264. Are these the (unmodified) measured melt rates or your estimated melt rates from section 2.3?

It doesn't matter which. We will include a plot of measured versus corrected melt rates in the supplemental. The points virtually plot ontop of one another the changes are tiny.

L265. The comma here is awkward. Perhaps use 'as compared to'

L273. This was only demonstrated for north-facing cliffs in Buri et al (2016b).

L282. I agree that the representation of air temperatures from off-glacier stations is not robust. This deserves careful comparison of estimated air temperatures from lapse rates derived from your on-glacier stations (for the shorter period) before an extrapolation across the glacier. More importantly, this could lead to a major uncertainty in your MFs for both debris and cliffs, even if the patterns do not change with more realistic air temperatures. At the very least an evaluation of the accuracy of the off-glacier stations for representing the on-glacier observed air temperatures is needed.

We disagree. There is no need for the off glacier temperature to be compared to on glacier sites. MF are relative parameters only relevant to the air temperature measurements. In addition we could also not do the MF correction and the melt rate results would be almost the same.

As long as the MF derived from the air temperature data we used is then used with air temperatures from the same stations the MF extrapolation is viable. We feel that this point is over emphasized. MF and lapse rates are only relative to the temperatures at station.

See Wheler et al., 2014: Effects of Temperature Forcing Provenance and Extrapolation on the Performance of an Empirical Glacier-Melt Model

L304-307. This list of summary statements is not terribly satisfying, and feels like a list of bullet points. More interesting is whether Kennicott's debris properties generally fit within the range of previous distributions (they seem to) which is meaningful as there are few published debris properties in Alaska generally. At the very least, it would be nice to have some numbers in the text?

We can add more context for the measurements made here with other studies globally. And emphasize that the results are not so different from other regions, if that is indeed the case. We note that these are the first measurements of this kind made in Alaska to date.

Table 1. The estimated debris surface temperature difference is not described in the text.

We will add that in.

Table 2. I would describe the contents of this table as 'measurements' rather than 'variables'.

We will change this.

Table 3. It seems odd to choose Buri and Pellicciotti (2018) to represent Lirung, as that study was primarily modelling synthetic cliffs rather than reporting backwasting measurements. I think the most appropriate study here would be Brun et al (2016).

Ok we will change this.

Figure 1. At what interval are these contours?

Figure 2. It would be useful to identify the sources and dates of the WV and aerial imagery in this caption or in the text.

Figure 3. I like this schematic, but it's not quite complete: missing are the thermistor strings and air temperature measurements (possibly others). Also, it would be fantastic to include some field photographs demonstrating the measurements.

Figure 4. Since you rely on the May Ck and Gates air temperature measurements, it would be very beneficial to show them here. Perhaps it would also be possible to combine panels (a) and (c), and (b) and (d).

We feel that combining the panel will make an unintelligible figure. We could add in the off-glacier data but we aren't sure how it really matters. MF are all relative to the temperature data they are derived from as long as data from the same stations used to derive the MF is used for extrapolation the principle holds. There is no absolute MF.

Figure 5. Can you indicate the lithology of the debris thickness in panel (a)?

We did not do a detailed lithology analysis, though we noted major differences in lithology.

Figure 6. This seems to be referred to out of place in the text. Also, I'd suggest switching the axes (so that elevation is the y axis) for easier comparison with Figures 1 and 5.

We could also just switch the x-axis to distance. But we will take a look.

Figure 7. I didn't catch a description of the bare-ice melt rate – what elevation was this at? In addition, this content is almost entirely repeated in Figure 8, so I'd suggest eliminating the figure, but depicting the bare ice melt rate in Figure 8.

We will clarify bare-ice melt rate. We could just make this a 2 panel figure 7 and 8 but this is a very minor change. If figure 7 was dropped it would very easy to desire a panel that is not comparative.

Figure 9. As described with my comment on L181, I don't think the point at the origin is justified, in which case a linear fit is entirely appropriate. Also, I'm a bit disappointed that we don't see any of the thermistor data!

We will re consider the non-linear increase as suggested. We are happy to show temperature profile data but we aren't sure what it adds. But we can show the exponential funnel!

Figure 10. I would suggest to merge this with Figure 9, as the content is very closely related. Also, I note that the units here (m 2 s -1) differ from that in the text (mm 2 s -1).

Also ok to keep them separate.

Figure 11. Over what time period were these temperature measurements taken?

From 10 am to 4 pm. We will add that in the caption.

Figure 12. Is it possible to identify the cliffs that bordered ponds or streams within one of these panels?

Yes would be neat to see and include.

In addition to the changes proposed above:

Part A: 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 and create more of a storyline we propose that we add these additional datasets/ideas to Part A:

- Provide error bars for the data, if the plots are too messy we will but the figures with the error bars in the supplemental. Noting that these uncertainties are all less than the extreme uncertainty presented in Figure 10a of Part B.
- A detailed analysis to explain the scatter in the ice cliff backwasting rates and meltfactors.
    - Do they correlate with local debris thickness, streams, or lakes?

- Make a comparison of our in situ data with data from else where, likely showing that they are consistent
    - This is important for the important global studies that will be coming out related to debris cover.
    - We will make a broad characterization of the Kennicott Glacier in relation to other glaciers

- Global debris cover anomaly. Highlight that the debris cover anomaly is likely global. We will do this with long profiles of dh/dt from multiple glaciers in the Wrangell Mountains and their debris cover extent. One figure will be added that shows multiple thinning profiles. One table will be added that further shows this. Since the dh/dt data has already been published by Das et al., 2015 we will use this figure as motivation for the individual parts.

- We also have additional data related to the geometry of the ice cliffs that we measured. We will put these data in the supplemental of Part A.

- Add in the paragraph description that links each of the three papers and helps guide the reader through each manuscript.