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Interactive comment on "Effects of decimetre-scale surface roughness on L-band Brightness Temperature of Sea Ice" *by* Maciej Miernecki et al.

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This study offers a novel and interesting look at the effect of surface roughness on measured sea ice brightness temperatures at L-band – an important and relatively understudied topic. The method of applying Geometrical Optics to model the sea ice emissivity from observed PDFs of the ice surface height distribution seems reasonable. Results from theoretical simulations are a valuable contribution to the sea ice remote sensing community.

However, it remains unclear from the study what the relative importance of decimetre-scale roughness is to variability in sea ice brightness temperatures, in comparison

to say ice thickness, thermodynamic state, snow depth, snow properties and open water within the radiometer footprint. I have provided a set of general comments on the methodology and recommendations for improving the analysis or taking it further. I've also made some minor suggestions to improve the readability of the paper and clarify a few confusing statements. I'd recommend this manuscript is reconsidered for publication in The Cryosphere following these revisions.

Please do get in contact if you have questions regarding these comments. Kind regards, Jack Landy

General comments:

1. It is not obvious from the paper what are the implications of your results for sea ice thickness measurements from satellite, e.g. SMOS or the upcoming CIMR mission. What is the relative importance of decimetre roughness compared to other factors? Do the current incidence angles employed by SMOS limit the sensitivity of measured Tb to roughness? I expected to see a statement on this in the abstract and some discussion later on the manuscript.

2. The airborne altimeter data provide measurements of the snow surface roughness, but this is not necessarily reflected directly in the underlying ice-snow interface roughness. There is no discussion of this in the manuscript and the potential issues/errors it could introduce. Which is most important for L-band emissivity, snow or ice surface roughness? Might the roughness be overestimated if it's the ice interface roughness that you need to know?

3. It is not clear whether the assumption of isotropically-oriented surface roughness features is valid, even when averaging model-data comparisons over 5 km. Is the sensitivity of modelled Tb to surface feature orientation linear? When modelling Tb over 5 km, the assumption is that Tb will be the average of a uniform distribution of surface feature azimuth angles. But is it reasonable to assume the average of short radiometer Tb integrations, from sea ice with lots of different surface feature orientations, is mea-

suring the same thing? Is there any geometrical shadowing of facets at the 45-degree incidence angle? If so, how do you account for this in the simulations?

4. Could you not just use the observed empirical CDF within each 70 m footprint, rather than the statistical model fit, to simulate Tb? i.e. integrate over the N pairs of angles for each facet within the 70 m footprint. Is this just to speed up your simulations (70ˆ2/0.5ˆ2 is only about factor 2 larger number of facets than your 10ˆ4 criteria), or so you can calculate average model results over 5 km sections? Using the observed CDF may produce a better model fit to the radiometer observations.

5. A particularly useful contribution of this paper would be a more in-depth model sensitivity analysis of the relative effect of roughness on measured Tb compared to other factors. The current results touch on this with e.g. Fig 6, but by keeping other factors constant in the simulations its impossible for the reader to understand the true sensitivity to roughness. For example, how different does Fig 6 look for a different set of sea ice constants? E.g. 3 m thick, fresh MYI, with thicker snow depth and a warmer surface? I would recommend removing Figs 7 and 8, which don't really contribute to the message of the paper, and adding some deeper theoretical analysis of the relative impact of roughness on L-band Tb.

6. Results from the comparison between modelled and observed Tb are not promising and are difficult to interpret here. I had many questions looking at Figs 9 and 10 that were not discussed within the text. Based on the theoretical results in Fig 6, you'd probably only expect an improvement to the 45-degree angle v-pol channel when including the effects of roughness, right? So the poor correspondence between model and observations is likely one or more of: model inaccuracy, the model configuration (no. layers, penetration etc.) not being adequate, simple treatment of ice thermodynamics, not having altimeter observations for the 45-degree footprints, or the limited treatment of snow. Without showing results from a model sensitivity analysis of these factors though, it is impossible to interpret which factor or set of factors is most likely. Why is there such a low dynamic range for modelled Tb's in most cases where mea-

sured Tb >220-240K? Can you add another figure showing the absolute differences between modelled Tb for simulations with and without GO roughness included, perhaps as histograms or as a function of the surface roughness?

7. The written English needs some improvement throughout the manuscript. I would recommend a careful proof-read to check spelling and grammar. A few e.g.'s just on the first page are: L2 'rely on', L8 you mean 'horizontal polarization'?

Minor comments/edits:

Page 1. Line 7. Effect on what? What scale of roughness? Multiple scales?

L 18. Surface roughness of the ice or snow, or both?

P2 L5. Both high and low spatial frequencies. . .

L11. What do you mean by 'stays unnoticed'? rephrase

L18. What is 8*lambda for L-band?

Intro Section. What about the other factors affecting Tb from sea ice? They are not the primary focus of this study, but can complicate your interpretations of the roughness effects, particularly when comparing model results to radiometer observations. So you should introduce the effects of e.g. thermodynamics, ice concentration, snow properties etc. here. Are there any previous estimates of the impact of roughness on L-band Tbs?

P3 L8-9. What was the air temperature then? Thermodynamic effects on the snow properties may explain your difficulties comparing model and observations, and the large impact of a snow layer on your simulations then?

P4 L1-3. Can you provide a little more detail on this as its such a substantial bias? How do you know it was purely additive? How was this tested?

P4 L10. Here I found myself asking if you used the same roughness data from nadir to simulate the 45-degree return. This was answered much later but you should state it here.

P5 L2-3. How was the sea level estimated from lead tie-points? Do you have uncertainties for the sea level, freeboard and ice thickness, that you can apply to estimate uncertainties in modelled Tb? How did you estimate snow depth uncertainty when applying a simple snow scheme?

L4. Do you use an iterative procedure to estimate ice thickness then, if the thickness is already required to estimate snow depth?

L11. Do you have a citation for the version of MILLAS used here, or was this added work completed as part of your study? If the latter, you need to describe model additions including equations for review (perhaps in an appendix).

L16. How are the ice and water salinities calculated? (You need to make it clear here that you use the ice surface T from airborne observations to constrain ice thermodynamics in the simulations). I'd like to see a table here of the constants used for ice, water and snow physical parameters, and then the range over which other parameters (e.g. roughness) varied.

L30-31. It's important here that you state ALS observations of roughness are averaged over quite a long window. As it's currently written, it sounds like you are simulating and comparing with measured returns over the exacts same 70 m window.

L34. Use a proper citation style for this reference.

P6 L4-5. Did you filter out all 70-m sections containing mixed classes, e.g. some open water? What about thin leads within the footprint? I'd expect many of your eventual 5-km sections contained at least some open water, so how was this accounted for?

L13. 'coast'

P7 L1-2. I'd like to see a figure which proves this. This cutoff limit between anisotropic

and isotropic orientation of surface features has not been shown before, so a novel result of this study. But if you want to prove there is a scale separation at 4.3 km you need to show the data

L10. Can you show the exponential function fit to each class of data in Fig 3, so we can see how well it performs?

Eq 9. What is R?

P10 L3-4. Is it reasonable to assume a constant gain pattern over the entire FOV? Do you have an estimate of the antenna pattern to compare to?

L10. So the T profile is calculated directly from surface T and the reference constant salinity?

P12 L5. If you refer to angles in degrees within the text, the x-axis in Fig 6 should also be in degrees

L9. 'And'?

L12-13. Confusing. What do you mean by this?

P13 L3. 'High'?

P13 L16-31. I can't understand why this section is included, along with Figures 7 and 8. Why not just calculate reasonable variations in MILLAS emissivity for different sea ice scenaios? E.g. warm/cold ice, different salinities, shallow/deep snow, different snow T or densities? Relative permittivities up to 10-20 are unrealistic for sea ice in most conditions, so they are not helpful for your analysis here. There's only really reason to show the cases listed directly on Line 24.

P15 L5. State this earlier in the method.

L7. How do you decide when it is needed?

L8-9. Is the roughness CDF calculated from all altimeter observations within this 5-km window then?

P18 L8. Unlikely permittivity but possibly thickness. What about open water within footprints? Could that have affected the radiometer measurements? Or maybe snow depth/property variations along track?

L9-10. You at least had the facet orientation info at least for the nadir looking antenna right?

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