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The Cryosphere Discussions

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

Maciej Miernecki et al.

maciej.miernecki@cesbio.cnes.fr

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Dear Dr. Landy,

Thank you for your remarks and time that you took to analyze the manuscript. Following your informative comments we include new subsections explaining more in detail the analysis of orientation of the surface slopes and a section dedicated to the sensitivity study of the model. The sensitivity analysis places the results in the context of the current and future L-band missions. Implication on the SMOS sea ice thickness product (SMOS SIT) and on SMAP and CIMR TB are now added to the manuscript. According to your suggestions, we also changed the wording of some sections. We took extra

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care to address all your remarks, nonetheless if some points require further attention we will gladly provide more exhaustive response.

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? Dothe current incidence angles employed by SMOS limit the sensitivity of measured Tb toroughness? I expected to see a statement on this in the abstract and some discussion later on the manuscript.

»> The discussion is now included in the manuscript: The SMOS SIT is derived from near-nadir measurements (0-30degrees) therefore the expected change in the TB due to the large scale surface roughness (up to -2.6K) is negligible compared to the uncertainty associated with other factors, such as sea ice concentration (-1.5K/%), and snow cover (8.5K/m)

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?

»> The altimeter measures the snow surface elevation but we lack snow thickness data. Therefore we have to assume that snow is plane-parallel layer to the sea ice surface. Although dry snow is transparent in L-band it has an impact on ice thermodynamics, which determine the ice effective temperature and emissivity (snow layer also refracts the radiation, but it is more relevant for higher incidence angles).

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In the sensitivity study we calculate the model sensitivities for different assumptions about snow thickness. The importance of the snow thickness is now illustrated on two new figures: 8 and 9. The change of TB due to the snow thickness is up to 18K (from 0 to 1m) for sea ice surface temperature of 250K.

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 5km, 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 radiometerTb 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?

»> Regarding the isotropic orientation of surface facets. We now include the analysis of the surface slopes azimuthal orientation: figure3. Although the comparison with the radiometer data is not conclusive at 5km, we aim at generalizing the roughness parametrization for a larger region, so as to infer the implications for SMOS/SMAP/CIMR (resolution of 40km), at such scale it is less likely that the sea ice will have coherent ondulations on the surface. The impact of such oriented sinusoidal surfaces on the angular characteristics is discussed in Ulaby and Long (2014) in chapter 10.4., which served as an inspiration for our approach.

In the simulation the shadowing occurs when the local incidence angle is greater than 90deg, therefore radiation from such facet is emitted away from the antenna. Current version of the statistical model, with the facet orientation drawn from the CDF, does not account for the “double-bounce” reflections.

4. Could you not just use the observed empirical CDF within each 70 m foot-

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print,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(70ËĘ2/0.5ËĘ2 is only about a factor 2 larger number of facets than your 10ËĘ4 criteria), or so you can calculate average model results over 5 km sections? Using the observed CDFmay produce a better model fit to the radiometer observations.

»> We considered the direct assimilation of the facet orientation from the DEM but this method will be only applicable to the nadir antenna as the side-looking footprint is not scanned. Another associated issue is the assumption on the ice thickness and snow thickness of each facet. Although these can be substituted with constant values for the whole footprint. Additionally, the position of the facets within the antenna gain function, which is unknown for this particular system mounted on the airplane. The assumption of the constant gain that we used in the manuscript is more accurate as the signal is averaged over a larger distance. All things considered, the assumptions that have to be made and rather small expected signal (up to 8K) directed our efforts to establish a simple robust parametrization which considers an isotropic slope orientation.

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.

»>Thank you for this suggestion, the sensitivity analysis is now included in the

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manuscript. Based on it we present that surface roughness has a much smaller impact on the L band TB of sea ice than snow cover. New figures: Fig 8 and 9 show the non-linear and non monotonic relation between TB and surface temperature, plotted for different snow thicknesses. Table 2. Contains the partial sensitivities to of the TB to the various inputs, for lower and higher temperature ranges as well as for set of assumptions on snow thickness. The figures 7, 8 are removed.

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 in-cluding 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 be-ing adequate, simple treatment of ice thermody-namics, not having altimeter ob-servations 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, per-haps as histograms or as a function of the surface roughness?

»> The discussion of the comparison between modeled and measured TB is now interpreted with the sensitivity study in mind. The simple emission model used in our study (one layer of ice with one layer of snow) has much more sensitivity to the snow thickness than to the surface roughness. We added new figures showing the histogram of the differences between measurements and the simulations setups for all four antenna feeds.

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', »> corrected

L8 you mean 'horizontal polarization'? »> corrected

Minor comments/edits:

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

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

P2 L5. Both high and low spatial frequencies. . . »> corrected

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

»> rephrased to: the surface roughness in negligible

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 proper-ties etc. here. Are there any previous estimates of the impact of roughness on L-bandTbs?

»> There are a number of studies regarding effects of surface roughness on the L-

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band TB of soils, as this wavelength is widely used for soil moisture retrieval. To our knowledge this is the first time that the impact of surface roughness is on L band emissions from sea ice is evaluated.

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?

»> During the 24. of March The average Sea ice surface registered by the KT19.85 was 251.7+/-3.5K. As the sensitivity analysis indicate, the assumption on snow thickness being a constant fraction of the sea ice thickness is a major source of uncertainty.

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?

»>According to the campaign report Hendricks et al.2014 the bias was determined and corrected by taking measurements over open water and performing so called wing wags to cross calibrate the two antena fids. Also the RFI filtration system based on frequency and time sampling masked out the samples that were showing characteristics of an artificial source.

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. »> corrected

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? »> According to the campaign report Hendricks et al.2014, the tie points were picked manually giving the reference

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for the elevation measurements. The sea ice thickness is calculated from resampled to 1second ALS data, we took the standard deviation of the 1s sample as uncertainty. We estimate the snow thickness as a constant fraction of the sea ice thickness, thus we assume a propagation of uncertainty derived from elevation uncertainty.

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

»> Unfortunately not, we assume a hydrostatic equilibrium and that the snow thickness is 1/10 of the ice thickness.

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 addi-tions including equations for review (perhaps in an appendix).

»> corrected. We included a short description in the manuscript.

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 thermody-namics 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. »>Thank you for this suggestion, we include the formulas and constants used in MILLAS in the Table 1

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. »> corrected

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?

»> We compute the surface roughness statistics from 70m section treating it as a one class. We exclude from the analysis the 1s sections with more than 5% missing data. This rather crude method assumes that thin ice or open water will not reflect the laser scanner resulting in missing data. An alternative would be to use the camera images, but those were not taken continuously.

L13. 'coast' »> corrected

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

»> New section: Section 2.2.1 no describes the azimuth orientation of the facets along the flight track with the figure

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?

»> R is the distance from facet to the antenna, explanation is now added together with the fits. Although, we show three example of smooth; medium and rough ice as placing all the classes cluttered the figure.

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?

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»> We do not have the antenna pattern for the setup during the campaign. It is possible to make assumptions about it based on the horn size, however when mounted on the plane the sidelobes and gain will vary. When considering the case of isotropic slope orientation the antenna gain can be considered constant.

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

»> Yes, according to the formulas presented in Table 1: (after Untersteiner, 1964) Snow thermal conductivity = 0.31W/(mK) Ice thermal conductivity = 2.034 W/(mK)+0.13W/m * Sice(g/kg)/Tice(K) Ice salinity = 4 g/kg 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'? »> corrected

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

P13 L3. 'High'? »> corrected

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 icescenaios? E.g. warm/cold ice, different salinities, shallow/deep snow, different snowT 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.

»> corrected, The section is reedited.

P15 L5. State this earlier in the method.

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L7. How do you decide when it is needed? »> corrected, refrazed

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

»> yes. The CDFs are computed from all altimeter observations within 1s section that have less than 5% missing data.

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?

»> The missing data criterion from previous point partially solves the problem of smooth thin ice or open water within the footprint. However, the sensitivity to open water is much greater than that to the surface roughness. Corrected to "...heterogeneous in terms of its thickness"

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

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