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 #3

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The manuscript makes the point that differentiating layers of light absorbing particle (LAP) mass mixing ratios (MMR), particularly the surface layer, is necessary to derive a realistic value for the resulting surface albedo. A theoretical justification is provided which is underlined by a qualitative field experiment. Based on this, a sampling protocol is suggested and instructive online material is provided.

The manuscript raises the important point that bulk LAP MMR in snow samples can underestimate the MMR in the surface layer where LAPs can accumulate due to melting snow, sublimation or dry deposition of LAPs. The surface layer is obviously the most important layer for the resulting surface albedo. It is hence important to characterize the surface layer separately and harmonize the sampling protocol in the community to produce more comparable results. While this issue certainly deserves dissemination and is a topic for 'The Cryosphere', the manuscript does not present any new results per se, but is rather method focused. This makes it perhaps better suited for a 'Technical Note', rather than a full scientific manuscript, especially because the presented field experiment results are very qualitative. In addition, several points will have to be addressed to correct and complete the work, hence I suggest major revisions.

Our original intention was to submit it as a technical note, but over time the manuscript grew to the size of an article. As you mention (and as is discussed in replies to Reviewers 1 and 2, the field experiment is very qualitative and we would consider moving it to be supplementary materials. Though qualitative, they do demonstrate the impact consistently.

General comments:

1) The manuscript will greatly benefit from more context: Why is the accurate determination of snow surface albedo important? In which models and how are such measurements used? What is the use of a point and snapshot measurement as described in this work? A common challenge is the upscaling spatially and temporally with such field measurements. Spatial heterogeneity of LAP MMR in surface snow is one issue. The other aspect is, which has also not been discussed, that LAP do not only accumulate at the surface under certain conditions, but that also new snowfall can reduce the MMR of LAP at the surface temporarily. So the temporal dimension is also very important to consider as high and low LAP MMR conditions at the surface might alternate. To be able to model this a number of processes need to be characterized towards their importance for surface albedo, and it will be important to convey where in this more complex consideration the value of this work lies. There is a vast amount of literature and some of it should be reflected in the introduction.

The original purpose of the research was to develop a model for the evolution of snow albedo over time. It quickly became apparent that quasi-2D layers were extremely important. The LAP concentration on the surface of a snow pack in the future is a function of the current LAP concentration and the quantity of snow that melts and any dry deposition. This then drives the next time period's melting. So, in a sense, in addition to being a point measurement, this could describe input to a model.

The "clean snow on top of a dirty layer" scenario is discussed in the sampling strategies section in the conclusions (line 267). This is common in many places where members of the author team have sampled (Colorado and Peru). Suggested sampling is to collect the fresh snow to determine the MMR, then clean off the fresh snow and treat the surface as having an LAP coating, then sample the sub surface snow below that layer.

2) This work is entitled "The measurement and impact of light absorbing particles on snow surface", however it focuses on the 'measurements' rather than the 'impact' (see need for additional context above). In addition, the work focuses on black carbon (BC) rather than the diverse types or mix of LAP to which mineral dust, brown carbon, organic carbon, microorganisms and others belong as well. Measuring all of these will further complicate albedo determination and is beyond the scope of this manuscript. But the mix is an important feature of real snow samples and will vary in composition by location and season. Hence, either a discussion of how this could be addressed and how sensitive albedo calculations are to it

is needed in the manuscript, or at the very least pointing out the issue is needed, and the title should be changed to "The measurement of black carbon in snow surfaces".

This comment brings up a lot of interesting details and problems. If we are interested in the light absorption capability (and thus it's impact on albedo) of anything, it is necessary to define appropriate units. Unfortunately, historically, the light absorbing particle in snow community has settled on something completely unrelated to light absorption: a mass unit of black carbon. As you state in your comment #6, this is not a constant. We should be discussing the impact (absorption cross section of particles) as opposed to a mass of something that has variable absorption efficiencies. Note that the manuscript uses effective black carbon (eBC), not black carbon throughout and we define it as in Grenfell et al (2011): the mass of black carbon that absorbs the same amount of light as the present light absorbing particles. And specifically, we relate measurements to Fullerene soot type black carbon since Fullerene soot is well characterized by the scientific community and is commonly used for calibration. As pointed out by reviewer 2, we do not address the light scattering capabilities of the particles.

The revised manuscript will include a lot more information on the impact of different distributions of LAPs at or near the surface. With the caveat that we are not discussing the impact of light scattering by impurities in snow, we feel that "Light absorbing particles" is definitely more appropriate than "black carbon" in the title.

3) Some more precise definition of the "2D surface layer" is needed. Since it practically cannot be 2D more details on the depth in this particular study need to be included and it also should be discussed how the depth of the surface layer can vary by location. There might be examples where the LAP enrichment happened within millimeters, while for other locations a centimeter depth would describe the surface layer equally well. Also, there should be more information given on the depth to which light penetrates the snow and on which parameters this depends (snow characteristics, LAP MMR etc.), to provide a reasonable range of the overall sampling depth that should be covered in different layers. One assumption this work makes is that under the surface layer LAPs are mixed homogeneously, which most likely is a special case. In most instances, more layers will need to be defined. This is briefly mentioned at the end of the manuscript.

Our assumption is that dry deposition does not become mixed into a thick layer of snow. Obviously, it can potentially trickle down between crystals, but this isn't likely to take it too far. When snow melts, studies have shown that some impurities (20%) can wash out of a snowpack, but obviously (look at snow in the late spring) a lot of impurities stay at the top. As noted in the general replies, the revised manuscript will include a lot more information and calculations on the thickness of the surface layer.

4) The definition of the asymmetry parameter 'g' is not correct, please revise.

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.

5) The albedo calculation with SNICAR requires information on snow grain size, a critical parameter for the snow albedo (in each of the layers). Not only the spread and MMR of LAP at the surface are important, but also the changing snow grain size during the LAP accumulation process. There is no information in the manuscript about the measurements or assumptions made. Please add this. The same is true for the solar zenith angle.

This information will be added to the revised manuscript. As stated in response to other reviewers, the emphasis of this manuscript is understanding the impact of the surface coating or very thin opaque layers in a snowpack. The impact of the snow properties is a different topic so we don't go into it much as we don't want to take away from the message of the manuscript.

6) The optical properties of BC depend on its size. There is literature (e.g. Schwartz et al., 2013, DOI: 10.1038/srep01356) showing that BC size in snow is larger than atmospheric BC due to (post-)deposition processes. This shifts the MAC to smaller values. This process is particularly relevant for situations where BC accumulates at the snow surface due to melting or sublimation, or when the MMR is so high that particles stick to each other. This work does not take the size of BC into account and also does not discuss how the artificial soot doping of the snow in the section 3 experiments compares to atmospheric soot that is deposited. While it is evident that this information cannot be retrieved anymore and is also not trivial to derive, at least a discussion needs to be included because the effect partly compensate the estimated albedo reduction reported here.

Yes, we agree. This is why we included calculations using a MAC of 5 rather than the standard Fullerene soot MAC of 9 (the current table 1). As stated in response to one of your earlier comments, the manuscript uses eBC, effective black carbon, so the particles absorb light as efficiently as "X" amount of Fullerene soot (with an assumed MAC of 9). Note that LAHM measures eBC, not BC, so the measurements related to Figure 3 are not related to the actual mass. (Activated charcoal likely has a much larger size than atmospheric BC).

7) The paper would benefit strongly from a more comprehensive uncertainty analysis that includes for example variation of the following parameters: snow grain size, MAC value, estimate of the LAP covered area (because it is not clear based on which consideration the overlap is estimated in the manuscript, should be made clear), the surface layer thickness etc. This will be very instructive to learn where the largest uncertainties come from, and for the sampling community for where to pay attention for most accurate measurements. The would particularly benefit section 3, which is qualitative only, and hence does not add new information to the manuscript per se. The information section 3 provides is also provided in section 2, the theoretical consideration.

More uncertainty analysis will be added to the revised manuscript.

8) Figure 2 should be made more accurate. The variables mentioned in the text should appear, the transmitted and reflected radiation should be marked by arrows. Snow grains and layers are important features as well as an indication of the light penetrating depth.

Figure 2 will be remade as suggested.

9) The suggested sampling strategy is not new per se, it is rather a refinement of an existing method.

True, in the revised manuscript we will re-word this.