

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

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 not

change 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 suppresses melt rates' – although common knowledge, it is worthwhile to specify a reference 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.

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

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

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.

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). 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).

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.

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.

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.

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.

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?

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. 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.

L156-7. This is an interesting comparison, and should be explored a bit in the Discussion. Is this due to latitudinal controls on T_a or SW_{in} ? 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.

L161. This is not shown in Fig 2.

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

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)).

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

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

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.

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.

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 Pellicciotti, 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).

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?

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.

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?

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

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

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).

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).

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

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.

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.

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!

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 ($mm^2 s^{-1}$).

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

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