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The Cryosphere Discussions

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

Anonymous Referee #2

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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

Specific comments 1. 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

2. 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.

3. 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”

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

5. 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.

6. 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?

7. 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?

8. 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.

9. 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.

10. 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.

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

12. 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_1ˆ2+ err_2ˆ2+ err_3ˆ2) = 0.21]. I suggest to redo the analysis (Fig9, Table 9) in this fashion.

13. Line 595: I think you mean “was NOT attempted”

14. 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.

Editorial P11, line 333 delete “therefore”

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