Review of “Comparison of optical -equivalent snow grain size estimates under Arctic low Sun conditions during PAMARCMiP 2018”, by Jäkel et al.

The manuscript tries to address the issue of snow grain size retrievals in the Arctic under low sun conditions. The topic is important and timely, considering the influence of snow on albedo and the heat budget in the Arctic, and the scarcity of data in the region.

There are several shortcomings in the manuscript and I recommend a major revision. I will summarize the major points below and then give some detailed comments.

1) I found the manuscript hard to follow and I had to reread most sections several times. The main issue is that this study uses several remote sensing products together with various radiative transfer models to estimate albedo and to infer optically relevant snow grain size. On top of that this study also uses models to estimate grain growth (or changes in the SSA/optically relevant snow grain size) as well as models that simulate changes in albedo. Then there is also the ECMWF and the ICON snow model. This is all extremely confusing and it is not clear to me how a grain growth estimate or albedo evolution “model” fits into this study. One way to improve the manuscript would be to restructure the paper and to separately summarize what is observed, what is modelled using the observations (grain size and albedo) and what is modelled using different inputs (which is the albedo evolution and SSA evolution). And then summarize how the different observations/models are combined and address what they can or cannot deliver. An overview figure or even table might help here.

2) Another problem with the manuscript is the different resolutions of the various retrieval methods. The resolution of the retrieval from a satellite borne measurements would be different from that of an airborne measurement, in that the spatial variability within each retrieved pixel is different, which will have an effect on the results. This should be discussed.

3) The manuscript refers to IceCube measurements as a ground-based reference, but fails to acknowledge that IceCube is not a direct measurement of the SSA, but has similar issues are other remote sensing methods. It should discuss and include the recent work by Calonne at al., which discusses the uncertainties of IceCube and compares the results to direct measurements of SSA from micro-computed-topography. The uncertainties are certainly larger than the here reported 10%.

4) Suppressing the vertical temperature gradient in snow over sea ice is not really an option in March. Considering that the air temperatures are around -30C and the sea ice around -2C, the temperature gradients should not be assumed non-existent. The result may fit better with the retrievals, but it would be for the wrong reasons. This should be addressed.

Detailed comments:

P1, L4: what’s “partly rough”? it should be defined.

P1, L15ff: it should be mentioned here that the large spread of MODIS retrievals may have something to do with the time of day and the shadows that are present at different times of day.

P2, L28: The variable curvature doesn't necessarily refer to convex or concave shapes, but can be applied to the same "shape" with varying grain size (and therefore curvature). The effect of "equilibrium metamorphism" is also quite small. The so-called equilibrium metamorphism is caused by grain-boundary diffusion and not by vapour diffusion, as demonstrated for example by Kämpfer and Schneebeli 2009 (Observation of isothermal metamorphism of new snow and interpretation as a sintering process).

Furthermore, rounded grains and grain growth are not necessary linked to equilibrium metamorphism, but have been observed under sinusoidal temperature gradients (see Pinzer and Schneebeli 2009, Snow metamorphism under alternating temperature gradients: morphology and recrystallization in surface snow).

P3, L4-5: It's a bit confusing to call the "optical-equivalent grain size radius" grain size. It is not clear later in the manuscript to which grain size the manuscript refers. Maybe for consistency and clarity, you could just refer to SSA.

P3, L20: While I agree that the macroscopic surface roughness is relevant, I don't think that the roughness in this manuscript is the macroscopic roughness. Figure 1 shows really high values of surface roughness that is quite large. This is not surprising, considering the horizontal resolution is 1 m (p5, L2). The colour bar on Figure one make it impossible to distinguish between roughnesses that are orders or magnitude in difference. Maybe consider using a logarithmic scale as well as a discrete instead of a continuous colorbar?

P3, L20: What about grain orientation and the asymmetry factor?

P4, L10-20: This should be rewritten, because it is not clear. The order of these sections does not make much sense, the way this is written.

P4, L25: Why is a 2012 paper cited for a 2018 campaign?

P5, L12ff: see also one of the major comment about the accuracy of IceCube. This should be discussed here.

P6, L18: There is a typo here. The MODIS wavelengths are in micrometer not meter. Also the lower value is 0.405 and not 0.4 microns (for consitentcy, because the higher value is given in three digits after the comma).

P7, L4: How is this surface roughness defined? Is it the RMS height or the correlation length? Or some combination of both? This should be clarified. Also see comment for p3, L20 on how to improve the figure.

P7, L16: remove "rather'.

P8, L25: So the asymmetry factor and the absorption parameter were constant for all calculations in the radiative transfer model? This is not clear from the text.

P9, L14-15: What are the default parameters?

P9, L30: I can not access the Essery 2001 reference. Can you reference something with a doi? Also ageing for albedo may work for warmer conditions, but it may not be representative for estimating albedo changes in -30C conditions. But as said, I don't have access to the reference.

P10, L23: Sensitivity study instead of sensitive study.

P12, L9: I thought TARTES was used to calculate snow surface albedo from SSA (and other snow properties). So you also use it to estimate the grain size? Can you please explain? Do you use it to calculate albedo from IceCube measurements, and then to calculate grain size from albedo retrievals?

P14, Figure 3: It would be interesting to see the signal to noise ratio at 1700 nm. Did you find any issues with this?

P15, L7: How was snow density measured? This was not mentioned before.

P15, L14: Considering the large uncertainty of IceCube, I think 5 microns in difference is extremely small. Maybe the uncertainty should be added here.

P16, L3: Are the "ground-based measurement" the IceCube measurements? Are the snow layers that are discussed here used anywhere else in this study?

P16, L16: Do "both data sets" refer to line A and line B?

P16, L22: The Essery 2001 reference that is not available is listen previously for the albedo "ageing" as well as here for the grain growth. Considering that it is important for this publication, it really should be accessible.

P17, L10-15: As mentioned above, assuming no vertical temperature gradient is not an option over sea ice in March. It really does not matter whether this model fits the observations, if it's for the wrong reasons. This needs to be fixed.

P18, L16: The solar Zenith angle should be adjusted for clouds. It certainly is not 83 degrees under cloudy conditions. It would be significantly lower. It needs fixing.

P21, Figure 7: I really like this figure. It should be given a bit more context in the text.

P21, L10-11: The agreement should be considered together with uncertainties of IceCube.

Figure 8: The differences certainly have something to do with the errors due to shadows. I am not sure how useful this analysis is if the shadows are not removed.

Figure 9: What's the resolution of figure 9? And why do you need this analysis if you have the TLS data? It's not entirely clear?

P23, L12: Unfinished sentence.

P25, L1-5: The small variations in in snow coverage do need to be excluded, otherwise I am not sure if the results from this study are valid. Same for the shadows (P29, L1-5), they should be excluded when estimating the albedo.

P28, L7: As mentioned above, it is very unlikely that the temperature gradient is zero. And even if it were, the curvature effect would not be driving the metamorphism.

P29, L2: After such a large study, the strengths and weaknesses of different methods should be summarized.