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Interactive comment on "Effect of small-scale snow surface roughness on snow albedo and reflectance" *by* Terhikki Manninen et al.

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Considering recent work done on albedo over snow surface roughness by Larue et al. 2020 (https://tc.copernicus.org/articles/14/1651/2020/tc-14-1651-2020.html), I have read the paper and listed some comments, with the hope to help improving this interesting paper.

Detailed comments:

L34. It is not clear the diameter of what it is.

L35. "the more appropriate" → "a more appropriate"

L46. Maybe consider Larue et al. 2020 here.

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Discussion paper

L91. “were located”. Do you mean sampled ?

L107. I recommend to use S.I units and scientific notation. kg m-3

L140. I do not understand “Brownian surface”, do you have a reference ?

L140. Can this hypothesis be tested ?

L153. I’m not sure to understand this sentence. I understand that the accuracy of the dataset is by far inadequate for the purpose of determining surface roughness. Is this correct ?

L160. I suggest to clarify the processing done, it is not reproductible as written here. Present the equations ?

L165. Please indicate accuracy of the instrument and typical range in the text.

L187. The equations used for the correction not presented, as well as some values for the order of magnitude of this correction. This would help to understand and reproduce what is done here.

L195. There are different ways to measure albedo, your method should be more precisely described (add a scheme or picture?). From what I guess, the instrument is looking downward and you take two successive measurements, one of the snow and one if the spectralon lying on the ground ? If this is correct, then the mearsurements of the clear-sky day must be denoted “bidirectional reflectance” or “biconical reflectance”, not albedo. This is critical for a precise comparison. In addition, I’d like to know 1) which size and which reflectance has the spectralon 2) if a tripod was used and shadow correction was done, and 3) how the spectralon leveling was guaranteed ? This latter point is very important for the accuracy, even an invisible tilt has a huge impact during clear-sky days.

L206. Same question for the spectralon as in the previous comment. Especially if the surface is rough, it seems difficult to level the spectralon without special equipment.

L207 “real broadband albedos” I don’t understand, the instrument is measuring BRF, not albedos ?

L 293. “per profile”. I guess that the model is 2D ? The results would be very different in a 3D configuration, in particular the probability of escape from the surface. Please add some precision about the configuration and the underlying approximation if it is 2D.

L296. This approximation might be good for ice or water but snow is close to a lambertian surface. This is expected to greatly affect the results.

L312. Could you define “rms slope” ? Maybe for sake of clarity write “rms slope angle”. However, as a general matter, I would recommend not to work with rms slope angle at all, because in theoretical studies (e.g. microwave or optical scattering by rough surface), the variable that naturally appears in the equations is the rms gradient (in m/m not in °), sometimes call quadratic mean slope or even rms slope in the radar altimetry community.

L320. Is it relevant to propose empirical relationships when the literature provides analytical solutions of the albedo of Gaussian rough surfaces (e.g. doi:10.1029/2012JD018181) ? The latter has also the merit to assume a lambertian surface which better applies to snow than the specular/mirror approximation.

L359. This sentence seems odd. If the surface is Gaussian, the ratio sigma/L is directly related to the mean quadratic gradient (equal with a factor or sqrt(2)). The gradient being the tangent of the slope, one should expect a very good relationship between the ratio sigma/L and the RMS of the slope angle. Why this is not the case here ?

L 366-370. “This result supports the view” is wrong. The correlation here can be explain by the fact that over the season grain size is changing in parallel with roughness, and that grain size has a large and well known impact on albedo. The statistical correlation here is inadequate to conclude on a causal relationship.

L375. I don't understand the very wide range of variation of measured albedo shown in Fig 9 given that a normalisation is done ? Can you explain and provide more information on the normalisation ?

L390. For clarity here add "using the TARTES model assuming a flat surface"

L393-395. How the (huge) uncertainty on grain size measurements is taken into account here to draw this conclusion ? It seems in addition that the selection of the grain shape based on the quality of the simulation/observation in Figure 9, interferes with this conclusion. By choosing another grain shape, a different conclusion could have been easily drawn, isn't it ? L411. Do you reach the same conclusion with different grain shapes ? How sensitive is this to grain size uncertainty ? More specifically, I'd like to see the result of March with spheres

L437. I don't understand what is meant by "bulk volume scattering". In the microwave range, surface and volume scattering are clearly two distinct mechanisms that appears in the equations. In the case of a diffuse medium like snow in the optical domain, surface scattering (in the microwaves meaning) is absent, only the volume does scatter the light (except if an ice lens is present at the surface). However, the layer truly contributing to the reflection is so thin (<1 cm) that calling the mechanisms "surface scattering" is acceptable (albeit with a different meaning from the microwaves).

The fact that surface roughness affects the albedo, is unrelated to surface vs volume scattering. The shape of the surface (i.e. surface roughness) is important even for volume scattering. I suggest to define the concepts used in the paper, as not all the readers understand the same way these words.

L443. This should be indicated in the method section, see my comment above.

L447. Not taking into account the atmosphere (white sky) appears to be a strong assumption for the roughness. It is possible to argue that in the blue range (where atmosphere has an effect) albedo is close to 1, no roughness effect is expected (your

equation or see Larue et al. 2020).

L450-455. It seems that the text is comparing Figure 16 (the modeling result) with Figure 14 (the measurements), but this is not clear. It is also not clear why the x-axis and y-axis are different between the figures. Why the probability of reflection predicted by the model could not be converted into BRF ? This is not explain in the method section. Ultimately if the goal is to provide a comparison, you should merge both figures.

L475. The sentence needs to be reformulated with referring to “p”, which is only defined in the Appendix.

L486 – 504. I don’t understand this part at all. What do you mean by “it may be that a surface layer containing very deep pits would benefit from some special attention.” or “the deep pit structure of the surface,” ? What is “pits” here ? What size / depth are you referring to ?

Section Discussion. I suggest to discuss your results w/r to results in the literature (e.g. Warren et al.1998, L’Hermitte et al., 2014 and Larue et al. 2020). The latter study has similar goal and scale to yours.

L585. The total amounts of absorbed and scattered signal is obtained when n tend to infinity ? “n” should be replaced by infinity.

L591. “n” was not defined (more precisely it was defined L261 in a different way), so it is difficult to understand n=0. Ps=0 is clear and it implies than any terms for n > 1 has zero contribution. But n=0 is not clear (see next comment).

L602. “the average value for n” contradicts the definition given L216 where “n” is the average of the number of event. I suspect that a different symbol should be used between the number of event before escaping and the average of this number.

(A25) uses “n” which is in the number of terms in (A17 and A18) for a given photon. It can not be “changed” to the average number of “n” for all the photons. This is equiva-

lent to switch sum and product... and results in the strange result (A27) which I don't understand (intuitively). I don't know if/where is the error but using "n" in two different ways seems risky.

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