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.

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.

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 ?

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.

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.

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.

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.

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.

Technical corrections

l.15 : « deceptive » is subjective and should not be used here

l.15 : « surface accumulation » is not defined

l.17 : the link between sampling strategies and «estimates of albedo » is not explained. It is a lack of context

l.19 : be more quantitative than « top thin layer » which could be 1 mm or 1 cm

l.20 : this sentence is redundant with l.17

l.23 : do your measurements really confirm that the new sampling method enables better albedo estimates ? Be more quantitative anyways

l.33 : provide a reference for the 100 ng g^{-1} value

l.34 : collect → concentrate ?

l.34 : remove last part of this sentence and just keep the reference to the figure

l.37 : This paragraph could be put in first position in the introduction because it explains why LAP are expected in snow. Before to detail how it can be measured and what values are generally encountered. Please provide references to detailed descriptions of the three processes mentioned.

l.42 : remove the fact that it is « uncommon » because it suggests that your study is very marginal, while it is certainly not

l.44 : the « two-dimensional layer » is an awkward term. Valid for the whole manuscript. Use «thin » or « concentrated » layer ?

l.49 : would it be possible in such a model to define a top layer of 1 mm with 0.5 g g^{-1} of eBC ? Would it significantly differ from the layer formulation presented later on ?

l.52 : the end of this sentence is not clear at all. Is there a difference between energy absorbed in the so called top layer and in a 1 mm layer ? Please detail the impact on the snowpack energy budget if relevant

l.62 : this first paragraph looks more like an introduction

l.71 : result → value or estimate

l.74 : it is reflected because it interacts with snow, so this is poorly formulated

l.76 : it can also be absorbed by the material under the snow if the snowpack is thin

Figure 2 : It should be improved to support the corresponding paragraph which is currently hard to follow. I recommend showing a cross section of the snowpack, with a well identified surface layer. That layer should display some « holes » to allow sunlight to directly reach snow and be reflected without interacting with the LAP. The terms R_s, σ and α_s should appear in this figure.

l.92 : why such an assumption ? Is it relevant or does it cover the literature range on this MAC ?

l.95 : what if the sample had been collected on 3 cm ? The surface layer would have been three times more concentrated in eBC, and the fraction of blocking would have been unity, meaning an albedo of 0. This highlights the limitation of this approach to estimate the properties of the surface layer. It seems very easy to obtain a fully bocking layer, while the small amount of snow, even in a few mm layer, would allow photons to travel and finally be reflected by this topmost layer. As a conclusion, a layer still has some thickness which allows photons to travel between LAPs.

l.98 : does the MAC depend on wavelength ? If so, please precise it, and how do you handle the fact that MAC might vary across the solar spectrum on which broadband albedo is computed ?

l.100 : how do you get this 34 %?

l.115 : Note that SNICAR certainly provides an albedo of 0 for a very high load of BC, which seems contradictory with your assumption that some of the radiation is reflected back (independently of the BC load). Could you clarify this ?

l.115-120 : very hard to follow. Should be rephrased or numbers should be displayed in Fig. 2

l.124 : how thick is the polluted layer in SNICAR simulation ?

l.132 : this equation should come much earlier, after paragraph l. 71-80, and the terms defined at this early stage

l.133 : the R_s term is very questionable, because it is valid only if directly reflected radiation by snow is not absorbed by surrounding LAPS, which is unlikely if the snow is surrounded by LAP.

l.135 : R_s corresponds to clean snow, while practically snow is not necessarily clean. Isn't it inconsistent ?

l.139 : this definition is wrong as explained previously

l.139 : grain size was not defined nor quantified up to now…

l.141-146 : where does it come from ? Should be rigorously derived

l.143 : « it was found » → provide details or reference or plots

l.150 : need more details about spectral range, resolution, instrument etc. Why do you mention albedo measurements here while they do not appear in Table 1 ?

l.151 : « reduced » compared to what ? Spectral albedo or any other albedo ? Could you define HDRF so that the reader can interpret the value of 0.16. Are you sure that the quantity measured (HDRF) can be compared to the albdo defined in Eq. 1 ? Which seems to be some diffuse directional-hemispherical albedo ? This should by the way be detailed.

l.170 : it is not clear what the authors aim to do. An uncertainty analysis should be perfomed forward, starting from the various contributions, and ending up with an uncertainty on computed albedo. Here this is not an uncertainty analysis that is performed. Such analysis would be very complicated. Just say that you investigfate how much of BC in a surface layer does result in a change of albedo of 0.005. Note that 0.005 is certainly far more accurate than your albedo measurements.

l.174 : again the surface reflectance is wrongly defined

l.178 : what about these properties ? What values are taken, what is the range of possible values and the impact on the overall albedo ?

l.179 : you do not prove that the impact of well-mixed impurities is less than neglecting the surface layer, you just state it.

l.182 : « we were compelled » → too subjective wording

l.183 : what instrument for spectral albedo measurements ?

l.186 : instrument details arrive too late and are not complete. What light collector is used ? What field of view, spectral range etc.

l.189 : what is a calibration spectrum ?

l.195 : how can this estimate of 3 cm be reliable ? It seems extremely hard to assess in the field

l.200 : how do the 4 experiments differ ? Is the initial snowpack similar ? Were they all taken the same day ?

l.214 : Could you discuss more the change in albedo reduction because sometimes mixing LAPs does not change by two the albedo reduction. In general, please describe in more details the differences obtained among these 4 experiments.

Figure 3 : why is the albedo of clean snow at 450 nm so low for experiment A ?

l.216 : why don’t you use a solar spectrum instead of a Planck curve ?

l.216 : you apparently compare a visible to a broadband albedo, this is inconsistent

l.220-221 : remove this last sentence which is redundant with previous text

l.232 : scraping surface snow does not sound like a robust and reproductible protocol. It’s very qualitative. Is it really critical if you take too much surface snow, given you can correct for the contribution of subsurface snow later on ?

l.246 : showing the equations for these calculations would be helpful

l.254 : in general this conclusion is not well written and does not summarize the main results. Also it provides too few perspectives.

l.260 : it is still not clear why albedo should be estimated from such measurements (which should again include snow physical properties) rather than measured

l.261 : using « Recommendations » here is awkward. You should provide some major results before to present the sampling method

l.268: I doubt brushing fresh snow without getting underneath LAPs is an easy task.

l.270 : « where the lead author lives » is not very useful for the readers

References

Bohren, C. F. (1987). Multiple scattering of light and some of its observable consequences. *American Journal of Physics*, *55*(6), 524-533.

Kokhanovsky, A. A. (2002). Analytical solutions of multiple light scattering problems: a review. *Measurement Science and Technology*, *13*(3), 233.

Tuzet, F., Dumont, M., Lafaysse, M., Picard, G., Arnaud, L., Voisin, D., ... & Morin, S. (2017). A multilayer physically based snowpack model simulating direct and indirect radiative impacts of light-absorbing impurities in snow. *The Cryosphere*, *11*(6), 2633-2653.