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Interactive comment on "The response of supraglacial debris to elevated, high frequencyGPR: Volumetric scatter and interfacial dielectric contrastsinterpreted from field and experimental studies" *by* Alexandra Giese et al.

Alexandra Giese et al.

robert.l.hawley@dartmouth.edu

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Reviewer 3 (Anonymous)'s REVIEWS ON TCD VERSION

Page 3, Line 10: "...low frequencies irrelevant for efficient areal coverage..." There is no reason why the systems used in the cited studies (and much larger systems) could not be deployed by helicopter (I have a helicopter-slung system more than 20 m long for example).

Fair point, we have stricken this language from the revised text.

"...dragged antennas, an approach that was impossible at our field site..." The surface of Ngozumpa, Lirung and Langtang glaciers are very similar to the pictures of Changri Nup shown here.

Fair enough. We would have gladly used an antenna-dragging approach at Changri Nup if it seemed possible, but it did not.

Page 3, Line 26: "frequency relevant to remote systems..." by which you mean drones. See above comment regarding helicopters, which are readily available for charter in the Khumbu.

We have stricken the relevancy language.

Page 4, Line 11: The frozen ice surface is critical here. By far the largest control on the dielectric contrast in such a setting is the presence or absence of water. It would be much easier to detect the ice-debris interface if you surveyed in dry weather by in thawing conditions (so debris is largely dry but ice surface is wet). This is what McCarthy et al. and Nicholson et al. did.

This is true, and in our Discussion we added that "in the ablation season, [other authors] likely encountered a wet interface, a 'saturated layer' of water on ice, below mostly dry debris. A wet interface gives a much stronger reflection than a dry one." However, it is not possible to repeat our field season in late Spring or Summer; were it possible, we would then have to consider the logistics of a potential snow cover on top of the debris and potential saturated debris (water saturated debris would attenuate the radar signal and, thus, introduce a further complication).

Page 9, Line 3: "Profiles at 50 ns and 100 ns..." Not sure what this means, please clarify.

By this, we meant that the profiles taken over the two along-glacier transects were collected with one of two different time range settings: 50 ns and 100 ns. We have updated the language to "profiles collected at time ranges of 50 ns and 100 ns. . ." to clarify.

Page 9, Line 8 and in general: I don't think the term 'volume scatter' is used correctly. It is normally used to describe the net result of multiple bounces from many discrete, closely-spaced reflectors of similar size to the wavelength, within some medium, which I think is what you have here. In such a case, the interfaces are likely to be air-rock, rock-rock and ice-rock interfaces. I think you're describing it as single-bounce reflections back from individual point scatterers within the debris layer, which is analogous to detecting an aircraft in flight (which clearly is not volume scatter). If it was dominated by single-point scatterers then you'd expect to see hyperbolae in the unstacked radargram (e.g., Figure 5) resulting from the radar moving closer to then further away from the point scatterer, but I don't see these. It's also not clear why the signal would penetrate through the debris above but reflect from these particular buried scatterers.

We agree on the definition of "volume scatter." The language in question that caused confusion over our use of the term has been removed in the rewrite.

Page 9, Line 9: "The volumetric backscatter is dramatically stronger than the surface return..." But the surface return is obscured/interfered with by the direct wave, so it is not clear what the surface return strength is from the field data. Indeed, the lab experiments show the surface return at least as strong as the volume scatter (Figure 10).

That's entirely correct, we can't say this for sure. We have deleted this statement from the revised version.

Page 11, Rock box experiments: these are really nice and I'd like to see more, with the aim of characterising the attenuation and ice-surface detectability. I suggest: Increasing the debris thickness (round trip of 57 cm is thickness of only 28.5 cm, which doesn't capture the potential range of thicknesses in the field). Vary the thickness and measure how the aluminium-base signal strength varies, to characterise attenuation with depth. With the pine base, try wetting the pine (by pouring water in at the base,

while keeping the debris largely dry) to simulate a thawing ice surface. Try the above but wetting the debris from above, to simulate rain or snowmelt. Repeat the above with a range of GPR frequencies.

These are great ideas and could form the basis of a whole paper in itself, but we feel they are well beyond the scope of this manuscript. The point of the rock box experiments was mainly to see (1) whether signals did penetrate at least to the average depth and the dryness found in the field and (2) whether that produced a bottom reflection. We clarified the aims of our study, including the purpose of the rock box experiments, in our rewritten Introduction. We also state explicitly in the Conclusions that future experiments should approach the depth problem.

Page 14, Line 14: "...area under the Hilbert transformed curve..." – meaning that the value calculated is the sum of the gain-corrected backscatter power over the chosen time window?

For estimating the depth of the debris-ice interface, we used the progressive, integrated area as a function of time under the gain-corrected, Hilbert transformed trace. This progressive area was taken as a measure of the total backscattered power at that antenna location, from the depth equivalent of the time range, using a dielectric of 3 for debris. We realize that the inherent beamwidth of the antenna allowed many backscattered events, including that of the surface reflection and the DC, to interfere, but, on average, the progression of this integrated area represented a measure of volumetric backscatter. This has been clarified in the manuscript.

This would seem to depend on both the gain function used and on the time-window selected.

That is correct. We used a spherical correction to approximate the actual loss caused by beam spreading. And, as the window lengthens, the area of integration under the trace increases for each unit of time.

As the signal attenuates with depth, eventually it reaches the noise floor (i.e., system noise), so if a long time window was chosen for exactly the same profiles, this would add 'power' to the summed magnitude because the this approach would sum up this noise. This would increase the total summed power and so the 38

Also, I think this approach assumes a linear relationship between summed backscatter power and debris thickness, but the effect of attenuation would mean that this is incorrect.

We used a geometric spherical beam spreading gain correction. Consequently, the deeper the returns, the greater the integrated area. Thus, we might expect that energy of return increase with depth. But it does not because the returns from deeper in the debris, for the most part, have been scattered away from the antenna direction. A linear relationship is what one might get if we had a pencil beam, like an X-ray in a CT scan of uniform material. Given that attenuation caused by scattering is complex, the LOOCV method is reasonable in that it makes no assumption of any such relationship.

This is because the near-surface returns contribute much more to the sum than the deeper ones, so for example, doubling the debris thickness would not double the summed backscatter power, the increase would be much less than double. I think the sensitivity of the summed power to increasing thickness would follow a decay curve.

Yes, the near surface returns do make a stronger contribution, and, yes, doubling the thickness would not double the summed backscatter power. However, our statistical approach is relatively insensitive to this nonlinearity. We have clarified this idea in the text.

Clearly the deeper debris should contribute less backscatter because much of the transmitted signal has already been scattered back by the debris above, and so is no longer available.

In most cases, it is only a small part of the signal that is backscattered because the presence of coherent pulses implies that single scattering is present. Thus, most of the energy that encountered a single rock went around the rock and just a small fraction was backscattered. The in situ wavelength for a 960 MHz signal is 31 cm/1.73 = 18 cm, which is much bigger than most of the clasts in the rock box and, as explained in the text, probably most of the clasts we encountered in our profiles on Changri Nup glacier. In summary, the nature of backscatter from rough debris combined with the wavelength we used presented a complicated situation in which the threshold method was a valid statistical approach. All of the profiles show single scattering and suggest that the loss of energy with depth was likely caused by deeper and deeper backscatter that did not reach the antenna (not by backscatter that was attenuated because of losses higher up in the debris layer).

This also means that the variation in the threshold calculated locally (e.g., the 35

Admittedly, there is some nonlinearity. We have now put the variability in thickness with thresholds calculated locally into Table 3, showing how the change in threshold percent gives a change in depth. The changes in depth remain smaller than the uncertainty.

A further implication of attenuation is that there is certainly a limit to the debris thickness that can be quantified by GPR (page 18, line 1, "...we do not have a thickness limitation.") – above a certain thickness, there is no longer enough signal for backscatter to be detected, and therefore no sensitivity to debris. It is not obvious a priori what this limit is because it depends on the debris properties, but once this is exceeded than all that can be said is that the debris is thicker than this detectability limit.

We have changed the language about our thickness limitation to show that our approach does not have such a thickness limitation within the range of debris thicknesses likely to be encountered in a supraglacial setting.

Finally, the 38% threshold is empirical and so does depend on the local properties (of porosity, lithology, grain size distribution, wetness). This means that it may be useful for interpolating thicknesses within a single survey, but is unlikely to be universally applicable (as suggested later).

We remove the assertion that 38% can be universally applied in our updated manuscript. Rather, we state in the Conclusions that the 38% threshold that matches debris thicknesses on Changri Nup Glacier is not completely transferable to other glaciers but, nevertheless, may indicate debris thickness on layers with mineralogy and porosity similar to Changri Nup's. Future work could assess the transferability of the specific threshold on other glaciers.

Page 14, Line 24: If the debris layer produces 38% of the backscatter, where does the other 62% of backscatter come from? Seems that you've already ruled out significant backscatter from the surface and sub-debris ice.

Page 16, Line 13 and elsewhere: be careful with the use of the word 'coherence' – it has a particular meaning in radar processing, whereas I think you're using it to mean 'detectable and continuous' or similar.

We have been more careful use of this term in the revision.

Page 16, Line 29: need to add these porosity uncertainties into the debris-thickness error budget (yellow lines in Figure 8 etc).

We believe it most appropriate to represent uncertainty in Figures 6, 7, A1, A2 and in Table 3 consistently and with the single metric of average RMSE from the LOOCV. However, it was a good point that this reviewer made, and we enhanced the Uncertainty section to include a discussion of adjustments to the depth scale caused by porosity differences/assumptions. We recalculated the threshold with a porosity of 30% and showed that it did not change much. We added to our Conclusions a clarification that our presented threshold percentage is specific to the porosity and mineralogy of the debris we measured. The 38% threshold that matches debris thicknesses on Changri Nup Glacier is not totally transferable to other glaciers but, nevertheless, may indicate

debris thickness on layers with mineralogy and porosity similar to Changri Nup's. Future work could assess the transferability of the specific threshold on other glaciers.

Page 17, Line 11: by far the greatest influence on ice detectability in at least the McCarthy and Nicholson studies is the wet ice surface.

We acknowledge this in the revised manuscript. In our Discussion, we added that in the ablation season, [other authors] likely encountered a wet interface, a 'saturated layer' of water on ice, below mostly dry debris. A wet interface gives a much stronger reflection than a dry one.

Section 4.3: see above regarding volume scatter versus point scatter.

We agree on the definition of "volume scatter." The language in question that caused confusion over our use of the term has been rewritten or removed in the rewrite.

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