The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-162-RC2, 2019

Interactive comment on "The measurement and impact of light absorbing particles on snow surfaces" *by* Carl G. Schmitt et al.

Anonymous Referee #2

Received and published: 12 September 2019

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).

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.

3) I believe that the suggested method for calculating surface albedo includes an incorrect 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.

4) What SZA is used in these calculations?

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.

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).

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.

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.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-162, 2019.