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Interactive comment on "The microwave emissivity variability of snow covered first-year sea ice from late winter to early summer: a model study" *by* S. Willmes et al.

Anonymous Referee #1

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General Comments

This paper describes novel coupling of a physical snow model (SNTHERM) with a multi-layer snow emission model (MEMLS) to address seasonal variability in Arctic and Antarctic sea ice emissivity due to the overlying snowpack. This analysis provides a basis from which the impact of emissivity variability on satellite passive microwave sea ice concentration retrievals can be determined. While the modeling approach is unique, I feel a number of issues must be addressed before the paper is suitable for publication:

1. Snow initialization – Is there a reason why 30 identical snow layers were used as

the initial conditions? Wouldn't only 4 or 5 layers be necessary to characterize typical winter season snow on sea ice, and shouldn't the characteristics of each layer be unique? Wouldn't it be more physically realistic to specify 2 or 3 fine grained wind slab layers overlying a thin faceted layer at the snow/ice interface? I think it's okay to use the same initial conditions at all sites, but further justification/explanation is need for using a large number of identical layers, which essentially creates a vertically uniform snowpack. The use of an unnecessarily large number of layers makes it more difficult to diagnose the sources of uncertainty in the MEMLS simulations.

2. Coupling physical snow models with emission models has been done in the past for terrestrial snow applications, as in the Langlois et al (2012) paper which is cited (but there are others. . .). The key issue is the use of grain size parameters from the physical snow model as direct inputs to the emission model. A grain scaling factor has been required in all previously published studies, and this factor is not consistent but varies depending on choice of models, snow characteristics, etc. Because of this, additional information on the SNTHERM derived grain size and the 0.12 scaling factor would be very useful. I suggest adding a figure or table which summarizes the seasonal evolution of SNTHERM derived snow characteristics which are most important to MEMLS (i.e. grain size; density; wetness). This would illustrate the seasonal evolution of the ERA-forced, SNTHERM simulated snowpack properties, which are fundamental to influencing the emissivity. Additionally, precipitation was not considered. This means the 30 cm snowpack metamorphosed (as forced by ERA meteorology) but new snow layers were not added. Showing the seasonal snowpack properties would again illustrate the impact of simulating only metamorphosis in the absence of accumulation.

3. Figure 2: The observed Tb values are shown in gray for cases when sea ice concentration was >90%. Presumably cases thrown out when ice concentration was <90%. This should be stated explicitly.

Page 5718 line 1: "The simulated data align well close to the 100% sea-ice concentration lines revealing at the same time a distinct variability that is more pronounced in the

Antarctic within both feature spaces." It's not clear what is meant by 'distinct variability'. It appears from Figure 2c and 2d that the simulated Tb fall over a much narrower range than observations, which indicates reduced variability in the simulations versus observations. Can an explanation be provided for the differences in the range of simulated versus observed Tb at both 19 and 37 GHz? One way to look at this would be to plot frequency distributions of the simulated versus observed Tb. Additionally, I suggest computing statistics to quantify the agreement between simulated and observed Tb. For instance, what is the RMSE and bias?

Page 5718 line 3: "Especially during the last month of our simulation period (Arctic: June, Antarctic: December), the beginning effect of surface melt causes the position of the simulated snow profile to move towards the open water tie point. This is expressed by elongated clusters pointing towards lower sea-ice concentrations mainly for the Arctic simulations." This is would be easier to view in Figure 2 if a different colour was used for each month, instead of black and grey. Related to my previous point, distributions and/or statistics could be used to illustrate how the agreement between simulated and observed Tb varies from mid-winter to the melt season.

4. As noted in my comments above, characterizing the uncertainty in the Tb simulations is important to illustrate the robustness of the coupled SNTHERM – MEMLS modeling setup. What is the impact of uncertainty in the simulated Tb values (Figure 2) on the emissivity values shown in Figures 3-5? Can you estimate how an uncertainty of x% in Tb translates to an uncertainty of y in emissivity? Some additional details on the uncertainties in the estimated emissivity values would be helpful.

5. Figure 3: The differences in the emissivity time series for Arctic versus Antarctic snow is attributed to phase related processes (page 5718 line 20). Can you clarify "…the ratio of evaporated snow mass to melted snow mass…"? Do you mean sublimated?What processes are driving the large difference in this ratio?

6. Figure 6a: How were these penetration depths calculated? This is not clear in the

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text. What is accounting for the slight reductions in penetration depth between months 1 and 4 when snow conditions are still dry, and why are 19 and 27 GHz penetration depths the same for these months? Essentially, you have a wetter, denser more emissive snowpack in the Antarctic due to a more ambiguous melt season than occurs in the Arctic. I suggest clarifying the text that describes Figure 6.

7. Page 5721 line 5: Were these trends calculated over the 2000 to 2009 time period? That's a short period for trend analysis. This paragraph seems like a cursory add-on, and I suggest removing it.

8. As stated in the Discussion, the contribution of the overlying snowpack is one of many sources of uncertainty in passive microwave sea ice concentration retrievals. While emissivity variations due to the snowpack were characterized in an innovative fashion, I was left wondering what the impact actually is on sea ice concentration retrievals, and the relative impact of snow characteristics compared to atmospheric effects, surface flooding/draining, sub-footpring heterogeneity, etc. While beyond the scope of this study, a full retrieval sensitivity study is really needed to solve this issue. . .

Editorial Comments

Page 5712 line 21 ". . .since more than 30 yr. . ." change to ". . .for more than 30 years. . ."

Page 5713 line 3: "A more detailed retrieval. . ." 'detailed' is not the right word choice here. I think what you mean is retrieval of additional sea ice parameters (beyond concentration).

Page 5713 line 21: change 'snow decay' to 'snow melt'

Page 5713 line 29: ". . .the problem that layered snow causes a low sea-ice concentration bias. . ." Can you expand on the uncertainties due to snow stratigraphy? Is it primarily due to larger faceted grains at the base of the snowpack, crusts/lenses from melt/refreeze events? A bit more detail here would be helpful.

Page 5714 line 9: "We do not take into account a pre-conditioning of the snow cover. . ."

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Not clear what is meant by pre-conditioning.

Page 5718 line 27: remove “exemplary”

Page 5720 line 4: replace ‘subsumed’ with ‘shown’

Figure 6c: x-axis label is incorrect

Page 5722 line 7: what is meant by ‘gap layers’?

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