Review of "Debris cover and the thinning of Kennicott Glacier, Alaska, Part A: in situ mass balance measurements" by Anderson et al.

This study is the first part of three publications that investigate debris cover on Kennicott Glacier in Alaska. Given the limited number of studies that measure properties and melt rates of debris-covered glaciers, these measurements and results are important for advancing our understanding of debris-covered glaciers. This is especially true when one considers the limited knowledge of debris-covered glaciers in Alaska. The measurements and results are presented well. For the most part, the study is easy to follow, well-written, and has sufficient references.

There are a few sentences/paragraphs that could be modified to improve their readability though. The only major comment is to make sure that this study is discussing results that specifically pertain to this part of the three-part study. There are also a couple places where additional detail or analysis would provide useful context to the modeling community; however, this would only require minimal additional work. Therefore, I recommend accepting this manuscript for publication subject to minor revisions. Please see my detailed comments below.

<u>Main Comments</u>

The reasons for studying Kennicott Glacier largely come across as reporting results across the three papers as opposed to stating what each paper does. For example, L55-57 state that the debris is thinner than most previously studied, but there is no reference to any studies concerning debris thicknesses on Kennicott Glacier. Similarly, L58 states there are more ice cliffs than those previously studied without a reference to a study that shows this. Hence, these appear to be results (and results from other papers) that are stated in the introduction.

Furthermore, the introduction states multiple times that the thinner debris increases the likelihood that melt hotspots will compensate for the insulating affects; however, thinner debris has higher melt rates, so it's unclear why melt hotspots would be more important for debris-covered glaciers with thinner debris because there would be less contrast between the sub-debris and ice cliff melt rates. If this is a hypothesis, then please state it this way. If this is supported by a physical basis, then please explicitly state this reasoning.

Lastly, the interpretation of the transverse variations of debris thickness appear to be poorly supported by the present figures and text. L135-139 state that mean debris thicknesses increase near the glacier margins. However, site a appears to be closest to the center of the glacier, yet it has thicker debris. Similarly, site c is between sites b and d. Perhaps this is complicated by how far downglacier these sites are, but this needs to be elaborated upon. The same is true for the conclusion, where this is discussed. I would suggest removing this from the conclusion.

<u>Specific Comments</u>

Italics indicate suggested grammatical changes

L26 - use of "thick" and "thin" is a relative term. I suggest adding in parentheses what constitutes thick and thin.

L35 – consider "and, when thick, suppresses melt rates." or "and suppresses melt rates when thick."

L39 – this sentence is missing its subject, so it’s an incomplete sentence. Consider using a semi-colon instead or adding the subject “Alternatively, this anomaly could be caused by…”. Also, “or” and “alternatively” are repetitive.

L41 – referring to the debris-cover anomaly here almost across as a result, i.e., Kennicott Glacier experiences the debris cover anomaly. If this is already known, then the reference should be added. If this is not known, then consider changing this sentence to give a broader overview of what’s being done, e.g., constrain patterns of … to understand the role of surface melt and ice dynamics on the surface lowering of Kennicott Glacier.

L55 – it’s not entirely clear why thinner debris would affect the anomalous glacier thinning explained by melt hotspots, since thinner debris will have melt rates that are closer to clean ice. Also, are there previous debris thickness measurements of Kennicott Glacier? If so, this should be cited; otherwise, the fact that Kennicott Glacier has thinner debris than those previously studied is a result.

L60 – It remains unclear as to why thin debris increases the likelihood that melt hotspots will compensate for the insulating effects of debris. Conversely, the way the argument is stated sounds like melt hotspots cannot compensate for the insulating effects of debris on glaciers; however, because the debris on Kennicott Glacier is thinner, the sub-debris melt rates are closer to clean ice melt rates and hence the melt hotspots are less important because there’s less of a difference to compensate for. The key here seems to be more on the sub-debris melt rates of thin debris than the melt hotspots. Please clarify this.

L61 – typo “similar” should be “*similar”*

L72 – typo in the reported elevation range? Also, is there a reference for this data? RGI inventory perhaps?

L73 – consider “… and our study area, the debris-covered tongue of Kennicott Glacier (24.2 km$_2$), is only…”

L77 – be consistent with reporting elevations. Perhaps “Above 700 m a.s.l.”. This should be done throughout the manuscript as well, e.g., L90, L131, L134, caption of Figure 1 “located at 1240 m a.s.l.”, etc.

L77-79 – is there a reference for these observations?

L86 – What do you mean by “Kennicott Glacier debris”? The debris properties? If so, state this “Because the debris properties of Kennicott Glacier have not been…”

L88 – consider “internal and surface debris temperatures, and …”

Figure 1 – delete the ")" after panel b in the caption. Change to elevations to m a.s.l. May Creek meteorological station is not shown on the map. I suggest adding this – perhaps it is covered by one of the legends.

Figure 2 – caption is unclear. "Dead" ice portion has daily mean surface velocities greater than 5 cm d^{-1} only during sliding events? Is this meant to be less than 5 cm d^{-1} with the exception of sliding events? Also, what does "and the observations of Rickman and Rosenkrans, 1997" refer to? Fix this reference.

L107 – Avoid the use of unnecessary acronyms like *LR* for lapse rate. This only makes the manuscript less readable, especially for readers who may not be as familiar with a specific acronym.

Figure 4 – The 4 panel figure is highly repetitive (e.g., shortwave radiation is shown in all 4 panels, and the MWS air temperature is shown in both panels). I would recommend using only 2 panels. Air temperature can easily show the 3 sites, and the two lapse rates can easily be shown on the same figure by using different colors or styles.

L128 – This line doesn't make sense "at 109 locations at the same locations we also measured". Is it means to be two sentences? Otherwise, perhaps "around the locations where we measured …".

Table 2 – is 0.001 cm an actual measurement? That is incredibly precise and thin for a debris thickness, which is hard to believe.

L135-139 - It would be helpful to provide context to the specific sites (panels) for each of these sentences, e.g., "debris thickness did not exceed 15 cm (Fig. 6c)"

L144 – Given the use of MF (used by Pellicciotti et al. 2005) instead of DDF (used by Hock 2003), I would consider either changing the "MF" to "DDF" or add the example citation of Pellicciotti et al. (2005). Note that in some fields MF or DDF could refer to multiplying multiple variables. I leave it up to the authors as to whether they want to maintain this original convention or adopt newer uses of it (e.g., degree-day factors shown as f_{ice} (Radić and Hock, 2011)).

L148 – Why the use of off-glacier air temperatures when you have data from on-glacier air temperatures? It would be interesting to see the off-glacier air temperatures over the same period of time – perhaps this could be added to Figure 4 as this would provide some indication of how much the debris warms the air temperature?

L153-157 – Given the impressive amount of data collected, it is disappointing that the authors do not provide a "best-fit" Østrem curve for comparison with other sites. While there is considerable variability in surface lowering, especially over thin debris that is dependent on local conditions as the authors state, this is clearly something that would affect all previous curves. Is there a good reason the authors did not do this? This could be a highly beneficial product for modelers. If uncertainty is the issue, the authors could easily add uncertainty bounds to the curves.

L176-177 – What does the “mean” debris surface temperature refer to? Is this the mean temperature over the entire study period (at least one week) or was this used to estimate conductivity on a shorter time period? I assume it is the former, but it may be good to be explicit, e.g., “… we then calculate K_e for each temperature profile *over the entire duration of the temperature measurements.”*. This would avoid any misunderstandings because the effective thermal conductivity could vary over time, e.g., if there was a change in debris moisture.

Figure 9 – Why is there a point for a debris thickness of 0 with an effective thermal conductivity of 0 W C^{-1} m^{-1}? This seems to be unphysical. I also question the “nonlinear” increase in thermal conductivity as a function of debris thickness. There appears to be a fair amount of scatter such that a linear fit might also produce a reasonable fit? Furthermore, if the (0,0) point is discarded, then the linear fit will likely cross the x-axis around 0.4 – 0.5 W C^{-1} W^{-1}, which is near the lower range of that estimated based on physical constants (L181; Nicholson and Benn, 2006). Hence, this would be more physically based. Lastly, why is thermal conductivity plotted on the x-axis? The way this is used in the statement seems to be how thermal conductivity varies due to debris thickness and not the other way around. Hence, the debris thickness is the independent variable (typically plotted on the x-axis) and the thermal conductivity is the dependent variable.

L181 – I question “The apparent non-linear increase”. See comment above. It would be good to at least see a linear fit as well.

L182 – typo, “*may be* due to…”

L185 – it would be valuable to make assumptions concerning the specific heat capacity and porosity such that a comparison could be shown for the differences in thermal conductivity based on the method.

L206 – type “*were* made…”

L205-206 – were these debris thicknesses already known from the previous debris thickness and ablation stake measurements or were these new measurements? Furthermore, how many “data points” were collected?

L214-218 - why the switch from backwasting rates to backwasting melt factors? It would be easier to read if it were consistent.

Figure 12 – caption, “based *on* the individual melt factor…”

L227 – shouldn’t have to restate acronym, although see previous comment about removing it altogether.

L233 – “related *to* the large areas…”

L245-248 – consider changing these sentences so that two sentences in a row don't start with "But…" as this should make it easier to read and understand.

L255 – Please state the percentage of debris thickness measurements that were derived from the top of ice cliffs to provide the reader with some sense of if this was for 50% of 100% of the measurements. "The majority *(X%)* of our debris thickness measurements…"

L278-279 – This sentence about Part B is confusing. What does estimate if ice cliff melt rates correspond to the location of maximum thinning under thick debris on Kennicott Glacier mean? Is "under thick debris" meant to refer to the debris-covered glacier? A specific part of the glacier? Or literally the areas where the debris is thickest? I assume this is generally referring to the debris-covered glacier, but please clarify to avoid confusion.

L281-285 – Is (1) different than (2)? Or is the poor representation of air temperature due to using the off-glacier meteorological data, which does not account for the variations in air temperature above the debris? Also, having sentences in the middle of these various points is very hard to read. I would suggest making these three separate sentences.

L285 – What does this sentence of the portion of fine material have to do with ice cliffs? This seems very out of place and appears to refer to the section on thermal conductivities.

L297 – missing Oxford comma, which seems to be used throughout the rest of the manuscript

L300 – "transverse debris thickness patterns broadly correspond with surface velocities" is out of place and perhaps meant for paper B or C. This paper showed no data on surface velocities.

L302 – may want to acknowledge the limitations that were described in the discussion, i.e., that most debris thickness measurements were from on top of ice cliffs and so caution should be used when using these for tuning and validating distributed debris thickness estimates as they may underestimate the actual debris thickness.

L305 – reconsider "non-linear" relationship. See comment above. Furthermore, is the larger point that "water" or "porosity" plays an important role in heat transfer? They are certainly related to one another, but most of the discussion seemed to focus on the role of finer debris and porosity. This should be consistent in the conclusion.

L308 – there is no evidence in this paper that the ice cliffs counteract the insulating effects of thick debris. More appropriate would be to summarize how the backwasting melt rates compared to the sub-debris melt rates. If this is a conclusion from Part B, then it belongs in that paper.

<u>References</u>

Pelliciotti, F., Brock, B., Strasser, U., Burlando, P., Funk, M., and Corripio, J. (2005). An enhanced temperature-index glacier melt model including the shortwave radiation balance: development and testing for Haut Glacier d'Arolla, Switzerland, *Journal of Glaciology,* 51(175):573-587.

Radić, V. and Hock, R. (2011). Regionally differentiated contribution of mountain glaciers and ice caps to future sea-level rise, *Nature Geoscience*, 4:91-94.