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Interactive comment on "Microwave snow emission modeling uncertainties in boreal and subarctic environments" *by* A. Roy et al.

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Reviewer #1 (Comments):

This paper concerns the modeling of microwave radiation from snow with DMRT-ML, to quantify the simulation sensitivity to parameter uncertainty. This is the most complete study to date due to the consideration of the forest contribution as well as density, grain size, ice lens and soil roughness variability or uncertainty, with the latter effects providing no surprises based on other similar studies. In addition, to my knowledge, this is the first study to look at the effects of the bridging assumption between low and high density snow in the context of real data. The nominal simulations are considered to include a given grain scale factor, the density bridging assumption and appropriate

treatment of ice lenses, before looking at other effects, which is a logical approach to take.

R1-C1 : This study looks at 3 different sites within the Canadian sub-Arctic, which gives a range of snowpack properties, but makes the paper somewhat hard to read. Due to the wide range of measurement locations, a figure with the sites indicated on a vegetation map would be useful.

We inserted a map of the 3 locations with the Land Cover of Canada 2005 (Latifovic et al., 2004) in background.

R1-C2 : In the James Bay measurements, the mean snow density from January to February decreased, and with minimal increase in grain size. Is this expected for this site? Is this due to the influence of recent precipitation, or spatial variability in the measurement locations? Also, how is the mean grain radius calculated?

The lower density in February is related to an error in density calculation. The snow density calculation was done considering the ice lenses. All the corrections were made (See also R2-C6)

The mean grain radius is the mean Ropt per layer weighted by the snow layer thickness. It is now mentioned in the text:

"During the JBJan campaign, 16 open area sites were measured where the mean snow (weighted by snow layers thickness excluding ice lenses) of all snowpits was 295.5 kg m-3 and the mean Ropt (weighted by snow layers thickness excluding ice lenses) was 0.17 mm (Table 1)."

R1-C3 : Constant soil parameters from a different study were used here (section 2.2.3). The authors must comment on the applicability of these parameters to the sites chosen for this study. The experiment presented in section 3.2.1 considers the effect of the roughness of the soil, but not the permittivity, which governs the Fresnel reflectivity and is a more fundamental parameter. The authors note this limitation later in the section, but I do not agree with their statement (pg 5732, line 15) that this does not affect their main goal. It may do, as the variability in the permittivity may cause greater snow TB variability than is possible to simulate with adjustment of the roughness alone. The authors should justify why a particular constant value of permittivity derived elsewhere is an appropriate assumption here or base the sensitivity on permittivity rather than roughness variability.

The parameters were inverted from independent angular measurement taken during the same campaign at James Bay. We thus clarify that point in Section 2.2.3:

"The values of ε', σ and β at 11, 19 and 37 GHz inverted by Montpetit et al. (2015) for frozen soil (Table 6) were used in this study. Montpetit et al. (2013) used independent snow free ground-based radiometer angular measurements taken at James Bay site in 2013 (same campaign). The parameters were also validated over Umiujaq (same campaign) snow removal experiment."

Because we use minimal and maximal values of optimized σ the range of TB variability of frozen ground is well represented. Also, the permittivity used were retrieved at the same site:

"However, one should be careful in interpreting these results as the optimization could also compensate for uncertainties in the permittivity of frozen ground. Nevertheless, because the minimal and maximal values of optimized σ are taken, this does not affect our main goal, which is to estimate the variability in snow-covered TB introduced by the soil in the model. Furthermore, as mentioned in Sect. 2.2.3, the permittivity used in this study were retrieved at the same site as this study."

R1-C4 : Pg 5730 line 9-11. This is really hard to see in the figure. There are multiple outliers that easily cover this range in the simulations, so this sentence should be more precise.

We add a dotted line in the Fig. 6 to clarify the point.

R1-C5 : Figure 6, right doesn't add much to the message of the paper and diverts attention as there are many figures in this paper. As it has already been summarised in a single sentence I would recommend removing the figure.

We removed the figure and change the paragraph:

"To test the bridging parameterization (see Sect 2.2.2), we used 13 tundra sites from the Churchill tundra database (Roy et al., 2013), 4 from Umiujaq and 2 from the James Bay snowpits. In each case, at least one snow layer with a snow density higher than 367 kg m-3 (ice fraction of 0.4: Dierking et al., 2012) is used. For each of the 19 sites studied, simulations at 37 GHz (the most sensitive frequency to snow) with and without the bridging implementation were conducted (all input parameters kept the same). The bridging has a relatively modest impact on simulations with an improvement in the RMSE of between 2 and 4 K at tundra sites (Umijuaq and James Bay). The greatest improvements are found for deep drifted tundra snowpits where there is a very thick wind slab with high snow and small rounded grains are present at the top of the snowpack."

R1-C6 : Section 3.2.4. How was the density of ice lenses measured in the field and what was the result (alternatively this comment may belong in the next section if 'was attempted' should be replaced with 'was not attempted').

We replaced for "was not attempted".

Technical comments (R1):

pg 5724 line 6. Make clear that the SSA is per unit mass rather than per unit volume.

We clarified the units

pg 5724 line 15 and onwards. JB may be a better, easier to read acronym than BJ.

We changed all the acronyms.

pg 5726 line 22. As a scaling factor of 3.3 has been applied following previous work, presumably non-sticky grains are assumed in the DMRT-ML simulations. This should be stated.

We changes the sentence :

“As such, a scaling factor of ïĄęïĂă= 3.3 assuming non-sticky snow grains from Roy et al. (2013) for the seasonal snowpack is thus applied to get an effective radius in the microwave range (Reff)”

pg 5727 line 21. In setting ice lens thickness to 1cm, how are the thicknesses of the adjoining layers adjusted, or is the overall depth of the profile in the simulations allowed to differ from the measured depth?

We mentioned that in the text :

“To keep the same total snow depth, the adjoining layers were adjusted by removing 0.5 cm of the layer above and below the ice layer.“

pg 5728 line 11. This should be > 350, not < 350.

It was corrected

pg 5729 line 20. The bridging implementation was tested for simulations based on snowpit data rather than tested on snowpits themselves.

Done

pg 5734 line 12. gains -> grains

Done

lg 5736 line 11. weaker -> less

Done

Reviewer #2 (Comments):

General comments The authors present a study assessing uncertainties in microwave emission modeling from snow covered ground, arising from uncertainties in assigning

model inputs from in situ information. One model, the DMRT-ML by Picard et al. is applied for the purpose. A set of surface-based radiometer measurements is used as a reference to model predictions. While not very original, the paper contributes regardless to an important topic in snow remote sensing. The results of the paper should be useful especially in guiding data collections in future, large scale campaigns of snow cover using passive microwave radiometry. The paper is well written and clear. However, I have some questions regarding the methodology applied, and would suggest the authors revise some of their conclusions before publication. See detailed comments in the following.

R2-C1 : Abstract, lines 42-43 and several places later on. Based on what is in the end a rather limited dataset, you draw conclusions that variations in the emission of frozen soil has only a small effect on brightness temperature of snow covered terrain. You alleviate this conclusion somewhat in the discussion (723-732), but it comes out very strongly in the abstract, which I feel is misleading. It is, for instance, unclear if the sites you had contained multiple soil types or not; the ‘frozen’ permittivity of clay rich soils, for example, will be quite different from that of mineral soil types, due to the ability to store free water even in sub-zero conditions. Organic soils represent yet another different scenario, as well as soils with a high saline content. You only have to look at e.g. SMOS data during winter to see that there are variabilities during the winter which clearly arise from soils with a different permittivity. It would be good to better bring out the limitations already in the abstract (i.e. your experimental findings apply only to a certain soil type), if you wish to raise this point at all

We clarify the results in the abstract. :

“A snow excavation experiment – where snow was removed from the ground to measure the microwave emission of bare frozen ground – shows that small-scale spatial variability (less than 1 km) in the emission of frozen soil is small. Hence, in our case of boreal organic soil, variability in the emission of frozen soil has a small effect on snow-covered brightness temperature (TB).”

We also clarify in the results section (the soil types were mentioned in Sect. 2.1.1) that the BJJan-transect were made in an old gravel with mineral soil and, the other were made in organic boreal soil:

“The analysis of small-scale soil variability in modeling the TB of snow-covered surfaces is conducted using the SEex from the transect during the JBJan (mineral soil) and JBFeb campaigns (organic soil).”

“TB variations of 0.5 K and 1.3 K were observed at the JBJan-transect site where the soil properties were more homogeneous (mineral soil), while a variation of 0.7 K to 3.8 K was measured at the JBFeb site with organic soil (Table 8).”

R2-C2 : Abstract, lines 51-52, also later on: it is bit of a no-brainer that downwelling emission from trees affects the measured Tb. This is well known and it would simply be a mistake not to include the downwelling canopy contribution – thus is a bit awkward to bring this out here. The absolute value of the Tb contribution, will be highly dependent on tree type and canopy conditions (frozen/thawed, snow covered/bare), as well as snow and soil reflectivity, thus I would refrain from giving a value here. If you insist, you should at least make it clear that this finding applies only to your study, test sites and the local conditions that prevailed.

From our knowledge, it is the first time that the contribution of forest emission reflected by the surface are quantified from ground-based radiometers. We think it is important to include in the abstract. We however mentioned in the abstract that the results are specific for our dataset:

“The results also show that, in our study with the given boreal forest trees characteristics, forest emission reflected by the surface can increase the TB up to 40 K. The forest contribution also varies with vegetation characteristic and a relationship between TB-down and the proportion of pixels occupied by vegetation (trees) in fisheye pictures was found.”

R2-C3 : Introduction, lines 105-107 and elsewhere: Here, you claim to “quantify the sources of uncertainty in the DMRT-ML model”. However, in the previous paragraph, you point out the two basic contributions to simulation (not model!) uncertainty: 1) model physics going wrong 2) insufficient or inaccurate input information. To me, the whole paper is about assessing point 2), as is in fact also pointed out in the discussion (line 740-741). This is an important distinction and you should be careful with the wording throughout the paper. In my view a better wording here would be something along the lines of: “we aim to quantify the relative importance of uncertainties of in situ information, when simulating microwave TB with the DMRT-ML model”.

We clarify the main objective of the paper:

“Hence, this paper aims to better quantify the relative importance of different geophysical parameters and small-scale spatial variability when simulating microwave TB with the Dense Media Radiative Theory-Multilayer model (DMRT-ML; Picard et al. 2013).”

We also clarify that point in the first sentence of Sect. 4 :

“This study presents a comprehensive analysis of the geophysical parameters contributing to uncertainty in DMRT-ML for snow-covered surfaces in boreal forest, subarctic and arctic environments.”

R2-C4 : Lines 111-112: sentence a little bit incomplete. Add something like “: : : snow emission modeling: inaccuracies in quantifying snow grains, snow density”

The proposed change was done

R2-C5 : Section 2.1, lines 163-165. Measuring SSA may be robust, but being a definition for optical wavelengths, how well is SSA related to the propagation of microwaves in snow? I’m sure the authors are aware of this ongoing discussion. You do not have to delve deep into the problem here, but at least the question and the related discussion should be acknowledged by citing some recent work by e.g. Mätzler, also justifying why you use SSA regardless of the acknowledged limitations.

To our knowledge, SSA is the only robust and objective metric that can be measured in situ. We are now working on a paper showing that SSA is a better metric than Dmax. We developed in the discussion on the justification of the use of SSA with some limitation.

"The error related to the physical simplifications in DMRT-ML was not investigated in this work, but our results suggest that the level of confidence of measurements is too low to test or significantly improve the DMRT-ML physics. In this study, SSA was used because it is a robust and objective metric that can be measured effectively on the field. However, the derived Ropt metric used in DMRT-ML is related to an optical definition (Grenfell and Warren, 1999) and might not represent the grain for microwave wavelength (see Mätzler, 2002). Further experiments on isolated snow layers as done by Wiesmann et al. (1998) but using new tools for snow microstructure parameterization could be applied to improve the physics of emission models. For example, more precise measurements of snow microstructure like X-ray tomography (Heggli et al., 2011) and the snow micro-penetrometer (SMP) (Schneebeli et al., 1999; Proksch et al., 2015) could be the next step to improve the understanding of the physics in DMRT-ML (e.g., Lowe and Picard, 2015)."

R2-C6 : Tables 1-5: the STD values for snow density seem very high to me, especially for Tables 1&2. Can you check these? If the values are correct, do you have a reason for the high degree of variability?

The average density and standard deviation of snow density calculation included the ice lenses. The values were recalculated without the ice lenses to give a better view of the snowpack characteristics. The values were corrected in the text as well (See also R1-C2).

R2-C7 : Section 2.2.3, line 340: epsilon' is the conventional symbol for the real part of the dielectric permittivity. The Fresnel reflectivity depends on < epsilon_r> = < epsilon'+ j* epsilon">. Is your epsilon' the real part of the soil permittivity (thus neglecting the

imaginary part, a reasonable approximation for frozen soil), or the magnitude of the complex permittivity?

We clarify in the text that it is the real part of the soil permittivity:

"where Γf,p is the rough soil reflectivity at a frequency f and polarization p (H-pol or V-pol) by its smooth Fresnel reflectivity in H-Pol (Γf,H), which depends on the incidence angle (θ) and the real part of the soil permittivity (ε'), weighted by an attenuation factor that depends on the standard deviation in height of the surface (soil roughness, σ), the measured wavenumber (k) and a polarization ratio dependency factor (β)."

R2-C8 : Section 3.1.1., line 374-376: what is the fundamental reason for the simulation to go wrong, when the effect of ice lenses is not included? Is it not very informative to just state that "improved simulations" are achieved with ice lenses. In other words, what is the physical effect that the ice lenses manage to simulate, which was lacking in the original simulation? This should be explained.

We clarify that the ice lenses inclusion allows taking into account its strong reflectivity:

These results show that a simple ice lens implementation in DMRT-ML helps to simulate the strong reflection component of ice lenses (decrease of snowpack emissivity), leading to improved simulations of TB.

R2-C9 : Still on ice lenses: can you elaborate your statement on line 380: what, in your view, are the reasons for the limited range of simulated values versus observations, if this is related to ice lenses. Coherence effects not accounted for by DMRT? Or, can this be something different (the soil perhaps?). coherence is mentioned in the discussion, but something could be pointed out here.

We chose not to elaborate on the reason for the limited range because it is well discussed in Sect. 3.2.4 and Sect. 4. But after review we add some sentences to start the discussion on that point:

"This feature suggests some limitations of ice lens and/or snow layering modeling in

DMRT-ML that can be related to the fact that coherence effect is not taken into account. Note that this underestimation of TB spatial variability is not related to the soil as demonstrated in Sect. 3.2.1. The modeling uncertainties related to ice lenses will be discussed more specifically in Sect. 3.2.4."

R2-C10 : Lines 417-418: rather than '5-10 K' and '10-20 K', give precise numbers. Note e.g. that RMSE at 11H exceeds 20 K, and RMSE for 19V lower than 5 K.

The numbers were updated:

"The RMSE values oscillate between 7.8 and 21.5 K at H-pol (Table 7). Since V-pol is less affected by layering in the snowpack at 11 GHz and 19 GHz, the RMSE are generally lower (between 3.5 and 14.4 K), while the RMSE at 37 GHz are similar at V-pol and H-pol."

R2-C11 : Figure8: error bars in scatterplot not very informative, they only make the symbols hard to read. I suggest to remove these

We think that the error bars allow showing that the effect of soil emission small-scale spatial variability has a very low impact on TB. For this reason we think it worth to keep the error bars.

R2-C12 : Section 3.2.2: summing of errors; is the 12% error in SSA considered random, or systematic? I think random? Then, you should rather perform a sum-of-squares addition of the errors, depending on how many measurements were used for a given snowpit [e.g. for three SSA measurements used in a sim: err_tot = sqrt(err_1ËĘ2+ err_2ËĘ2+ err_3ËĘ2) = 0.21]. I suggest to redo the analysis (Fig9, Table 9) in this fashion.

In our case, the error of 12% is added to the SSA of each layer of snowpack. Hence, the TB error account for the sum of the error of each layer. The error in TB resulting from the uncertainties in SSA measurements for each layer correspond to an integrated errors which are not independent from each other, since there are multiple scattering

between the layers. In the Table 9, we show the extreme variations in TBs for the extreme cases where all the errors in SSA are in the same way (all positive + 12%, and all negative -12%). This gives the limit cases. In reality, one can assume a random error in SSA measurements between + and - 12% to calculate TB with a random error applied on SSA (TBsimrand). We assessed the average variation in TBs (TBsim - TBsimrand) resulting from 100 runs with random error ($\pm$ 12) in SSA for each layers of every snowpits. The Table R.1 below shows the results. As expected, the variation is significantly lower than those shown in Table 9 of the paper. This Table gives the lower limits , while the Table 9 gives the highest limit.

Table R.1: TB variation (TBsim - TBsimrand) associated with random error ($\pm$12%) applied to each SSA measurements (average of 100 runs) JBJan JBFeb JBMar UMI 11H 0.0 0.1 0.1 0.0 11V 0.0 0.1 0.1 0.0 19H 0.3 0.6 0.8 0.3 19V 0.3 0.6 0.9 0.3 37H 2.1 2.0 1.7 1.2 37V 2.7 2.4 2.1 1.4

We added the following sentence in the text:

"We assessed average variation in TB resulting from 100 runs with random error between $\pm$ 12% applied to SSA for each layer and snowpit. As expected, the results show that the variations between initial simulation and simulation with random error on SSA are significantly lower than those shown in Table 9. With random error applied on SSA measurements, the variations are lower than 1 K at 11 and 19 GHz, and between 2 and 3 K at 37 GHz. These values give the lower limits of TB error related to SSA uncertainties, while values in Table 9 give the highest limit of the variation in TB."

R2-C13 : Line 595: I think you mean "was NOT attempted".

It was changed.

R2-C14 : Conclusions, p26, lines 760-765. You could cite Derksen (2008) in the discussion here, who suggested the use of 11-19 GHz in place of 19-37 GHz for deep snow to alleviate saturation effects at 37 GHz.

We added a sentence on the possibility to use 11-19 instead of 19-37:

"This could be of interest for the SWE retrieval approach, knowing that 19 GHz TB becomes sensitive to snow when snow grains become larger. As proposed in Derksen (2008) 11 and 19 GHz frequencies could be usefull for SWE retrievals for deep snow to overcome the problem of saturation at 37 GHz (see Rosenfeld and Grody, 2000). At 11 GHz, snow is almost transparent throughout the winter demonstrating the utility of this band for monitoring soil conditions (phase, temperature) under the snow (Kohn and Royer, 2010). "

Editorial P11, line 333 delete "therefore"

Done

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C2841/2016/tcd-9-C2841-2016-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 9, 5719, 2015.