The authors would like to thank the three anonymous reviewers for their comments and suggestions. In the revised manuscript, we will address all of these comments. Detailed responses to how these comments will be addressed are included in this document. In reading the reviews, there were a few topics that we feel require fundamental (if minor) changes to the manuscript to increase understanding. We will address those here and refer to these comments later when responding to individual comments.

The terminology of a "layer" in the manuscript will be changed in the revised manuscript. We regularly referred to "surface layers" of LAPs in the manuscript, but it seems easy to confuse this with different "layers" in the snow. The difference being that "layers" in the snow are generally assumed to have macroscopic thicknesses on the order of centimeters while a "surface layer" of LAPs in the manuscript refers to a layer of non-ice particles, that are not mixed into a macroscopic snow layer. In the revised manuscript the LAP layer will be referred to as a "coating" or "surface coating" of LAPs to distinguish it from snow layers including layers of snow with LAPs mixed in. We feel that this change will make the discussion and interpretation more clear.

Below is a new table, a version of which will go in the revised manuscript. The Vallunaraju case is used to demonstrate the impact of different layer thicknesses. SNICAR was run with the same total eBC with the eBC being distributed evenly through different layer thicknesses (MMR increased as layer thickness decreased conserving total eBC). The visible albedo calculated by SNICAR (assuming a total snowpack depth of 10 meters) shows large differences depending on how one partitions the LAPs. Note that the modeled albedo using equation 1 in the manuscript is 0.38. Also, note that this was a much more extreme case than that shown in Figure 1, thus, although we don't have direct albedo measurements from the case, we suspect that the actual albedo was closer to 0.3 than 0.7. This table will be included in the manuscript as well as discussion of these results.

layer thickness (m)	LAP conc (ng/g)	LAP below	visible albedo
0.1	800	0	0.8914
0.01	8000	0	0.7013
0.001	80000	0	0.4189
0.0001	800000	0	0.3116
0.00001	8000000	0	0.2974
0.000001	80000000	0	0.2983

All three reviewers mention the use of the asymmetry parameter is erroneous and incorrectly defined. In the revised manuscript, the surface reflection will be redone more realistically. The 15% estimate in the original manuscript is likely too high based on initial calculations suggesting that this factor is less important.

Anonymous Referee #2

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The paper focuses on the impact of the vertical distribution of light absorbing particles (LAPs) on versus in snowpacks on surface albedo, and it specifically addresses how sampling must be designed to account for cases where LAP are on the surface of the snow, rather than mixed into the snow. A sampling strategy is recommended. A qualitative demonstration of the effect on albedo of having LAP on the snow surface versus mixed into the snow.

The paper raises a relevant and, in for some snowpacks, important point: Albedo calculations that use mass mixing ratios (MMRs) of LAPs in snow which assume the LAPs are uniformly mixed into the snowpack will be biased toward higher albedo, if in fact the LAP are concentrated at the snow surface. This topic is relevant for The Cryosphere, where many paper on the effects of LAPs on snow albedo have been published.

However, I have several significant difficulties with the paper:

1) The authors present their proposed sampling strategy as if it is a *new* strategy. In fact it's simply a refinement of the approach used in previous studies. While some studies have analyzed for LAP using a single or uniform sampling depth at all locations, in some cases sampling has specifically tried to isolate layers that appear to be uniformly mixed – including, sometimes, sampling very thin surface layers (e.g. ice crusts), and in some cases sampling both 'surface' and 'sub-surface sample' layers, even in some cases with the surface sample covering quite a thin top layer of the snowpack. What is suggested here is a sampling strategy is simply taking the same overall approach, but for cases where there is actually an accumulation of LAP on the snow surface of sufficient thickness limiting the surface sample to an even thinner layer (e.g the top couple mm of the snow surface).

While it is indeed not a "new" strategy and is a refinement of typical sampling practices, when a significant surface coating is present, the albedo calculations can be markedly different. Numerical experiments with the SNICAR model (done in response to a question from Reviewer #1) lead us to conclude that when a surface coating is present, treating the coating as being 50 microns thick will lead to albedo calculations that converge with the calculation technique described in this publication. While trying to collect only the top 50 microns of snow is impossible, radiatively, all of the mass in the top layer of sampled snow could be modeled to be in a 50 micron surface snow layer. The text of the revised manuscript will detail these SNICAR experiments and the data will be placed in a table.

2) This surface layer of LAP is alternately presented as being a "2D" layer and being sampled over some depth – so it is clearly not really only 2-dimensional. The text should be edited for better consistency and accuracy. While I understand the gist of what the authors are trying to say, the surface LAP always has some finite depth.

True, it is not an actual 2D layer. As stated above, it can be treated as a 50 micron layer. When considering scale, when dealing with ice crystals on the tens of microns scale at the smallest, LAPs can be much smaller, from tens of nanometers for black carbon to a few microns for dust, so a coating of these particles would be very thin compared to ice crystals in the snow pack. Also, surface LAP layers tend to be more common in melting conditions when the ice crystals are larger. The text of the revised manuscript will fully elucidate this.

3) I believe that the suggested method for calculating surface albedo includes an in- correct interpretation of the asymmetry parameter, g. A g of zero, for example, means that light is scattered equally in the forward and backward directions (the case for very small particles). A g of 0.80 does not mean that 20% of the light is scattered upwards.

As stated in the initial comments, the surface reflection will be redone and will likely come out lower thus reducing the impact of this factor.

4) What SZA is used in these calculations?

All input parameters will be detailed in the revised manuscript. Although, as detailed in response to Reviewer #1, the impact of the LAP surface coating is the focus of this publication and the impact is treated similarly regardless of underlying snow properties, so we feel that going into too much detail on the snow properties would reduce the impact of the message we want to get across.

5) It's not clear how the light-scattering properties of the particles on the snow surface are accounted for; as far as I can tell only absorption is accounted for. By process "B" in Figure 2, some amount of incoming light is "immediately reflected"– but in the text, the light lost before reaching the snowpack is all via absorption (lines 98-100). Reflection by the snow, not the particles, is then accounted for using an approximate asymmetry parameter (lines 104-106; correction needed per point 3 above). I don't see discussed anywhere how much scattering there would be off of the particles themselves. LAPs in ambient snow will rarely if ever be pure soot. In the case of glaciers or snowpacks near light-colored deserts, for example, particles accumulated on the snow surface could be much more strongly light scattering than soot. This would affect both the up- scattering/reflection of incoming sunlight and how much light propagating up through the snowpack (path "F" in Figure 2) is then scattered back down into the snowpack by the surface particle layer, where it will have additional opportunities to be absorbed by LAP mixed in the snowpack. As such, I believe the model given accounts only for surface layers of particles that are nearly purely light absorbing, and this is not valid for use with ambient snowpacks.

Thank you very much for this comment. We are indeed only considering absorption and not scattering by snow-borne impurities. As stated in response to reviewer #3, we are also focusing on "effective black carbon", and are thus focused on just the absorption component of "extinction". We will re-word the revised manuscript appropriately. On a side note, for studies using the LAHM measurement technique, we do have some filters collected in Nepal (by co-author UH) that have very high quantities of scattering particles that have extremely little absorption.

6) The suggested sampling strategy is to collect one sample that isolates as best as possible the surface layer of particles, while collecting as little of the surface snow as possible, then collecting a single sub-surface sample (depth not specified). However, vertical variations in the MMR of LAPs in the sub-surface snow may also be important. For example, a snowpack that has significant accumulation of particles on the surface might also have a thin melt- or wind-crust that has quite a different LAP MMR than the snow immediately below that. Or perhaps there was a snowfall of a few cm, then a long period of no new snow but significant dry deposition, then another cm of relatively clean new snow – then the accumulation of particles on that snow surface. While vertical variations in the MMR of LAP in the snow will be most important near the snow surface, all such vertical variations will matter down to the penetration depth of sunlight (approx. 10cm, +/- depending on LAP concentrations and snow grain size). The proposed sampling strategy is certainly a good *minimum* requirement for snow with very high concentration of LAPs on top of a snowpack. However, a single sub- surface sample as a recommended strategy is only appropriate where the snow below these particles has very uniform LAP MMR – a very specific configuration that will occur in only a very limited number of cases. If an improved sampling strategy is to be proposed, it would be better to account for a larger range of possible cases by specifying that, whenever possible to do so, multiple sub-surface samples should be collected. If visible layers are present in the snow, sample depths could be determined visually; where no visible vertical variations are present, if several sub-surface layers can be collected it would be best to sample thinner layers towards the snow surface (e.g. collecting samples of 2cm depth, 3cm deep, then 5cm deep for the case where three sub-surface layers could be collected, to cover the full 10cm that light is likely to penetrate).

This is very true and is what we address in the conclusions around line 270 (not to the extent that you do, but we will expand the text). In Colorado, where the lead author has done a lot of sampling, more often than not, there is a layer of fresh snow over the top of a "former" surface that has an LAP accumulation layer. Especially in a situation with 2-3 cm of fresh snow over a very dirty surface, it is important to characterize the snow at all layers. An important point that will be elucidated in the updated manuscript will be that layers need to be treated as layers (no matter where they are in the snowpack). If a 1 mm thick layer is really dirty no matter where it is in the snowpack, it needs to be treated as a 1 mm thick layer with an appropriate LAP mass in the layer and it should not be averaged over a depth substantially more than it occupies. There is no "one size fits all" sampling strategy. The strategies suggested in the manuscript are meant to be "one size fits most". The authors all work with citizen science groups, so a one size fits most strategy is most likely to get decent results with untrained data collectors. On line 270, the manuscript states that "much more complicated snowpacks" need more complicated treatments and our hope is that scientists who need more accuracy will do more detailed sampling and with the knowledge gained from the manuscript, collect snow with different layers being treated as layers with substantial depth or with minimal depths (more 2 dimensional). We will emphasize the importance of sampling layers appropriately in the updated manuscript.

7) A simple thought experiment is sufficient to know it must be the case that concentrating LAPs at the surface of the snowpack will have a greater effect on albedo than will mixing the same amount of LAPs throughout some depth of the snowpack. This does not need to be qualitatively demonstrated via experiment. What *is* needed is quantitative verification of the proposed theoretical method of calculating albedo in the case where LAPs are on the snow surface, using the sampling strategy and calculation method proposed. The experiment described in Section 3 (results shown in Figure 3) doesn't provide such a quantitative test. Thus, I question the value of including discussion of this experiment in the paper.

As stated earlier, we feel that the qualitative demonstration of the results (the experiments shown in figure 3) are important to show, but due to the constraints, it was not possible to conduct the experiments in a way that could be very accurately quantified. Thus, we would consider moving this section to being supplemental to the publication and not part of the publication.

The revised manuscript will show that the SNICAR values trend towards (but not exactly to) the equation calculated results as the surface layer in SNICAR trends thinner and thinner for the Vallunaraju case. Unfortunately, we don't have albedo measurements for that day (it was a citizen science expedition) but the albedo was definitely closer to 0.3 than 0.7 (the SNICAR calculations for a 10 micron layer and a 1 cm layer respectively).

8) If the experimental results are going to be included two things need to be addressed:

a) The setup had the FieldSpec located 30cm above the snowpack, viewing an area 20-30cm in diameter to which LAPs had been added. I am assuming the FieldSpec has something like a cosine-weighted input. (Such details should be specified). The instrument therefore would have been viewing an area that includes snow without added LAP, correct? This is not mentioned or accounted for.

b) The "clean snow" (snow to which LAP has not been added) is reported as having an eBC MMR of 12 ng/g. In the four cases shown in Figure 3, the ~500nm albedo of this snow varies from <0.8 to >0.95. An MMR of 12 ng BC/g snow isn't sufficient to explain a 500nm snow albedo of <0.8. In addition there's either actual variations in the snowpack or significant measurement uncertainties that are not being accounted for here, demonstrating that this is a very qualitative analysis.

Agreed, as discussed, qualitative, but we feel it is worth including given that it demonstrates the effect, although qualitatively only.