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Interactive comment on "Supraglacial debris thickness variability: Impact on ablation and relation to terrain properties" *by* Lindsey I. Nicholson et al.

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GENERAL COMMENTS: Nicholson and co-authors report on the collection and analysis of surface topography and debris thickness data from selected sites across a debris-covered glacier margin in the Eastern Himalaya. Their data set is rich and valuable, and the analysis is reasonable and brings out many interesting features in the data. Two salient implications arising from their work are: 1) ablation rates estimated using mean debris thickness tend to underestimate ablation compared with methods that account for the frequency distribution of debris thickness; and 2) gravitational reworking of supraglacial debris plays a significant role in setting the frequency distribution of debris

thickness. Both of these implications are important for understanding and modeling the role of debris cover in glacier ablation, and for this reason, I think this paper could be an impactful contribution to the literature on debris-covered glaciers. However, in motivating the work and discussing these implications, I think there are some opportunities missed and issues not addressed that could make the paper more robust. For these reasons, I recommend minor to moderate revisions prior to acceptance.

SPECIFIC COMMENTS: 1. There is much discussion of descriptive properties of the debris thickness frequency distributions, particularly their skewness and kurtosis. These are certainly reasonable statistics to derive from populations of thickness measurements, and I can maybe infer their utility for this problem in retrospect. But it is difficult to understand from the introduction and methods why these statistics are reported in so much depth and what meaning the authors expect to convey through them about the processes on the glacier surface or the impacts on ablation. I suspect that (assuming a linear debris thickness axis) the thickness distribution is usually positively skewed in most glaciers, but is there a physical reason to expect that? Under what circumstances, if any, might we expect a negative skew? What could be the physical meaning of a low kurtosis – could we reason that more transport by gravitational failure would result in lower kurtosis? Would predominantly "diffusive" (slope-dependent only, as in landscape evolution modeling) transport result in higher kurtosis? What if gravitational instability were very widespread – would we expect multimodal distributions, with many thin areas that recently destabilized and also many thick areas that received slides/flows from above? Perhaps we don't know enough to address these questions in a formal if-then kind of way, but I do think it would help to build the case for caring about the shape of the distribution, and feel that some practical or theoretical basis should be offered to make the case.

2. This is partly a comment about insufficient information, but perhaps also a missed opportunity to elaborate on the use of frequency distributions to scale ablation rates from Ostrem curves. (Lines 202-205; 358-362). Their approach is described briefly

in lines 202-205, but it took several read-throughs for me to understand what the authors meant in that sentence. This could be made much clearer, and if this approach is widely used (it is entirely possible that I've overlooked something elsewhere in the literature) it might be nice to reference others who have used the method. More significant in my mindâĂŤthough it would be reasonable to argue that this is beyond the scope of the work presentedâĂŤI think an important link between these data and some powerful and more generalizable implications could be drawn here, but is missing. The authors are implicitly using the frequency distribution of debris thickness multiplied by the Ostrem curve as their "best" value for ablation modeling; otherwise they would have said that this method overestimates ablation compared to using the mean, rather than the converse. If they (as I suggest in #1 above) established some reasonable expectations or hypotheses about the shape of the debris thickness distribution, it should be possible to offer concrete hypotheses about how these distributions would affect ablation. Without too much difficulty I think one could establish an a priori expectation that a realistic frequency distribution of debris thicknesses would result in more predicted ablation than if the whole area had the mean of the same distribution. With that as a sort of hypothesis, the significance of a set of measured debris thickness distributions would be more apparent to the reader. I think that if these suggestions were implemented, this paper would read more clearly and could have a more substantial impact. As it is right now, there seems to be only a weak connection forged between the efforts at characterizing debris thickness distribution and patterns and the efforts to estimate ablation rates, and this connection is apparent (at least to me) only after reading the whole paper.

TECHNICAL CORRECTIONS (L = line in manuscript): L77. Add space between "has" and "been". L202-203. "Ablation rate and surface temperature [delete 'is'] calculated for. . ." L287. It is not clear what the "recurrence rates" refer to. Is this the repeated appearance of supraglacial ponds in a particular area? If so, is it distinct pond bodies, draining/refilling of unchanging basins, duration of ponding, or something else? L341. There seems to be a word like "of" missing between "kurtosis" and "debris". L364.

Change "order or the effect" to "order of the effect". L367-368. One could argue that since the use of mean debris thickness seems to consistently underestimate composite ablation rates, it is not worthless but can still have value as a lower bound. Going one step further, even the debris thickness distribution derived from higher-spatial-resolution measurements could include some spatial averaging, so at what point are we looking at a small enough area that refining the resolution even more wouldn't further increase ablation? L499 and L517. A nitpicky stylistic thing, but I dislike the word "shallow" used as the opposite of "steep". I suggest "gentle" or "gradual" instead. Section 5.4. I think the gravitational stability modeling is a reasonable piece to include in the analysis, but it would be prudent to present some assessment of the sensitivity of the model results presented (i.e., areal extent of predicted instability) to unknown values introduced to or inferred from the model, like the ice-debris friction coefficient or debris hydraulic conductivity. These could significantly change the results. Figure 2's caption and the text on line 109 indicate that there should be a panel (b) for Figure 2, but none appears in the copy of the manuscript I've seen.

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