The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-113-AC2, 2019

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "Metamorphism of Arctic marine snow during the melt season. Impact on albedo" *by* Gauthier Verin et al.

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Received and published: 17 October 2019

Comment from author's Referee

Introduction should be compacted, leaving out all those details and concepts that deviate from the main scope of the paper.

From p.2 line 19 to p.3 line 17: what is the point here? Please move part of the text into the discussion section, to compare the results of this paper with previous ones, and leave in the introduction only the key message (few lines only), avoiding all the details and concepts that deviate from the focus of the paper. The introduction should explain the relevance of the treated issue and the problems that previous works have left unsolved and this paper will help to solve. Is the message here that the snow

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processed studied in this work have been studied already a lot? What is then the added value of this work?

Author's response

We agree that too many details concerning snow metamorphism are given in the introduction.

Changes in manuscript

Details in the introduction will be reduced to a minimum. However, some information given are essential to propose a scenario of the snowpack formation for both years. Thus, snow metamorphism processes will be detailed when necessary in the discussion section.

Comment from author's Referee

The snow layer identification seems sometimes arbitrary. Due to the large spatial heterogeneity of the snowpack, it is hard to obtain the stratigraphic evolution of the snowpits, except in a broad line. The number of speculations on the evolution of the stratigraphy should be reduced to a minimum.

p.8, line 18-19: "Furthermore, layer II could be divided into two distinct layers of indurated faceted crystals which showed highest densities values, up to 500kgm$-$3, topped by a wind slab." Why the uppermost dune layer is called IIb instead of III? Isn't it generated by the same process at the same time?

Generally, it is very difficult to see correspondence between the layers described in fig 5 and the SSA and density profiles shown in Fig 6. The distinction between layer I and II is quite obscure in Fig 6 (do indurated depth hoar (layer I) and indurated faceted crystals (layer II) have same SSA and density?). Also the distinction between layer II and IV is hard to see in Fig 6. It seems to me that the schematic picture of the stratigraphy in fig 5 can be applies to few selected cases, but then most of the profiles are much more complex, also in view of the spatial heterogeneity. I therefore recommend

referring to Fig 5 only as an example of the observed stratigraphy, valid for a subgroup of snowpits, and mark in fig 6 what are the snowpits with stratigraphy that follows fig 5. Alternatively, I recommend to mark the 4 layers (e.g. with horizontal black lines) in each snowpit shown in fig 6. Actually, I recommend marking the layers also in case that stratigraphy is shown only for a selection of snowpits.

Author's response

The snow layer identification was based first on stratigraphic observations (snow grain shape) and then potentially on the SSA/density vertical profiles. The stratifications presented in Figure 5 summarized all the information sampled at the snowpits. Despite the spatial heterogeneity (snow depth and snow layer thickness) and the time evolution (snowfalls and melt), we believe that our summarized stratifications are relevant as we always observed the same snow layers in each snowpit or at least the bottommost layers. Concerning the five dunes, we decided to introduce layers IIa and IIb because, both are similar to layer II of conventional snowpits and present enough distinctions to justify a subdivision. This distinction was not observable for conventional snowpits maybe because this layer was much thinner than for dunes. It is true that the chosen stratigraphic layers presented in figure 5 do not match with the vertical profiles of SSA and density in Figure 6. It is because some stratigraphic layers, very different in term of grain shape, show very close values of SSA or density (for example layers I and II).

Changes in manuscript

We understand referee's comments, however we think that our stratifications should remain unchanged. The corresponding explanations in the text will be improved to avoid any misunderstanding from the reader. In addition, in Figure 6 dunes profiles will be labeled for a better recognition, and we will find a way to mark each layer (according to Figure 5) for every vertical profiles.

Comment from author's Referee

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It is not always clear to which campaign (year) the results and discussion refer to. This should be clarified, and possibly the differences in snowpack characteristics between the two years should be discussed more extensively (personally I find the large observed differences very interesting, especially from modelling perspective).

Author's response

The large differences between the two years are indeed very interesting. Unfortunately, in 2016 much fewer snowpits were studied and the sampling began while snow had already experienced wet metamorphism at almost every depth.

Changes in manuscript

Years will be specified systematically and differences between 2015 and 2016 snowpacks will be briefly presented in section 4.1.

Comment from author's Referee

p. 7, line20: "several snowfalls". During the observing period in 2015, only two snowfall events were marked in Fig 4. In addition to these, where there other light snowfall events? If so, please mark all of them (see also my comments later)

Author's response

Changes in manuscript

Missing minor snowfalls will be specified in Figure 4.

Comment from author's Referee

Can the authors provide more descriptions of the melt ponds? How deep they were, where they open or frozen, how large they were, and were they covering the totality (or more than 90%) of field of view of the downward looking head of the spectro-radiometer (having, thus, a radius of more than 3m)? How many melt ponds have been measured? Had they varying characteristics? Are the provided values for bare ice and melt pond

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albedo some averages? What was then the standard deviation? Also, in the discussion section it would be good to compare with the bare ice and pond albedo measurements carried out in previous studies.

One final question: is it so that melt ponds did not form in 2015? Or simply the campaign stopped before the ponds formed? In my opinion, the differences in snowpack characteristics between the two years is very interesting, and very enlightening for modellers. Could the author include a discussion on slush layer and melt ponds (how they formed in 2016, and possibly why they did not form in 2015) in section 4.1?

Author's response

Phase IV has been introduced to mark the snowpack vanishing and the profound changes in sea ice albedo. Properties of bar ice and melt ponds are not studied in detail because we focused on snow properties. In Figure 7, the provided albedo spectra are averages spectra of both surfaces (measurements were made over ice or ponds only). In 2015, ponds formed only few days before we left the ice camp. The pond formation was similar to 2016, ponds were first very extended before shrinking and settling.

Changes in manuscript

We think that we should not discuss of slush layers and ponds formation as many process behind remain unclear. We also lack of data that clearly link the snow cover, the ice properties and the slush layers or ponds locations.

Comment from author's Referee

p.10, lines 18-19: "Simulations with SSA reduced by 20% (see Figure 8 and Table 3), larger than the expected uncertainty, is not sufficient to offset the bias which is lowered to 1.0% at 500 nm." Based on the personal experience of some colleagues, who found strong overestimation of SSA using a similar measurement principle (IceCube), I think that in melting conditions the error in SSA measurements done with the applied sam-

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pling technique is closer to 100% than to 20%. The fact that the error increases with increasing wetness of the snow, and that it is most severe for the 1000nm than for the 500nm albedo, suggests that there is a problem in the SSA estimation. I recommend replacing the albedo modelled using +-20% SSA with the albedo modelled using + 100% SSA.

Author's response

Simulations with SSA reduced by 20% were made in order to investigate a potential bias in our SSA measurements. The results show that such a bias (far above the DUFISSS uncertainty) cannot explain the discrepancies between measurements and simulations, particular during phase I when snow was completely dry. It is true that melting conditions affected the SSA measurements during phase II and caused the largest discrepancies between measurements and simulations as discussed in section 4.3. However, those conditions did not prevail during this phase. For these reasons, the observed constant bias could not have been induced by erroneous SSA measurements.

Changes in manuscript

The argumentation showing that the constant discrepancies between measurements and simulations are not induced by a constant bias on SSA measurements should be based on observations during phase I. Observations during phase II were potentially affected by melting and then should be dismissed in this case.

Comment from author's Referee

p.11, line10-11: "Albedo spectra of bar ice and melt pond (as shown in Figure 7d) were used as soil albedo in TARTES." There cannot be snow above an open melt pond. Did the measured albedo spectra correspond to frozen melt ponds or to open melt ponds? They have quite different albedo. Did the authors measured the albedo of slush? Is it equal to the applied albedo of melt pond? These model results may be used to

interpret the evolution of albedo during phase III -IV in 2016, but not in 2015 (which is the year illustrated in Fig 6 and 8), where neither slush nor melt ponds were observed.

Author's response

The measured albedo correspond to open melt ponds. The aim of the study is to investigate the effects of the widest range of "soil albedo" beneath the snowpack on surface albedo. This is why we used the albedo of both bar ice and open melt pond. In 2015, a slush layer was observed in some cases, but as the snowpack was deep enough, it did not affect significantly the surface albedo ant then was not taken into account for simulation.

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The text will be clarified as described above.

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