The Cryosphere Discuss., 7, C2938–C2945, 2014
www.the-cryosphere-discuss.net/7/C2938/2014/

The Cryosphere Discussions

TCD

7, C2938–C2945, 2014

Interactive Comment

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 #2

Received and published: 2 January 2014

The overall objective of the study carried out by Willmes et al is to quantify the differences in microwave emissivities in several regions of the Arctic and Antarctic due to different weather conditions (specifically air temperature, humidity, wind speed, and short- and long-wave radiations). To that end, the authors used two one-dimensional models modified for sea ice: the thermodynamic snow model SNTHERM, and the microwave emission model MEMLS. The initializations of sea ice and snow properties were simplified, and applied uniformly in every location. The atmospheric forcing was extracted from ERA-interim, and applied every six hours from 2000 to 2009. Only the first (last) six months of each calendar year were considered in the Arctic (Antarctic). Results show that the ranges of simulated brightness temperatures at 19 and 37 GHz (vertical polarization) do not cover the range of observations. The analysis of the simulated emissivities reveals that higher variability values were obtained in Antarctica than in the Arctic. It is suggested that freeze-thaw events, and the timing of melt may contribute to this hemispheric difference.

The approach considered in the study of Willmes et al is interesting and has some potentials. As mentioned by the authors, the approach could be used to refine retrieval algorithms of sea ice concentration and snow depth. However, the accuracy of the simulations is not shown. Therefore, the conclusion of the paper appears very high, especially since the study is limited to sea ice concentration higher than 90%, for which algorithms often have the best performance.

The results are valuable and could be suitable for The Cryosphere. However, as it stands, the manuscript still needs further work regarding: 1) the snow simulations with the SNTHERM model; and 2) the rational about the hemispheric differences in simulated emissivities.

1) The snow simulations with the SNTHERM model

SNTHERM is a snow model developed for seasonal snow on land. It has been modified for sea ice applications. The basal heat flux is a key variable controlling the temperature gradient, the water vapor flux, and thus impacting snow metamorphism. However, the authors mention page 5715, line 23: "Since we focus on the surface forcing, we neglect the basal (ocean) heat flux and sea-ice growth." The author need to justify this assumption. Microwave radiation penetrates in snow and ice, therefore, it is sensitive to the vertical structure of the snow and ice covers, and thus sensitive to both the surface and bottom forcing. Can the difference in simulated snow properties obtain with and without heat flux be quantified?

Page 5716, line 12: "Linear temperature profiles are assumed in sea ice and snow with the temperature at the snow/ice interface representing one third of the total temperature gradient from the sea-ice bottom to the snow surface. " The microwave radiation has frequency-dependent penetration depths. Therefore, are these assumptions about the temperature profile realistic? Does the temperature profile adjust itself during the very short 5-day period of spin up?

Snow scattering is a key radiative transfer process within the snowpack. The authors should compare to other studies the value F=0.12 considered to convert the optical grain diameter simulated by SNTHERM and the exponential correlation length needed as input in MEMLS. How dependent to F are the results presented in the manuscript? Which correlation length was used in the ice?

In figure 3, the simulated emissivities at 19 GHz appear very stable. I would expect that the increase in snow grain size lead to a decrease in brightness temperature at 19 GHz. Are the observed brightness temperatures also stable at 19 GHz? I recommend the authors to show for some locations the time series of simulated snow properties, as well as some profiles. Regarding figure 3, I am wondering whether December is too early in the Antarctic compared to June in the Arctic, because emissivities remain lower than 0.98 (not characteristic of a melting snow cover).

2) The rational about the hemispheric differences in simulated emissivities

The objective of the study is to quantify the differences in microwave emissivities in several regions of the Arctic and Antarctic due to different weather conditions. However, there is no discussion about the different weather conditions between the regions and hemispheres. Only some ideas are presented about melt/refreeze cycles, and timing of snow melt. Some discussions about the input forcing appear necessary. For instance, is there a correlation between emissivity variability and temperature variability, or wind speed?

The authors neglected precipitation (snow fall) in their study. I expect that snow depth varies significantly in the regions considered, especially in the Antarctic. Why was this meteorological variable neglected? Also, the authors conclude that their results can be used to refine snow depth retrievals, but snowfall was not considered. I would

TCD

7, C2938–C2945, 2014

recommend to conclude the study accordingly to the results presented, or to further detail how their results could be used considering the number of assumptions made.

Major comments The abstract requires a careful attention. Results expressed as % about the emissivity variations are not presented in the manuscript. Moreover, the manuscript does not provide path to assess sea ice concentration and snow depth retrieval algorithms.

Throughout the introduction, the references used in several sentences are not the best choices.

Throughout the manuscript, adjectives used must be supported by values, e.g.:

p. 5718 , l. 8. Quantify "pronounced", and if possible assess with observations;

p. 5718 , l. 29. Quantify "pronounced differences";

p. 5719 , l. 29. Quantify "much more sensitive";

p. 5720, l. 20. Quantify "significantly lower";

p. 5720, l. 22. Quantify "The rate of decrease";

p. 5722, l.1. Quantify "could probably smooth the emissivity variability".

P. 5715 , l. 2. The snowpack is initialized on January 1st and July 1st in the Arctic and Antarctic, respectively. At these dates, the winter is well established, could you discuss these dates and their implications?

P. 5715 , l. 19-20. SSM/I type sensors do not have the same frequencies as AMSR-E (e.g. 85 GHz vs. 89 GHz). They also do not have the same incidence angles. SSMIS observations are made with an incidence angle of 53.1 degrees, closer to the Brewster angle for the air/snow interface than the value you considered in your study (50 degrees). Since the effect of incidence angle can be large, either some rationales, or new simulations with the observation characteristics of one sensor are needed.

P. 5715 , l. 24-28. These initializations values are not supported by references, are they realistic?

P. 5715 , l. 28. According to the documentation for MEMLS, Version 3: “The applicable salinity range covers 0 to 0.1 ppt, but may still work up to 1 ppt. ” The authors used salinity up to 12 ppt. This seems to be outside the range of validity for the dielectric constant. Can the parameterization still be used safely with such high salinity?

P. 5716, l.27. What is the motivation of using the NASA Team tie points as the reference to calibrate the factor F? If the NASA Team tie points were derived during a specific month, was the same month used in your study to derive F? Would the calibration be more accurate if performed over an entire winter using SSM/I observed brightness temperatures? The value of F=0.12 could be put in context with the other studies, such as Langlois et al. (cited elsewhere by the authors), Montpetit et al. (2013), Brucker et al., (2010), Durand et al. (2008), Wiesman et al. (2000), and likely others:

B. Montpetit; A. Royer; A. Roy; A. Langlois; C. Derksen, "Snow Microwave Emission Modeling of Ice Lenses Within a Snowpack Using the Microwave Emission Model for Layered Snowpacks," Geoscience and Remote Sensing, IEEE Transactions on, vol. 51, no.9, pp. 4705-4717, 2013

L. Brucker , A. Royer , G. Picard , A. Langlois and M. Fily, "Hourly simulations of seasonal snow microwave brightness temperature using coupled snow evolution-emission models in Quebec, Canada". Remote Sens. Environ., vol. 115, pp. 1966-1977, 2011

M. Durand , E. Kim and S. Margulis, "Quantifying uncertainty in modeling snow microwave radiance for a mountain snowpack at the point-scale, including stratigraphic effects”. IEEE Trans. Geosci. Remote Sens., vol. 46, no. 6, pp. 1753-1767, 2008

A. Wiesmann , C. Fierz and C. Mätzler, "Simulation of microwave emission from physically modeled snowpacks" Ann. Glaciol., vol. 31, no. 1, pp. 397-401, 2000

P. 5718 , l. 1. Even though there should be four time more simulated points than observed points, I do not see a good alignment of the simulated and observed brightness temperatures. The authors must support their statements with values.

P. 5718 , l. 16 and p. 5719, l. 11+. Simulated emissivities over the Antarctic sea ice do not show intense melt. Are emissivities higher one to two months later, in order to compare similar (high) emissivity values as in the Arctic. I do not understand the authors points, can it be phrased differently?

P. 5721 , l. 17. I do not understand how algorithm accuracies can be determined since the simulated brightness temperatures do not span the full range of observations. I believe the accuracy of the simulated brightness temperatures first needs to be quantified. Would the method be more suitable for precision assessment?

Minor comments Abstract. The term atmospheric forcing is at this point unclear, and one could wonder if an atmospheric microwave radiative transfer model was used.

Abstract. "Small but significant emissivity trends ...", in which direction is the trend?

P. 5712 , l. 21. since more than → for more than

P. 5712 , l. 22. None of these references support the statement.

P. 5713 , l. 2. I think that none of the algorithms used in these references consider frequencies below 18 GHz. In addition, I recommend to support your statements with peer-reviewed articles rather than reports.

P. 5713 , l. 4. The study of Hass (2001) uses active microwave data, whereas the entire study deals with passive microwave data. Is this the most suitable reference? Does it support your statement about sea ice age?

P. 5713 , l. 13. I do not think that the reference to Markus et al., 2006 is appropriated here.

P. 5713 , l. 16. Explain the term anomalous emissivities.

P. 5713 , l. 28. Is snow stratification different than layering?

P. 5714 , l. 12. What does typical atmospheric forcing mean?

P. 5715 , l. 19. A reference is needed.

P. 5716 , l. 15. In the Arctic several locations are not in regions where first-year ice is the main ice type.

P. 5717 , l. 2. The use of correlation length in microwave remote sensing goes back before 2012, use appropriate references.

P. 5717 , l. 6. How were these ratios selected?

P. 5717 , l. 19. Why at high concentration only? Sea ice concentration algorithms tend to work accurately and provide similar results at high ice concentrations. How was the threshold of 90% defined?

P. 5718 , l. 8. How were the emissivities calculated?

P. 5718 , l. 22. Rephrase this sentence.

P. 5720 , l. 3. Is this about the standard simulation? If so, it may be better to address this paragraph before discussing table 1.

P. 5720 , l. 20. How was penetration depth calculated?

P. 5721 , l. 19. Provide some examples to illustrate “manyfold”.

P. 5721 , l. 25. Do these algorithms use monthly tie points?

P. 5721 , l. 26. I think tie points are expressed in brightness temperature, and not in term of emissivity.

P. 5722 , l. 1. Which atmospheric effects?

P. 5722 , l. 2. I do not think this sentence is correct. Please, provide more description about the weather filters.

TCD
7, C2938–C2945, 2014

Interactive Comment

P. 5722 , l. 14. What are raw brightness temperatures?

Figure 1: Why was the Sea of Okhotsk not considered? While describing your results, could you emphasize the similarities/differences between the regions mostly covered by first-year vs. multi-year ice?

Figure 2: Which line corresponds to the NASA Team and Bootstrap algorithms? Which line correspond to summer and winter? Which tie point is first year ice?

Figure 4: I am surprised that some emissivities (e.g. LS and ES sectors at 19 GHz) have lower standard deviations at horizontal polarization than at vertical polarization. Can this be discussed?

Interactive comment on The Cryosphere Discuss., 7, 5711, 2013.