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The Cryosphere Discussions

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

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.

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.

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”.

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.

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

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.

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.

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.

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.

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

Specific comments.

l. 39f: Add to point 2) that particularly in areas with open soil or mineral dust wind blown addition of LAPs can be important.

l. 42: Are these situations really uncommon? Why not simply state that they occur?

l. 90: The authors do not use four different methods, they rather use four different model variants of SNICAR.

l. 101: from the text it is not apparent why the perfect spread of LAPs would result in 0.36 m2/m2. Please add an explanation.

l. 143: What does “it was found” mean. Is there a reference for it? If yes, it needs to be added. If the authors found it should state “we found”.

If the Schmitt et al., 2019, AMTD manuscript is available it should be added. If not, citing it does not help very much.

Figure 1: The caption states that it is the ablation zone that has not experienced any significant snowfall in the dry season. In that case, I would not expect snow on the ice of the ablation zone. And if there are remnants would they not have undergone several melting and freezing cycles? If that is the case what was the SNICAR input for snow grain size? Or is the dry season the cold season and the snow does not melt? This needs to be clarified.

Technical comments:

l. 38: replace “through” by “when”

l. 62: “. . ., an MMR is a reasonable. . .”

l. 86: “In the Svalbard case, . . .”

l. 89: “.. those scenarios to estimate the range. . .”

l. 91: delete “using” in front of “SNICAR-Online”

l. 182: reformulate to “. . . LAP concentration, we experimentally validated these results.”

l. 266: What is meant by “and if a surface layer is possible”? Please rephrase.

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