The authors would like to thank the editor and the referee's comments on our manuscript. Following the comments, we make the following replies and corresponding revisions to the manuscript. Each item of the original comments from the referee is in *red italic*, followed by our reply. Moreover, in the marked version of the revised manuscript, the revisions are highlighted with 'REV3'.

REVIEW 3

Summary

Flooding and slush over Antarctic sea ice have been a major reason to cause errors and uncertainty for snow depth and/or sea ice thickness retrieval from Satellite remote sensing for southern oceans. The purpose of the paper hopes to improve snow depth and ice thickness retrieval from passive microwave (1.4 GHz) brightness temperature via satellites such as SMOS and SMAP, by improving the existing RADIS-L v1.0 to v1.1, by introducing brine-wetted snow and lush layers over Antarctic sea ice. The modeling results are validated based on three types of datasets: 4 buoys, some ASPeCT data, and OIB data. Very surprisingly, the ASPeCT data actually gave the best results, only 1 out of 4 buoys gave reasonable results, validation from OIB seems not as good as stated. They attribute the biases between modeled and observed Tbs mostly to sub-grid scale ice surface variability and snow stratigraphy. One of the results is that the adding of a slush layer into the model decrease the modeled Tbs. This is obviously true. Anyway, I agree this is very important topic of study, but the paper needs to prove that the v1.1 is indeed effectively modeling the wet and slush layers for Tbs, and in my opinion, it is not the case in its current format.

General comments

The paper in somehow confuses me on the brine-wetted and slush layers. I hope they can make corrections throughout the paper, to brine-wetted snow layer and slush layer, since slush layer is not snow layer any more. Slush layer is a mixed snow, ice, and water, more close to water in my opinion. And the slush layer does not exist too long (not sure how long, may be a few hours?) before refreezing to become the ice (or snow-ice).

Reply: we agree with the referee's suggestion to clearly distinguish the brine-wetted snow and slush. For this study, the highest temporal scale is 1-day (Table 1), especially the comparison of modeled Tbs with satellite measurements. Therefore, we consider the slush due to inundation an intermediate stage, and we mainly focus on snow-ice, which may also contain a significant portion of brine (i.e. liquid content). Necessary revisions are made to the manuscript to be more clear about several concepts: slush, brine-wetted snow, and snow-ice.

Also need to make sure the improvement of the modeling from v1.0 to 1.1 is only adding the "slush layer", since the v1.0 already has the brine-wetted snow layer. Right?

Reply: the v1.0 of RADIS-L does not include slush or snow-ice, since the model was developed for the Arctic region, where snow-ice is not a dominant feature. The new version of RADIS-L (i.e., version 1.1) adds the support of snow-ice and its effect on Tbs.

The paper in somehow confuses me about active and passive microwave sensor. From the title, I thought it uses radar data, but clearly the paper did not use radar data. see my comment on L166, I suggest to change "L-band" to either "passive" or "1.4 GHz". If this is agreed, you should do it throughout the entire paper.

Reply: we confirm that the L-band passive microwave remote sensing is the focus of the study. We also use ALOS PALSAR (i.e., L-band SAR) images (HH-polarization) as auxiliary data to the result analysis.

As suggested by the referee, we slightly change the title to: "Quantifying the Influence of Snow over Sea Ice Morphology on L-Band Passive Microwave Satellite Observations in the Southern Ocean".

Specifical comments

Line 7, change to "brine-wetted snow and slush layers"

Reply: revised accordingly.

L35, should be "zero ice freeboard"

Reply: revised accordingly.

L127, do you mean "vertical snow salinity"?

Reply: corrected.

L166, "this fully polarized sensor…." is wrong. I do not think it is fully polarized. Since SMOS is passive and it is either H or V, not HH, HV, VH, or HH. Also my comment about the title. L-band is mostly for active sensor, for passive sensor, we usually use frequency.

Reply: we acknowledge this incorrect statement. This sentence is revised as: “This sensor measures the Earth's emitted radiation at 1.4 GHz at both horizontal and vertical polarization and multiple incidence angles”.

L199, should be “horizontal transmit and receive polarization”, not the other way as here. Remember if it is active, it transmits first, then receives.

Reply: revised according to the referee’s comment.

L204, I do not know why you want to use 35 degree, not the real incidence angle for each pixel? I think SNAP can do it by directly use the real incidence angle of each pixel.

Reply: we choose to correct for the incidence angle the PALSAR image to 35-deg, in order to get a more uniform image. This image is then used for visual inspection for key characteristics such as sea ice concentration. The use of the un-corrected, original image (from SNAP) is possible, but not necessary.

Section 3.1 SNOWPACK. Is this RADIS-L v1.0? if yes, should you change the section to it? If not, what it is the difference between them?

Reply: we would like to clarify that SNOWPACK and RADIS-L are two different models. SNOWPACK (Wever et al., 2020) is provided by third-party developers, and it is a prognostic snow-sea ice model for snow stratigraphy and forced with atmospheric forcings. We use SNOWPACK to simulate possible stratigraphy features and layering of the snow pack over sea ice.

On the other hand, RADIS-L is developed by the authors, and it is a radiation model for L-band radiometers. The improvement of RADIS-L (from v1.0 to v1.1) mainly involves the inclusion of slush/snow-ice.

L243, should “X- and L-band” be the relevant frequency of them?

Reply: we confirm that the two bands of L-band and X-band were the original focus of the RADIS-L model for passive radiometers.

L282, equation 4, the surface Temperature Tsurf, I believe it is more controlled by the air temperature, rather than as provided by the equation.

Reply: we would like to clarify that we do NOT mean that Tsurf is controlled by the equation. Instead, under quasi-equilibrium status for themodynamics, the temperature profile from the surface (i.e., Tsurf) to the internal (i.e., snow-ice interface) follows the relationship in Eqs. 4.

L305, "Therefore, we treat the snow slush …..", please check this sentence and make sure it is the right statement? I am little confused.

Reply: according to the suggestion by the referee, we revise the sentence as: "Therefore, we simply treat the slush as newly formed sea ice, with a variable and high volume of brine".

Section 3.3, wondering why you want to use 30-50km, not use the 9km for SMAP and 12.5 km for SMOS?

Reply: we would like to clarify that the effective resolution of both SMOS and SMAP is in the range of 35-50 km. The enhanced resolutions of 9 km and 12.5 km are achieved using nearest-neighbor algorithm interpolation. This method reduces geophysical and temporal variability and brightness temperature uncertainties by aggregating single-angle brightness temperature measurements for each grid point. The process of improving the resolution from coarse (e.g., 35-50 km) to fine (e.g., 9 or 12.5 km) using interpolation, along with the characteristics of L-band data, introduces limitations. These factors can affect the accuracy of the fine resolution data and result in nearby data points being correlated rather than truly independent measurements. Therefore, we choose the coarse resolution to adhere to the band properties and minimize influences from nearby grid cells. For more details, please refer to the L3B brightness temperature (Kaleschke et al., 2012) data guide: https://www.cen.uni-hamburg.de/en/icdc/data/cryosphere/docs-cryo/documentation-smos-tbs.pdf

L353-354, when there was heavy snowfall, it will general flooding, slush, then slush freezing to snow-ice; this entire process should increase ice thickness not decrease ice thickness. I do not understand your statement here.

Reply: we hereby correct the previous wrong statement. And as mentioned by the referee, the heavy snowfall could potentially increase the ice growth. And according to buoy measurement, the sentence is revised into "Snow and ice remain stationary, followed by heavy snowfall (over 35 cm snow depth) starting from 11-Sep-2013 in PS81/506 (Fig. 2b)."

L402-406, this is hard to align the satellite pixel with the buoy location, given the buoy collecting continuous data while the satellite only gets one data per day. Also the buoy (ice floe) is moving. Not sure how you process your buoy data to match the satellite data? You average all data for the day or you only use the data collected when the satellite passing? Maybe averaging these data a few hours before and after the satellite passing?

Reply: we would like to clarify that all data are aligned to a daily basis. First, the Tb product (SMOS) has relatively coarse spatial resolution, although more than 1 satellite pass is possible within a day. Therefore the daily average (of several passes) is computed for Tb. Second, we compute the daily mean location of the buoy and find the cell of the Tb product that contains the buoy's (mean) location. Due to the relative coarse resolution of Tb, we do not encounter problems due to the buoy's drift (i.e., no case with the buoy covering a large number of cells within a day).

L407-408, this is a good statement but it is only true for 1 out of 4 buoys. See figure 4 (a). this makes me to wonder something is wrong. This would be due to my comment on the line 402-406, or other reasons we do not know. I believe you need far more example than these 4 and may be also expand to other sections of the Antarctic, currently 3 in Weddell sea. The one not in Weddell sea is a fast ice which could be very different from the pack ice situation. And I am not sure if the flooding and slush layer would happen at all for fast ice case. I take it back, from figure 2d, it shows negative freeboard. But please check if this is indeed.

Reply: we would like to clarify that as referee 3 mentioned, the negative ice freeboard was observed over the ZS-2010 buoy, and referee 1 also mentioned that the negative ice freeboard occurred near the iceberg and substantial snow accumulation directly contributed to the snow-ice formation. While the simulation accuracy of Tb might be limited due to the presence of the iceberg, any changes in Tb are likely attributed to variations in snow since the iceberg remains constant. Secondly, We also want to emphasize that the inclusion of brine-wetted snow improves the simulation of Tb. Significant improvements were observed for buoys 1, 2, and 4. Although the improvement for buoy 3 is small in terms of the statistical fitting and R^2, the overestimation of Tb caused by snow did witness a reduction. We believe the improved model better captures the changes in Tb due to sudden daily snow accumulation as seen in buoys 1, 3, and 4, though further progress is needed for systems with existing brine-wetted snow. Lastly, we want to note that the buoy-based data used in this paper, which contain both snow and ice measurements, already represent the most comprehensive dataset available to date. We would be eager to validate our findings with additional data if new data becomes available.

L415-416. It is true "Tbs reduction from 243.8k ….to 226.1K", but it also involves with a few increases and decreases in between. Can you explain that.

Reply: we would like to clarify that the variability of modeled Tb is largely due to that of the surface temperature as measured by buoys (in Fig. 4). The buoy's Tsurf contains large temporal changes (i.e., decreases and increases) due to atmospheric activities. Since we assume thermal quasi-equilibrium, this translates to the large changes of internal temperature as well (and Tbs). We argue that although this assumption leads to artificially large variability of internal temperatures, the long-term (beyond several days) change of Tbs in deed contains the physical signal of atmospheric signature of cooling and warming.

L458, if ZS-2010 buoy was on the fastice, why you talk about trajectory? Please explain here.

Reply: we have revised the sentence to: "... we conduct Tbs simulations at the location of the ZS-2010 buoy".

L462-467 and figure 4d, your statements seem be suspicious. I see v1.0 and 1.1 have the same patterns but different magnitudes. Compare with satellite Tbs, however, there are a few cases, with inverse change. R2 of 0.35 does not mean a good relation, in my opinion.

Reply: As mentioned earlier, the presence of a nearby iceberg and proximity to the coast (Li et al., 2023, see their Figure 1) affect the Tb measurements from SMOS. The emissivity from the iceberg and ice shelf within 30-50 km influences these measurements (see the buoy location in Fig.1, overlaid by Tb on September 1, 2010). Consequently, it is a unique study case which is different from the ones on drifting buoys in the Weddell Sea. Nonetheless, most of the Tb variability due to significant snow accumulation is captured, with the exception of the abrupt increases and decreases around October 25th. We have confirmed that this is due to a sudden change in the sea ice concentration (light blue line in Fig.2).

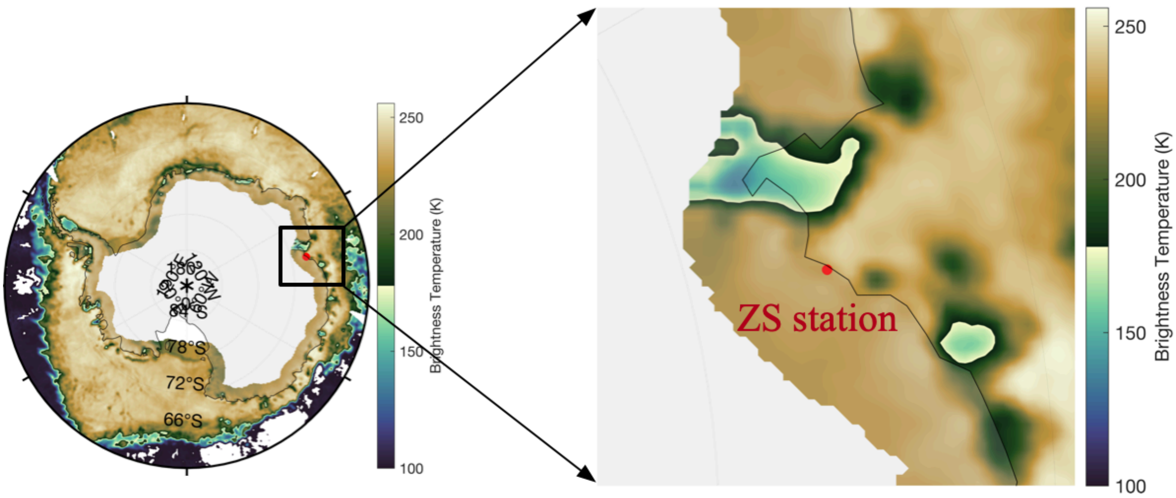


Fig.1 ZS buoy location overlaid by Tb on September 1, 2010

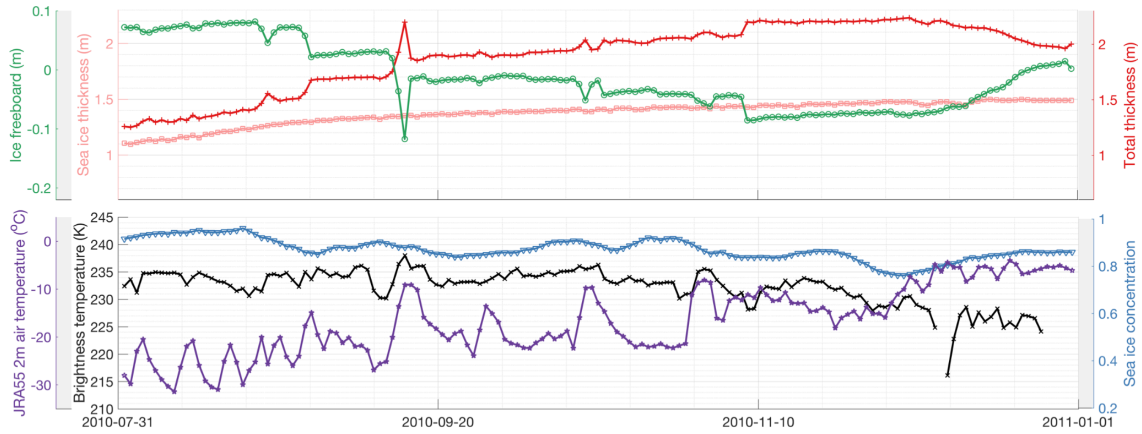


Fig.2 JRA55 air temperature, sea ice concentration and brightness temperature along ZS-2010 buoy.

Section 4.2.2, validation with ASPeCT shows better results than the buoys. In line 488-489, please explain to me what you mean small-scale? What kind of parameters in small-scale would increase your modeling results? In term of SAR imagery, it will not give Tbs.

Reply: We would like to clarify that the small-scale features in lines 488-489 pertain to the context of airborne (OIB) validation. In this context, one of the key parameters in the radiation model—sea ice concentration—should be provided at the highest possible resolution. The ASI sea ice concentration retrieved from passive microwave data is currently the highest available resolution at 6.25 km. However, as noted in Section 5.1, SAR images reveal small-scale leads, which are not captured by the ASI sea ice concentration due to its coarser resolution. This inability to differentiate sub-grid scale (small-scale) variabilities is a primary reason for the overestimation of Tbs. Future work using SAR-based sea ice concentration, which can provide sea ice concentration data at resolutions finer than 1 km, could help reduce this bias and improve the accuracy of the radiation model.

L492-493, not sure what you mean "This analysis is …. Near Wilkes Land"?

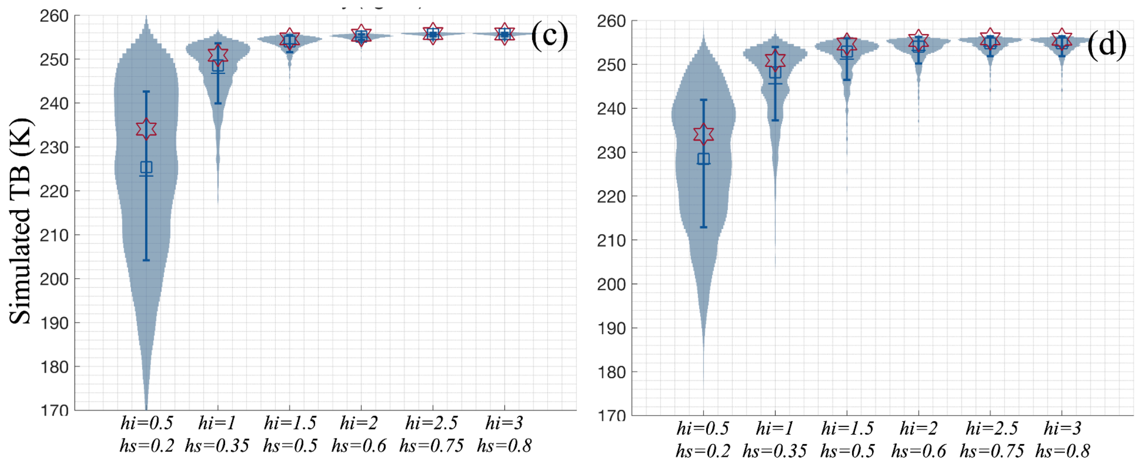
Reply: in order to be more clear, we revise the sentence to: "This analysis is based on observations from five snow and ice transects located near Wilkes Land". We have also added the reference to Fig. 1 in which the locations of the campaigns are shown (see the next comment).

L495-497, not sure where you mentioned ice stations 2,3,4,6,7? They are in the figure 1?

Reply: the locations of the SIPEX II campaign (the ice stations) are shown in Fig. 1. Necessary reference is added.

L565-569, mentioned about 2.5m ice and 0.5 m snow for the experiment, I am not sure if this snow depth would cause flooding and slush over ice? It may be but I am not sure. but figure 10 shows 0.2m snow with 2m ice, 0.3m snow with 3m ice, etc.., I am pretty sure these cases would not cause flooding and slush…; please explain. Also as you know and talked about that most of the Antarctic sea ice is thin ice. Your ice here are 2-6m, which is very unnormal. I would suggest to model ice thickness from 0.5 to 3m, the maximum.

Reply: We acknowledge that the combination of sea ice and snow thickness cannot trigger flooding under the assumption of hydrostatic condition. As suggested by the referee, we have revised the sensitivity tests in the updated version of the manuscript.. Specifically, we have adjusted Figure 10 to include sea ice thickness values of 0.5, 1, 1.5, 2, 2.5, and 3 meters, and correspondingly snow depths of 0.2, 0.35, 0.5, 0.6, 0.75, and 0.8 meters, so as to maintain negative freeboard. The sensitivities are updated below:



L657-658, "Given these considerations…", this statement is very interesting, since from your results, the ASPeCt data seems gave the best validate of simulation. Please explain.

Reply: As noted by Referee 1, ASPeCt data has significant uncertainty due to its reliance on visual judgments made by humans. Therefore, we emphasize the need for caution regarding these uncertainties associated with ship-based sea ice and snow measurements.

Conclusions section includes a lot of contents that are not really from this paper, many of them could be included in the Discussion section, some of others are very suspicious *statements. For example, line 642-643, I do not see where you discussed this and how you come to this: "The scenarios we have discussed …in various settings".*

Reply: we have revised the paper, to include the discussion in Sec. 6 (Conclusions) to Sec. 5 (Discussion). Also, as suggested by the referee, we refrain from claiming that "The scenarios we have discussed provide critical benchmarks for improving radiometer designs…". Careful examinations are also carried out to improve the soundness of the this part of the manuscript.

Figure 1. why you only show three layers, not the fourth layers that you claimed to improve in the v1.1? .To me this is for the v1.0, right?

Reply: we want to clarify that Fig. 1 is for v1.1, which is designed as a simple and standard Three-layers scheme: dry snow, brine-wetted snow, and sea ice. Four-layers scheme (dry snow, brine-wetted snow, slush (snow-ice), and sea ice) is further discussed in Sec. 5.2 to further improve our understanding, and this needs more (slush) information in the Four-layers scheme.

Figure 4, very similar pattern between v1.0 and 1.1, but really not that match with the observations, except for the case in (a), i.e., the buy 2016S31.

Reply: we want to clarify that from v1.0 to v1.1, the main difference is in areas where brine-wetted snow exists, resulting in a consistent pattern between the two versions but with generally lower Tbs in v1.1. Additionally, the model uses some bulk parameters, and the input datasets have potential representation related uncertainties. Therefore, we expect some discrepancies when compared to the coarse satellite footprint of SMOS.

Figure 8, I do not see "the differences between simulated and SMOS Tbs …", where are those Tbs in this figure?

Reply: all the pentagram symbols (in panel a, b, c, d, e) are colored according to the difference in Tb (simulated minus observed). Faint colors indicate smaller Tb differences.

Figure 10. I hope to see the same value range of 242-256 for all y-axis of a, b, c, d.

Reply: according to the suggestion, we have revised Fig. 10 to align the range of the y-axis of the panels.

Reference

Li, N., Lei, R., Heil, P., Cheng, B., Ding, M., Tian, Z., and Li, B.: Seasonal and interannual variability of the landfast ice mass balance between 2009 and 2018 in Prydz Bay, East Antarctica, The Cryosphere, 17, 917–937, https://doi.org/10.5194/tc-17-917-2023, 2023.

Kaleschke, L., Tian-Kunze, X., Maaß, N., Mäkynen, M., and Drusch, M.: Sea ice thickness retrieval from SMOS brightness temperatures during the Arctic freeze-up period, Geophysical Research Letters, 39, https://doi.org/10.1029/2012GL050916, 2012.