Reviews for: "The sensitivity of satellite microwave observations to liquid water in the Antarctic snowpack" by Picard et al., 2022

1 Description

In the article "The sensitivity of satellite microwave observations to liquid water in the Antarctic snowpack", the authors discuss and provide new insights into the theoretical basis of passive microwave liquid water detection algorithms over shelf ice.
First, the authors compare different permittivity models for wet snow and discuss their applicability for microwave emission modeling. Further, based on a Monte-Carlo approach, the authors retrieve the dry snowpack properties at 8 different sites on the Antarctic shelf ice and then simulate the sensitivity of different microwave channels to the appearance of liquid water in snow. The results are compared to satellite observations.
Several experiments are performed analyzing e.g., the sensitivity of microwaves to liquid water layers in snow at different depths and with different magnitudes.
In the last part of the manuscript, the authors give recommendations how their results could be used to improve existing or develop more advanced passive microwave melt detection algorithms and for an improved evaluation of firn models.
In general, this article provides interesting and new results which are of high scientific relevance and can help to improve existing or develop more advanced passive microwave melt detection algorithms. The article is clearly structured and the individual sections and experiments are well motivated. However, some of the figures need to be improved. A few statements are not clear or hard to reproduce from the figures. Additional clarification and information is sometimes required. Therefore, I recommend the article to be published after minor revisions.

2 General Comments

I have two general comments. 1) the manuscript would benefit from improving some of the figures. Some of the lines in the figures are hardly visible or distinguishable from other lines. I recommend that the authors check all figures again and increase the line width when appropriate or choose different colors for different lines. Further details are provided in the specific comments.
2) Overall, the modeling setup is well described with the right amount of detail. This method is a very nice approach highlighting the potential strength of snow emission models like smrt. I think it could be very useful for other scientists who are interested in using the model or the method if the authors would provide a more detailed description of the experiment setup in the supplement (e.g., adding the initial range of all input parameters, differences of the different sites, similarity of the ≈100 output profiles,...)

3 specific comments

L10: *and on the site* → Here I would specify what controls the layer depth (e.g., density, grain-shape, terrain properties)

L18: *climatic indicator* → climate indicator

L47 *when it reaches* → It reads like you mean a threshold for the surface but instead it is a threshold for the brightness temperature

L55-56: What about snow redistribution? I believe this could be more important than snowfall events and also much harder to predict/simulate

L92: *...presence of aquifer Montgomery...* → presence of aquifer, Montgomery

L95: it would be helpful if the authors would provide a map with the different study sites marked

L100: *The Soil Moisture...* → Observations from the Soil Moisture...

L106: Do you average the observations from the incident angle range?

Table 1 Caption: *AWS names are from (Jakobs et al., 2020)* → AWS names are from Jakobs et al. (2020)

Table 1 Caption: You need to specify which temperature is used (i.e., 2-m or skin-temperature?)

Table 1 Caption: *melt days are from AMSR2 19 GHz H-pol channel.* → I assume a published algorithm is used? (Then the reference is missing) Or is it based on the 20K threshold? (Then why did you chose this channel?)

L128-130: Earlier, you wrote that you use SMOS data from 50 to 55°. I was wondering, do you then also simulate the SMRT output for this incident angle range?

L134: *microwave* → microwaves

L175ff: Based on Figure 1, I cannot reproduce the authors argumentation that the MEMLS V3 model behaves close to the coated spheres model in the high water regime. Aren't the differences between these two model much larger than e.g., to the Colbeck 1980 (Pendular) model? In addition, it is really hard to distinguish the different models in the low water regime, which would be the most important part in case one wants to know the minimum water content detectable by microwave observations. I suggest adding a zoomed-in version of Figure 1.
Also, would in not make sense to, in addition to the Hallikainen 1986, use a model which predicts higher changes in permittivity (e.g., the Colbeck 1980 (Pendular) model), to have an idea of the possible ranges of the sensitivity to liquid water?

L192: Sensitivity of what? The brightness temperatures? I would've assumed that (small-scale) surface roughness variations can have an impact at least at H-Pol. Maybe the authors could provide a rough number for the impact of surface roughness variations at the snowpack brightness temperatures

L201: I assume that the 2-m air temperatures is used?

L204ff: I wonder if the authors would have included L-Band data, how would the optimal snowpack change? I guess the other frequencies would then look worse?

L2010: *400–910* → wrong symbol. Also later in the document, – and - are sometimes mixed up. I suggest the authors to carefully check throughout the document

L218: $d/2–2d$ → This reads d/2 minus 2d but I guess you mean d/2 to 2d

L221: The citation should be earlier

L230: I was wondering, since you compare point simulations with large-scale satellite observations, have you considered the effect of slopes in the footprint? I guess there might as well be slopes on the Antarctic shelf-ice within one satellite pixel. If they have an impact on the observations, your retrieval method would compensate with changed snow properties and thus the snowpack might not be representative for the specific satellite pixel.

L250: *distinctively low SMB (Table 1)*

L249-250: According figure 3, the snowpack at Amery has the highest correlation length (i.e., largest grains) of all sites at 8 m (which is around the e-folding depth at 6 GHz, I guess). This could (partly) explain the relative low brightness temperatures observed at this site.

L262-264: Here, you discuss the dependence of H-Pol on the ice layer density. However, I miss the relation/implication of this for the brightness temperatures at the different site. Since you use the ice layer density as an variable in you model input, it would be nice to also discuss how (qualitatively) this variable is different at the different sites and how this relates to the H-pol observations.

L272: For clarification, is the RMSE calculated from the set of 100 snowpacks for each site?

L275-282: I was wondering, did you use the same scaling factor for all sites? From figure 2, it looks like you sometimes over- and sometimes underestimate the observations at L-band, so wouldn't a variable scaling factor make more sense?

L295-299: For Amery, larsen C and Larsen B, the correlation length at 8 m is much higher than at 20 m. I have difficulties to find a physical explanation for that. Could this is an artifact of the choice of the depth of the tie points. Since the e-folding depth at 6 GHz is well below 20 m depth (based on the examples you provided for Baudouin), the contribution of this tie point to the model result is very low. If that's the case, this should be shortly discussed in this paragraph.

Figure 3: The color for 0 cm is hard to see. I would recommend to increase opaqueness here

Figure 3, caption: *grain size* → correlation length

L310-311: here you write that the surface density is $220 Kgm^{-3}$. However, according to figure 3, this value varies between 200 and 400.

L314: *imaginary part of the water permittivity is extremely high (Fig. 1)* $\rightarrow$ I'm not able to see from figure 1 due to the large range of the permittivity.

L316-319: What does *pixel.day* mean? Which AMSR-2 observations are you using? Only for the Baudouin grid cell or for the whole Antarctic shelf region?

L320-324: I have difficulties reproducing the numbers given in this paragraph. Are you still describing the results shown in figure 4 at 19 GHz? E.g., none of the H-pol values in figure 4 (a-c) reach 260 K and ΔTb at 19 GHz, H-pol seems to be less than 60 K

Figure 5: This figure needs to be revised since different lines are very hard to distinguish. One option could be to show less frequencies and then show an "errorbar" plot with the σ or 2σ spread as shaded contours.
Figure 5: There seems to be a step at several frequencies e.g., at 19 Ghz H-pol between 12.5 Kgm^{-2} and 15 Kgm^{-2} total liquid water. I was wondering what is the possible reason for that?

L345-350: Another conclusion would be use V-pol over H-pol since its much less affected by surface processes while having sensitivity to liquid water content

Figure 6: Same as figure 5, the contrast of the curve for the highest standard deviation is too low

L373-374: Mention that the figure only covers the 1st regime

L376-377: I don't see how the Zwally algorithm mitigates the problem of high (winter) brightness temperatures, since they are using a fixed threshold (30 K) which would not be reached for e.g., Halvfarryggen

Section 4.2.4: While it is interesting to discuss the shortcoming of the H86 formulation, it would be also of value to compare the results of the (reasonable) selection of models shown in figure 1 to get an idea of the spread of the solutions.

Figure 8: decrease the Y-extend

Figure 9: The lines for 1 and 2 cm are hardly visible. Why is in figure 9b only H-Pol shown?

L405-406: *We find that varying the thickness of the wet snow layer has little influence on the results if the total amount of liquid water is fixed ~~when varying the thickness~~*

L409: *for small amounts of water*

L411: Given the statement "many authors" I at least expect some references

Figure 10: It would be nice to add the dry snowpack brightness temperatures (e.g., as triangles at the right end of the different lines)

L403: *(decreasing in exponential shape)*

L434-435: *especially at 37 GHz, due to the accumulation of fresh, fine-grained, snow over the summer, metamorphosed, coarse-grained snow.* I have problems understanding the last part of this sentence.

Figure 11: (Caption) *Time-series of AMSR2 brightness temperature at Wilkins at V-pol.* The figure would benefit from adding the ERA5 2m air temperature so one can easily assess when melting starts/ends

L455: *with the depth of the wet snow layer*

L455: *and weaker than at the*

Figure 12: Add polarization shown here.

L456: *because the dry snowpack is already close to a black body ~~even when dry~~*

L459: *features* → feature

L461-463: To me, it looks like at 6 and 10 GHz, the results at V-Pol are well comparable and only at the higher frequencies, they strongly differ

L475: *brightness temperatures*

L487: *characterized by ~~a~~ very low*

L497:500: This sentence is quite lengthy and written in a somewhat colloquial language. I would recommend to rephrase this paragraph and maybe split it into several sentences

L504: *triggered* → triggering

Figure 15: Use solid lines for V-Pol threshold

L510: *overall lower sensitivity of L-band compared*

L511-512: I'm not sure I understand what you are referring to here. From Figure 16, it does not look like the signal at V-pol becomes saturated at $30\,kgm^{-2}$, also I do not see a maximum at H-pol at 14 but rather at $8\,kgm^{-2}$. Please explain what exactly you are describing here

L521-522: Earlier you stated that a threshold of 10 K brightness temperature differences would still be way above the noise level. Refining this statement (so what would be the minimum acceptable threshold) would help the interpretation of the modeling results

L527: *(213 K at H-pol)*

Figure 17: The different behavior of V-pol and H-pol needs to be addressed in the text. Why is there a maximum at H-pol when the wet layer is buried just below a snow layer?

L563-563: Since the snowpack evolution during the wet season is not (or only partly in experiment 3) covered by your simulation setup, this statement is not really a finding of this study but more a general problem.

L571: I don't think, using V-pol would "avoid" the problem of not detecting wet snow with high water content. It would rather mitigate the problem towards slightly higher water contents

L584: *particularly on the ice shelves*