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 #1

Review of « The measurement and impact of light absorbing particles on snow surfaces », by Carl G. Schmitt et al.

General comments

This paper investigates the impact of a thin layer of light absorbing particles (LAP) on the albedo of a snowpack, compared to an equivalent snow layer in whiche the LAP are well-mixed. It provides a theoretical framework to account for such a layer, and applies this framework to snow albedo computations for various snowpacks. An experiment is also set up in the field at a high altitude site in Colorado (USA), which qualitatively corroborates the theoretical findings. Finally, a sampling method is proposed to distinguish well mixed LAP from thin concentrated layers of similar LAP. This study overall demonstrates that the impact of LAP on albedo is much stronger when the latter are concentrated at the top of the snowpack than when they are homogeneously distributed within the snowpack. It means that the vertical resolution at which mass mixing ratios (MMR) measurements of LAP are performed can greatly impact the estimated albedo of a snowpack.

The topic of the study is relevant to *The Cryosphere*. The paper is well written and relatively easy to follow. However the novelty of the research is more questionable because it has been known for a long time than the impact of LAP strongly depends on their location within (or on top of) the snowpack. It has the merit, though, to propose a method for albedo computations in case a layer of LAP is located on top of the snowpack, and it sends a warning to people used to perform LAP measurements in snow, with some suggestion (clearly illustrated on a webpage) for a sampling protocole. The utility of such albedo computations is unfortunately poorly illustrated, which limits the paper impact. The physics behind the albedo computations is also approximative and some critical details regarding spectral measurements and snow physical properties make the study too approximative. Eventually the interpretation of the experiments is very limited. I believe major revisions could strenghthen the impact of the paper and make it singular among an already numerous literature on the topic.

Specific comments

1) The abstract is not really an abstract, it is more a condensed introduction. An abstract is meant to provide all the main quantitative results of the study. The abstract should be entirely rephrased to put forward the results and provide enough details, so that a reader would not need read the full paper to catch the essence of it.

The abstract will be re-worded in the revised manuscript

2) There seems to be a direct link between LAP vertical distribution and albedo estimations. However, if someone wants to know the albedo of a snowpack it's definitely easier and more accurate to measure it than to measure all the relevant vertical properties of the snowpack to feed a radiative transfer code. Hence it would be very helpful to understand in which context such albedo computations are needed. I think that it is most relevant to estimating the radiative forcing of LAP in snow, and to compute albedo in numerical models for weather or climate predictions (e.g. Tuzet et al., 2017). In general, the study references too few papers which highlights a lack of context.

The revised manuscript introduction will include more details on uses of this type of research. Specifically, dry deposition can be estimated by modeling of the airflow of plumes of smoke or other LAPs. In many cases, it is not possible to measure the albedo directly. Also, since LAP concentrations evolve with time during the melt season, correctly estimating surface snow melt (and deposition of the melted snow's LAP load) can improve the forecasting of albedo.

3) In the past, studies of the impact of LAP on snow albedo have mostly considered MMR, as pointed by the authors. It is not clear what the limit of this representation is, if the topmost layers in such representations become thinner and thinner. Said differently, how do albedo computations with a 1-cm-thick layer containing 8000 ng g^{-1} of eBC differ from those obtained with the introduced surface layer ?

How thin should be the topmost layer in the classic MMR representation to match the surface layer value ?

This is a very interesting question that has driven to a large extent the revision of the manuscript. SNICAR has been run with two layers, the top layer containing all of the "surface coating" LAP mass (in SNICAR, the MMR was increased with each decrease of layer thickness so that the total LAP mass remained in the top layer), and a 10 meter layer below with low LAP concentrations. With an initial top layer thickness of 10 cm, the calculated albedo was much higher than predicted by the equations in the submitted manuscript, but as the layer was made thinner, the snowpack albedo leveled off at a value similar to the value predicted by the submitted manuscript equations. The maximum layer thickness that resulted in similar results was 50 microns. A new table will be included in the revised manuscript to show these results and the SNICAR results calculated with different layer thicknesses.

4) The attempt to isolate the LAP surface layer from the snowpack underneath is interesting. However the physics behind this is not very rigorous. First of all, the so-called *surface reflectance*, estimated very simply from the asymmetry parameter of snow, is wrong. The asymmetry parameter *g* of a particle is wrongly defined. It is not the ratio of forward to total scattered radiation, but the mean cosine of the deviation angle between incident and scattered light. In particular, an asymmetry parameter of 0 means that as much light is scattered backward than forward. It does not mean that nothing is scattered forward as suggested by the authors. The paper by Bohren (1987) may provide useful insight to solve this issue. The quantity you defined is more likely to be (1-g)/2. More exactly you could find formulae for single scattering reflectance, e.g. in Khokanovsky (2002). Eventually, this quantity will depend on the solar zenith angle, which is not mentioned at all in the manuscript.

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.

Also, the computation of the total area covered by a given amount of LAP is very approximative. It seems that the LAP is first treated as a dilute medium to compute its MAC, but then a somehow arbitrary (at least not rigorously justified) scaling factor is applied to account for LAP overlaping. This point deserves more explanations, because the impact on the overall albedo is very large, and it is not properly accounted for in the uncertainty analysis. Reaching an albedo accuracy of 0.005 with such a loose definition is unrealistic. Also, it appears quite easy to obtain a total blocking of incident radiation with this definition, while the remaining snow in the LAP layer would still let some light travel through it.

The MAC is a physical property of the substance being measured in units of "area of effective absorption" per "unit of particles" where "unit of particles" can be mass or number or something else to define a quantity. With that, we can define a total absorptive area, but in some cases, the total absorptive area could be higher than the total area being investigated. This suggests that some of the area could be hidden, and the assumption is that the absorptive area of multiple particles could potentially overlap and since an absorptive area cannot absorb more light than is coincident, overlapping is likely. To define the area covered, a python program was written to estimate overlapping by randomly distributed particles. Simply, a 1000 by 1000 array was created to represent an "area" of snow, then "particles" were assigned to points at random within the array. Any point in the array that was occupied by one *or more* particles was considered to be occupied. If, randomly, no particles were assigned to a point, then the point was assumed to be clean. Using this strategy, numerous runs of the program were made adding more and more particles. A curve was fit to the results which is parameterized in equation 2. In the revised manuscript, this will be better explained and a plot will be added showing the parameterization. Regarding your comment that it is "quite easy to obtain total blocking", I am not sure I understand. A number divided by the same number plus a little (the square root of the number squared plus 1) is always going to be less than 1.

5) At no occasion the physical properties (primarily density and specific surface area SSA) of the snowpack are defined, while they are certainly required by SNICAR. In particular all the quantitative results of the study strongly depends on the SSA, which is not discussed at all. Also, the used configuration of SNICAR is not detailed (number of layers, snowpack thickness, underlying material, solar zenith angle etc.). It's also worth noting that SNICAR assumes spherical particles for snow, while the authors refer to hexagonal crystals to compute surface reflectance, which sounds inconsistent.

The impact of different physical properties of the snow is discussed briefly around line 175. While the physical properties of the snow are important to estimate albedo using the SNICAR model, substantially delving into this would detract from the message that it is important to consider a surface coating. The physical properties of the snow do not substantially affect the coating on the surface. In the revised manuscript, the physical properties used for all the calculations will be listed.

6) The spectral dimension of albedo is only very loosely discussed. The wealth of the spectral albedo measurements is unfortunately poorly explored because only broadband albedo values are given. The same study could be done at individual wavelengths before to work on broadband albedo. Because the impact of LAP strongly depends on the wavelengths, such an initial step would provide much more physical insight and could potentially be more convincingly supported by spectral albedo measurements. In particular the light penetration depth of radiation in the snowpack is never mentioned, while it provides a good estimate of where LAP might still impact snow albedo. The authors are definitely invited to discuss this spectral dimension in a future version, and they have room for it.

It would be very interesting to look at the spectral variability of this effect as it could have significant implications when considering LAPs with variable absorption such as dust and brown carbon. Unfortunately, due to likely time required to advance the study in this direction, we are unable to move in this direction at this time. We would be very happy to see an investigation on this topic, but cannot conduct it ourselves within the time constraints.

7) The *in situ* experiment is not sufficiently well described, and the results analysis is definitely too short. Figure 3 shows very distinct features for distinct experiments, that should be analyzed in more details, because they certainly contain unseful physical insight.

Due to conditions and time constraints, it was not possible to do the in situ experiments in a well quantifiable way. As pointed out by another reviewer, the albedo at 450 nm for experiment A was much lower than would be expected and much lower than the others. The reason for this could not be quantified as the results were calculated days after the experiments. There was always several minutes as well as changing of positions by the researchers between the plate measurement and the measurement of the LAP doped snow, which could have caused a number of factors affecting the results. The doped snow measurements were always taken in quick succession with little repositioning of the researchers. As such we did not feel that trying to exactly quantify the uncertainties would be reasonable but we do believe that the consistency of the observed trends were such that subjectively, they added evidence to the story. Given the difficulty in exactly quantifying the results, we would consider moving this section to supplementary material rather than it being a section in the publication should the editor or reviewers think that would be better.