In black : **referee comments**

In blue : authors answers

— REFEREE 1 —

Review of “Uncertainty of Antarctic sea ice concentration using passive microwave retrievals in the marginal ice zone” by Marta Stentella et al.

The MS is describing the simulated sensitivity of the Bootstrap algorithm sea ice concentration estimates to snow-ice-ocean surface parameters simulated with SMRT for sea ice and PARMIO for open water in the MIZ - Antarctica. There are two cases with two first-year sea ice profiles (one in summer conditions and one in winter) and the simulated brightness temperatures are tuned by varying each of the snow-ice-ocean surface parameters (LWC, SGS…) until a match is made with observations. The default surface parameter values and range of variability are picked from the literature without specific reference.

The authors thank the referee for their careful reading of the manuscript. The comments raised several important points that we have addressed in the revised version, both in the analysis and in the written presentation.

Some things are unclear and needs to be specified, for example, judging from the plots, the sea ice concentration algorithm, which is used, is the frequency mode bootstrap, i.e. the part of the bootstrap algorithm normally used over open water. Is that correct?

RC1.1 Yes, we use the Bootstrap algorithm in frequency mode.

The primary aim of this study is to assess the order of magnitude of the impact that surface-property variability has on brightness temperatures in the marginal ice zone (MIZ), particularly at low sea-ice concentrations (SIC < 80%), where the ocean contributes strongly to the mixed-grid cell signal. Our focus is therefore on the low-concentration regime, where retrieval sensitivities differ significantly from those in consolidated pack ice. We recognise that this framing was not emphasised clearly enough in the manuscript. Since our aim is to study how surface-property variability propagates into retrieval uncertainty in this low-SIC regime, we use Bootstrap as a representative algorithm rather than comparing multiple algorithms, which we considered out of the scope of the study. This frame is clearly stated in the next version.

We revised the text to clarify our motivation:

“The area of interest is the low-concentration MIZ, whose boundaries are considered between 15 and 80% SIC (Strong and Rigor, 2013), and where the ocean contributes strongly to the mixed-grid cell signal, leading to a signature that differs substantially from that of the consolidated ice pack. Accordingly, we chose to work with the Bootstrap algorithm in frequency mode (BF algorithm) as it performs comparatively better in regions dominated by open water (Andersen et al., 2007; Ivanova et al., 2015)."

Further clarification is in the revised background and introduction sections of the manuscript (RC2.3).

The range over which the snow-ice-ocean parameter is varied has a large impact on the magnitude of the SIC variability. The handle on the parameter variability and the initial profile is very loose and therefore the magnitude of the SIC variability is very uncertain. I would suggest either to constrain the snow-ice-ocean parameter variability with models or observations or both.

RC1.2 Our approach is close to the reviewer's suggestion. More precisely, a key challenge in constraining these parameters more tightly is the limited availability of observational data providing statistically robust, circumpolar-scale measurements, particularly for the Antarctic MIZ, where snow properties, salinity profiles, and thin-ice characteristics remain sparsely and unevenly observed. Additionally, the microwave emission models restrict the representation of the medium, and matching measurements to meaningful parameters is often a challenge, for instance for the grain size.

Given these limitations, our approach has been to use parameter values informed by the available studies and by the knowledge within our team as initial, physically-reasonable, starting points before tuning them on the background observations and before assessing how variability in these values propagates into SIC uncertainties. For example: Soriot 2022 paper is about the Arctic region but it served as a starting point for the initial snowpack configuration and many snowpack parameters fall within the range presented in Massom 2001.

We generally chose relatively broad parameter ranges to capture the expected physical variability across the Antarctic region considered. As shown in Figures 2,3, the modeled brightness temperature variability is comparable to the observed variability, suggesting that the parameter ranges are sufficiently representative without being excessively broad. However, some degree of subjectivity is implied in the analysis, because of the current state of observations where data remains sparse. An example of the limitations imposed by observational availability is the value the model requires for the optical radius. Further discussion on the choice of the radius parameter is provided in the response to RC2.5.

We added a reference column in Table 1 (and in the new table; Table 2) to list the references for the parameters chosen as starting input for the forward modelling approach.

Different sea ice concentration algorithms have different sensitivities to snow-ice-ocean variability (e.g. Tonboe et al. 2022) and for other commonly used algorithms (e.g. NT, NT2, ASI…), the sensitivity response may be different. I would suggest either to include more algorithms or to make this a case study for the Bootstrap algorithm.

We have explicitly stated that this is a case study, together with an explanation of the choice of the Bootstrap algorithm (more details in answer RC1.1).

The atmosphere (in addition to windspeed) is somehow included in the simulations, but the SIC variability due to water vapor and cloud liquid water is not quantified. At 36GHz and at low concentrations this is non-negligible. Anyway, a description of what is going on with the atmosphere is needed.

RC1.3 In the revised manuscript, we have included the suggested sensitivity analysis. For this we have used ERA5 reanalysis data for varying atmospheric profiles across different circumpolar locations for the two seasons, and updated the text accordingly, including the contextualization within existing literature. To this end, we used the PyRTlib model that is connected with SMRT, allowing us to inject ERA5 profiles smoothly.

The revised manuscript includes the following additions:

Section 3.4 (Atmosphere simulation) describes both the clear-sky atmosphere used in the sensitivity analysis of snow-covered sea ice and wind speed over open ocean, and the PyRTlib-based approach used to assess the sensitivity to varying atmospheric profiles.

Section 3.7.4 (SIC sensitivity to the atmosphere) presents the corresponding results and discussion.

Figure 5 illustrates the TB variability over the ocean for the 108 atmospheric profiles considered.

Figure 10 shows the impact of atmospheric variability on the SIC retrieval.

One of the major components of the SIC uncertainty in the MIZ is the resampling uncertainty (Tonboe et al., 2016; Lavergne et al., 2019). You also use a resampled dataset and part of the uncertainty in those data are due to this resampling uncertainty and part due to geophysical noise (that you simulate). It is not clear if the observed TB variability has an influence on the snow-ice-ocean parameter range of variability in T1, but you are only characterizing part of the 'uncertainty' with the simulations. I think that you should mention that.

RC1.4 This limitation is clarified in the Discussion section. 'Finally, this analysis is restricted to address only a sensitivity analysis of the geophysical noise, represented by the variability of brightness temperatures around the open-ocean and 100% SIC tie points. We do not explicitly address additional sources of uncertainty related to spatial smearing, such as those introduced by the resampling of satellite swath data onto a fixed grid or by footprint mismatches between frequency channels, as described for example by Lavergne et al. (2019).' (Further details in the new background section in answer RC2.3).

I think that some references are missing from the reference list, some suggestions are given below.

RC1.5 During manuscript preparation, several references were removed but we agree that the suggested literature is highly relevant, and we have incorporated these references in the revised version of the manuscript. Further details are in answer RC2.2.

Specific comments:

I suggest to change the title, for example: “Sensitivity of the Bootstrap SIC to surface parameters in the Antarctic MIZ”

RC1.6 We have revised the title and changed it to: “Sensitivity of the Bootstrap sea ice concentration to surface parameters in the Antarctic marginal ice zone using passive microwave retrievals”.

P1, L3: delete “global”

RC1.7 Done.

P2, L40: Three surface types are assumed, first-year ice, multiyear ice and open water. New-ice and bare ice does not fit the ice-line. Please clarify.

RC1.8 In the revised version of the manuscript we made explicit this consideration about the signature of thin ice:

“As discussed by Comiso (1995) in the description of the Bootstrap algorithm in frequency mode, the radiometric signature of new and thin ice formed in the marginal ice zone typically falls below the OA line, occupying an intermediate region between the open-water and thick-ice signatures as it can be seen in all the new ice types modelled (Fig. 5, 6, 7) . This means that the presence of newly formed thin ice (thickness <0.3m) yields lower brightness temperatures associated which cause a lower estimate of SIC through any employed SIC retrieval algorithm (Tonboe et al., 2022) and the presence of thin ice disrupts the linear relationship for the SIC, yielding a nonlinear 100% SIC line.”

P3, L83: replace “pure ice” with “100% ice”, and in general ‘pure water’ with ‘open water’.

RC1.9 Done.

P3, L86: add after “properties”: “or the fraction of new-ice within the resolution cell.”

RC1.10 Done.

P4, L88: Is the water tie-point adjusted daily? Please clarify.

RC1.11 In the current version of the manuscript, both the water and ice tie points are shown with respect to a single-date observational background, which may lead to the ambiguity that this model parameterization aims to represent a daily tie-point varying within the season. In the revised version, we updated the figures to include observational backgrounds constructed from multiple dates. Specifically, for each season - the warm season (December to February) and the cold season (June to August) - dates are selected at 15-day intervals over the period 2012–2024, yielding 96 dates per season. For each date, the ascending orbit of the satellite passage is selected, providing brightness temperatures in vertical polarisation at 19 GHz and 37 GHz. This multi-date background allows us to demonstrate that the chosen circumpolar parameterization for both the cryospheric and ocean components is representative of the typical surface conditions observed across the season considered. The AMSR2 observations background serves, therefore, as a sanity check for the seasonal snowpack parametrization, rather than for a parametrization of the daily dynamic tie point, which is not addressed in this study.

We rephrased line 87-88 to: 'The Bootstrap algorithm, among others, adjusts the tie points and the 100\% SIC line on a daily basis to account for seasonal and meteorological variability to account for the variability that depends on season and the meteorological conditions (Comiso, 2013).'

And specified that we do not address the description of daily but seasonal tie points: "The resulting reference simulations define the seasonal circumpolar tie points T_{SI} and T_{OO} in Eq. 1, representative of mean seasonal conditions rather than the precise daily condition."

P4, L91: why do you have empty sections?

RC1.12 The appearance of consecutive headings is due to the nested structure of the document, conforming to the Copernicus template.

P4, L114: It is unclear if there is a relationship between salinity, temperature and brine volume? I think that there should be.

RC1.13 In the current version of the manuscript, salinity, temperature, and brine volume fraction are varied independently in the sensitivity analysis. This choice was motivated by two reasons: first, to isolate the specific contribution of each parameter to the passive microwave signature; and second, because the empirical relationship between brine volume fraction and temperature (Leppäranta & Manninen, 1988) -which uses coefficient from Cox and Weeks (1983)- introduces its own uncertainty . However, following the reviewer's suggestion, and given the particular sensitivity of the passive microwave signature to the brine volume distribution, especially in the top layer of thin ice, we included, in the revised manuscript, a dedicated sensitivity analysis for each of the three parameters (salinity, temperature, and brine volume fraction), fixing the remaining two in each case. This is described in section 3.7.2, together with Table 2 showing the values assumed by the physical parameters and the relative references.

P5, L118: Please explain why you have a microstructure model with hard spheres and then you use the IBA for computing the scattering. Is that consistent?

RC1.14 It is consistent, as demonstrated in Picard et al. 2022, IBA can be combined with any microstructure provided it has an explicit autocorrelation function (Matzler and Wiesman, 1999), including the sticky hard spheres for which Löwe and Picard 2015 derived the autocorrelation function. Furthermore, Picard et al. 2018 demonstrates close equivalence between IBA and DMRT when using the SHS microstructure.

However in the revised version of the manuscript we included results obtained with the SYMSCE model (Torquato et al. 2021).This is motivated by the inclusion of the snow ice layer analysis, which can feature densities approaching the transition regime (~468 kg m^{-3}) where IBA becomes inaccurate. Employing the SymSCE model, the scattering coefficient remains continuous across the full density range. This leads to a small change in the output results but it does not affect the conclusion.

In the revised manuscript: "The calculation of the scattering and absorption coefficients is performed using the symmetrized strong contrast expansion (SymSCE) theory (Torquato et al., 2021) that features a continuous scattering coefficient across the full density range, also at intermediate densities around 468 Kgm−3 (Picard et al., 2022)"

P5, L124: why this set-up and why 13PSU and 0.05m? please provide some references or explanation.

RC1.15 We added the references (Table 2, RC1.13), used at the initial step of the forward model approach, with the limitation outlined in answer RC1.2; for the thin ice, a new parameterization is introduced in the revised manuscript to include three layers and have a more realistic representation of the temperature gradient as in Sect. 3.2 of the revised manuscript.

P5, L135: What other atmospheric contributions other than wind?

RC1.16 In the revised version of the manuscript we included ERA5 atmospheric profiles in the model that prescribe vertical profiles of pressure, air temperature, water vapor (specific and relative humidity), cloud liquid and ice water and ozone. This is described in the sensitivity analysis conducted over different atmospheric profiles as in answer RC1.3.

P5, L143: This is unclear… you only include datapoints with SIC >15%? And what do you use it for?

RC1.17 We corrected the mask selection and updated its description in the manuscript: "We applied a mask to the dataset to select the sea ice area and a limited portion of adjacent open ocean. The mask was created using the sea ice concentration (>0%) corresponding to the date under analysis for the warm and cold seasons, and extending it for 100 Km"

T1: please provide some specific references to these values. I guess that they are static when they don't have a min and max?

RC1.18 Yes, the values are static when the range is not specifically listed, this is stated more clearly in the revised manuscript "The parameter range is sampled using 21 evenly spaced values, corresponding to a uniform increment across the full range. If the minimum and maximum values are not provided, the parameter is kept constant." As noted in the response to RC1.2, we provided additional references within the constraints outlined above.

P7, L157: Several of these parameters are closely correlated, e.g. brine volume and temperature, they will never vary independently. Please explain.

RC1.19 The objective of this work is to quantify each parameters' contribution to the SIC uncertainty independently. As noted by the referee the brine volume fraction plays a particularly relevant role in the PM signature (Kwok et al 2006) for nilas, we included therefore a sensitivity analysis as in RC1.13.

F1: P should be half way between 'O' and the intersect with the ice line if 'P' represents 50%, but it is not. Is it just a sketch? There is some confusion between the ice tie-point 'A' and the ice line 'I' in the figure caption. I think that you should include both the multiyear and first-year ice ('A') tie-points in the plot.

RC1.20 The point P is the result of the simulation for 50% SIC and falls, therefore, half way to between the point O and the intersection with the ice line. We are unsure why P does not look in the middle, but it is. We changed the green colour used for the intercept point I to red.

(Given the modification of the reference snowpack that does not include the dry depth hoar anymore in the summer season in this example in Fig.1 we showed the dry snowpack observation in the warm season).

Given the focus on the MIZ we did not model the multi-year ice in this study, we now listed the ice types more clearly in the manuscript.

T2: Please explain: 'Water substrate – True'? Please use sea ice terminology.

RC1.21 The substrate in the SMRT simulation is the lower boundary conditions for the radiative transfer equation. In this case we impose the ocean beneath the ice. This has been removed from the table and clarified in the text: "In the sea ice simulations, the boundary condition for the radiative transfer equation is imposed by activating the "water substrate" for the lowest layer in SMRT, which represents the ocean beneath the ice."

P15, L242: delete 'existing'

RC1.22 Done.

Suggested references:

Added.

Lavergne, T., Sørensen, A. M., Kern, S., Tonboe, R., Notz, D., Aaboe, S., Bell, L., Dybkjær, G., Eastwood, S., Gabarro, C., Heygster, G., Killie, M. A., Brandt Kreiner, M., Lavelle, J., Saldo, R., Sandven, S., and Pedersen, L. T.: Version 2 of the EUMETSAT OSI SAF and ESA CCI sea-ice concentration climate data records, The Cryosphere, 13, 49–78, https://doi.org/10.5194/tc-13-49-2019, 2019.

Tonboe, R. T., Eastwood, S., Lavergne, T., Sørensen, A. M., Rathmann, N., Dybkjær, G., Pedersen, L. T., Høyer, J. L., and Kern, S.: The EUMETSAT sea ice concentration climate data record, The Cryosphere, 10, 2275–2290, https://doi.org/10.5194/tc-10-2275-2016, 2016.

Tonboe, R., Nandan, V., Mäkynen, M. P., Pedersen, L. T., Kern, S., Lavergne, T., Øelund, J., Dybkjar, G., Saldo, R., & Huntemann, M. (2022). Simulated Geophysical Noise in Sea Ice Concentration Estimates of Open Water and Snow-covered Sea Ice. IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 15, 1309-1326. https://doi.org/10.1109/JSTARS.2021.3134021.

— REFEREE 2 —

Review of

Uncertainty of Antarctic sea ice concentration using passive microwave retrievals in the marginal ice zone

by Stentella, M., et al.

Summary: Changes in the surface properties of the sea ice / snow and open water surface in the marginal ice zone (sea ice concentration (SIC) 15% to 80% as defined in this manuscript) have an impact on microwave brightness temperatures (TB) used to compute the SIC. These surface properties could be different from those within the ice pack (> 80% SIC). This could have consequences on the retrieval uncertainty of the SIC because, typically, these retrieval methods are based on so-called tie points - i.e. reference TBs defined and/or selected for 0% and 100% SIC. This manuscript aims to better understand the contributions of various surface properties and their variation on the SIC retrieval using microwave TBs. For this purpose the authors use the SMRT model and the PARMIO model, simulating the TB response to various ranges of relevant surface properties (e.g. salinity, temperature, grain size and surface wind). Instead of presenting this response in form of TB or emissivity changes, the authors compute the resulting impact on the SIC retrieval for a few typical SIC values within the marginal ice zone.

The manuscript is very well written. The topic is interesting and deserves attention. The work and the results presented will contribute well to the existing knowledge in the field.

I have a number of general comments or concerns (see below) that I see mandatory to be improved in a revised version of the manuscript. These general comments are elaborated in the specific comments you will find further down. I also have a number of editoral comments which I put a the end.

The authors thank the referee for their constructive comments and suggestions. We appreciate the depth of expertise reflected in these remarks, which helped us improve the clarity and robustness of the study. These improvements have been incorporated into both the modelling framework and the presentation of the results.

General Comments:

GC1: The manuscript would benefit substantially from a more careful incorporation of work that has been done in this field.
Additional relevant literature on this topic has been included in the revised manuscript, particularly in the new background and introduction section versions and through references added throughout the text. More details on the answers to address this issue are in RC2.2, RC2.3. Additionally in the revised discussion section we incorporated additional comparison with the work done before.

GC2: The motivation to carry out this study - aka why do we need such an investigation when studies exist for open water and near-100% sea ice concentration - is not sufficiently clear or elaborated.
Motivation for this study is elaborated in the revised background section, where additional comparisons with previous work are provided, as described in responses RC2.3.

GC3: The motivation why you chose the retrieval algorithm you chose and the models you chose is not sufficiently well laid out.

The motivation supporting the choice of working with the Bootstrap algorithm in frequency mode (BF) has been included in the manuscript as in answers RC1.1 and RC2.3.

SMRT and PARMIO are two state of the art microwave emission models for the cryosphere and the ocean respectively that have been validated extensively in their respective environments. We choose to use them in combination in a region like the southern marginal ice zone because it is an area where the ocean contribution to the signal is increasingly important and the modularity and flexibility of these two models allows for a thorough sensitivity analysis spanning both over the relevant parameters and choice of physical theories. This is now stated more clearly in the introduction.

GC4: It is not sufficiently clear how well the models chosen are fitting the purpose of your study. The results are not fully convincing and maybe they are not convincing because the choice of model parameters is not sufficiently well thought through.

Concerning the choice of the parameters we have added further discussion and references in RC1.2, RC1.13 (for the thirst year ice and the nilas), RC1.3 for the atmosphere, RC2.7 for grease and slush (ice types added in the revised version of the manuscript).

GC5: It would be helpful for the reader whether your investigation targets ante-processing or post-processing "errors".

We investigate ante-processing errors: uncertainty on the SIC lead by the variability of the physical parameters. This has been clarified with improved coherent wording throughout the text as listed in comments 2.3: “The total uncertainty on SIC retrievals is usually defined through two components: the algorithm uncertainty, which includes sensor noise and the residual geophysical variability of the ocean, sea ice and atmosphere, which is the focus of this study, and the smearing uncertainty, arising from the mismatch between satellite footprints and the target grid (Tonboe et al., 2016; Lavergne et al., 2019), which we do not address in this work.”

Specific Comments:

L10: It is not entirely clear what you mean by "SIC error" in this context. I propose that you motivate further up why you are interested in the SIC error and why.

RC2.1 We changed ’error’ to uncertainty and, in the expanded Background section (answer RC2.3) we have better motivated why an accurate determination of the SIC is important, to highlight the relevance of evaluating its associated uncertainty. We answered with an additional explanation in comment RC3.4.

L68-90 / Background: I suggest to elaborate on this section --> GC1 / GC2 / GC3 / GC5

RC2.2 The current version of the manuscript has been improved by incorporating more of the previous work. The reference list was reduced too strongly during our internal revision process, and we therefore restored the most relevant references as well as those suggested, integrating them into the background section where appropriate. This also helped more clearly articulate the motivation for this study: specifically the importance of improving our understanding of sea-ice concentration in the transitional regime between open water (0%) and the consolidated ice pack (100%), the value of gaining further insight into this intermediate range and therefore the choice of the Bootstrap algorithm to analyse the investigation of the SIc uncertainty (RC2.3).

I recommend to look into studies where weather and other effects on sea ice concentration retrieval using PM TBs have been studied or discussed. For instance: **Oelke, 1997**, Atmospheric signatures in sea-ice concentration estimates from passive microwaves: modelled and observed, International Journal of Remote Sensing; **Kern, 2004**, A new method for medium-resolution sea ice analysis using weather-influence corrected Special Sensor Microwave/Imager 85 GHz data, International Journal of Remote Sensing.

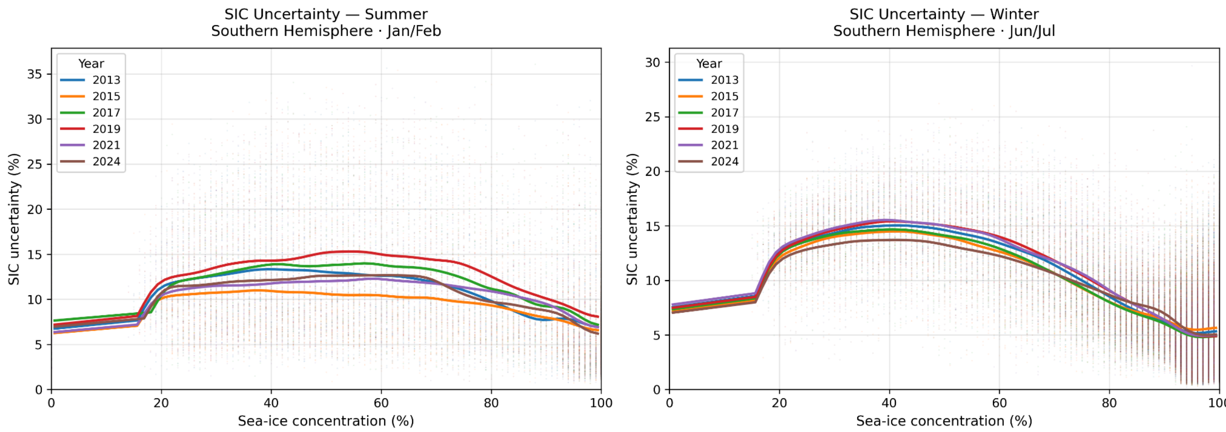
This might be a starting point to dig into literature to understand and motivate your study. A lot is known already about which snow physical quantities and atmospheric quantities cause which errors / biases in the SIC retrieval. This needs to be laid out here. Also, that the thickness of sea ice when thin has an impact is well known. This needs to be laid out here as well. It would important to demonstrate what is going to be new in your study. Maybe it is just that SMRT has not yet been applied to the sea ice environment and this is the main motivation of the study. In any case, please make sure to point out the novelty and where your work makes progress in this field of research.

Secondly, you might find it helpful to look a bit into the more recent publications about sea-ice concentration retrieval, e.g. **Lavergne et al., 2019**, Version 2 of the EUMETSAT OSI SAF and ESA CCI sea-ice concentration climate data records, The Cryosphere.

Thirdly, the work of **Kern et al. (2019, 2022)** might enlight you a bit about the effect this natural TB-variability at 100% has on the SIC retrieval at 100% (and at 0%) and to some degree also for your SIC range: Kern et al., 2019, Satellite passive microwave sea-ice concentration data set intercomparison: closed ice and ship-based observations, The Cryosphere; Kern et al., 2022, Satellite passive microwave sea-ice concentration data set intercomparison using Landsat data, The Cryosphere.

Finally, you might want to motivate (even) a bit more why an investigation like the one you do, utilizing the Comiso Bootstrap Algorithm and the channels used could also serve to better understand retrieval uncertainties on the one hand and errors - aka - biases to what we believe is the truth on the other hand. In that context you might want to elaborate how your work is related to the sea ice concentration retrieval uncertainty estimates that are provided alongside the NOAA/NSIDC and EUMETSAT/OSI SAF products.

RC2.3 Concerning the NOAA/NSIDC uncertainty estimate, this discussion is now added in the revised manuscript, with the reference to the dataset product used; the plots are shown here for visualization purposes.: “As a further point of comparison, the uncertainties provided with the NOAA/NSIDC product show mean values around 10-15\% across 20% to 60% SIC range, with a decrease towards higher SIC in both seasons. The uncertainties of this product are different estimates compared to those presented in this work, which characterises the sensitivity of the brightness temperature signature to the surface properties, and a first estimation of this variability on SIC prior processing. Nonetheless, the order of magnitude is consistent: near the upper MIZ boundary the mean is between 5 to 10 percentage points, with more scatter between the year-to-year variability in the warm season. This is expected due to the higher variability of the snowpack surface properties of the first year ice during the melt season, and it is in agreement with the uncertainty range derived for each parameter, in the first year sea ice configuration. The higher uncertainty observed in winter compared to summer in the 20--60\% SIC range may be due to the larger impact of higher thin ice fraction, yielding a higher uncertainty in the SIC range."



The relevant references have been included in the revised manuscript. The background and introduction sections have been expanded to provide a more thorough overview of previously conducted work, situating the present study within the existing literature and better motivating the need for this investigation in the MIZ environment. Additional comparisons with prior studies have also been incorporated into the discussion.

L94-102: --> GC4, I am wondering whether you could provide some evidence from the published literature that your choice of layers used to describe the snow on top of Antarctic sea ice is a reasonable representation of the true conditions. While depth hoar might be an ubiquitous feature on the old sea ice in the Weddell Sea I doubt a bit that this is a common feature for the sea ice in the marginal ice zone - which - to a large part might be forms of young / new ice and pancake ice. I note in this context that your "model" sea ice is first-year sea ice. I am also wondering what the two "superimposed sea ice layers" are? Are these a result of snow-ice formation processes? This is not sufficiently clear. What I am missing in your description is how humidity in the snow and salinity in the basal snow layers is treated.

RC2.4 Regarding the snowpack configuration, the depth hoar layer is included to represent the ice pack at 100% SIC, where it is commonly observed, particularly at higher latitudes (Massom 2001). We acknowledge that in the MIZ this layer can be absent or negligible; this case is represented in our simulations by setting depth hoar thickness to zero. As shown in Figure 9a, 11a through the orange points, the SIC uncertainty associated with depth hoar thickness variability is small at low MIZ concentrations. To make this scope clearer we added, in the revised manuscript: “We selected the snowpack layering over the first-year sea ice at 100%, based on the frequency of occurrence of the layers in Massom 2001. ”

And we stated that the forward modelling method aims to a circumpolar representation from the background section: “By employing SMRT and PARMIO together, to have modularity and flexibility, we perform a sensitivity analysis of the physical properties of the sea ice, ocean and atmospheric components, sampling a broad range of parameters drawn from the literature to represent the circumpolar variability across two seasons.”

The two superimposed ice layers allow for considering a temperature gradient between the snowpack and the ice layers. In the new version of the manuscript we included a parametrization for the snow-ice layer at the interface between the ice and the snowpack and we clarified the role of this layer. In addition, we specified that the thin top sea-ice layer represents the temperature gradient, important within the sea ice, and we used it to test the sensitivity to the snow-ice interface temperature.

Concerning the inclusion of more ice types the answer is in RC2.7.

How does the model compare to MEMLS and MEMLS modified for sea ice?

RC2.5 Model comparison is out of the scope of this manuscript. Nevertheless, it would be possible to run SMRT in a MEMLS equivalent configuration from within the model itself (Picard et al. 2018).

MEMLS modified for sea ice is not publicly available.

What I am missing in the description of the simulations (in general) is whether and how you take into account i) the incidence angle of around 53 degrees and ii) any surface roughness of the sea ice. What is the roughness length scale used for the sea ice?

RC2.6 Yes, the incidence angle is taken into account in the simulations where we run the simulations with the AMSR2 configuration and we use no roughness length scale for the sea ice. This is clarified in the text in these lines:

- ”No additional roughness length scale is taken into account for any sea ice type.”
- “Finally, the sensor employed in the simulations is AMSR2, using the 19 GHz and 37 GHz channels, both with an incidence angle of 55°.”

L122-124: Given the fact that a considerable part of the marginal ice zone - particularly in the Southern Ocean - can be composed of young / new ice, the way how this ice type is treated is clearly not optimal. What you can resolve with this kind of representation is "dark nilas" - that kind of thin sea ice that forms (often still elastic) sheets of ice of up to 5 cm thickness. But this is only a small fraction of the thin ice that is encountered in the marginal ice zone (in the Southern Ocean). How about frazil and grease ice? How about thicker types of sheet ice such as light nilas (up to 10 cm), grey-ice (up to 15 cm), grey-white ice ...? And, finally, how about that classical type of young ice that is encountered in the Southern Ocean regularly: pancake ice which is characterized by small (10 cm) to large (3m) diameter ice floes, exhibiting rims of often several centimetres thick slush? --> GC4

RC2.7 Modelling the full range of newly formed ice types remains very challenging for current microwave emission radiative transfer models, and we recognise the importance of stating this limitation more explicitly and to try to improve our sensitivity in this direction.

In the new discussion section there is: ‘A remaining challenge concerns the difficulty of modeling the variety of newly formed ice types present in the MIZ, such as pancake ice, for current microwave emission models; therefore, this also applies to SMRT. ‘

We expanded the ice type configurations to better represent the variability of the Antarctic MIZ, as much as possible. This includes for example exploring the gradient within the thin-ice category and adding a snow-ice parametrization (answer RC2.4). In addition, we included a section including a sensitivity analysis to fraction of grease and slush in the sea ice brightness temperature tie point. These additions helped illustrate the large-scale variability our approach aims to capture at low SIC, without departing from its circumpolar, high-level scope.

In the revised version of the manuscript we added these new ice types description and modelling in:

Section 3.1: Cryospheric brightness temperature simulation; general description of the ice types modelled.

Changes in Section 3.1.1, for the snow ice layer addition:

- We added to Line 97: [...] “the depth hoar layer, a snow ice layer at the snow-ice interface and two superimposed sea ice layers characterised by a temperature gradient. [...] On the sea ice surface we impose a snow ice layer forming through flooding by ocean water during intense snowfall events and characterised by large grain sizes, elevated salinity, and higher density relative to the overlying snow layers (Massom et al., 2001). The first year ice is represented by two layers: a thinner, colder upper layer, and a thicker lower layer, in contact with the ocean, reproducing the vertical temperature gradient characteristic of first year sea ice (Lewis et al 2007).”
- Line 108: “The microstructure model used for the snow and the snow ice layers is the unified_scaled_exponential (Picard et al., 2022) which takes the microwave grain size l_{MW} as input (Picard et al., 2022). We compute l_{MW} as the product of the

polydispersity and the Porod length. The microwave polydispersity value (Picard et al., 2022) is set to 0.7 for the windpack and 1.5 for the depth hoar and the snow ice ."

In the revised manuscript, a new Section 3.1.2 (Dark nilas, slush and grease simulation) has been added, describing the simulation approach for these three ice types as detailed in RC1.13. The corresponding results are presented in Figures 5, 6, and 7 and are discussed in the Results and Discussion sections of the revised manuscript.

L125-136: --> GC4, What I find is not sufficiently clear in this paragraph is the range of atmospheric conditions PARMIO is considering. Are these conditions given by theoretical formulas or is PARMIO driven by, e.g., atmospheric reanalysis data?

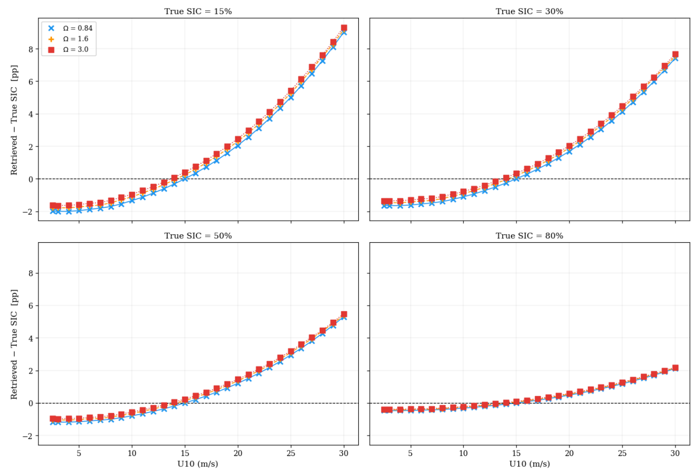
Secondly, you are quite exhaustive with respect to the treatment of foam. I was wondering, however, how relevant this is once you are within the MIZ? Does the model incorporate the influence of the fetch on the ocean surface roughness which is substantially reduced once being in the sea ice?

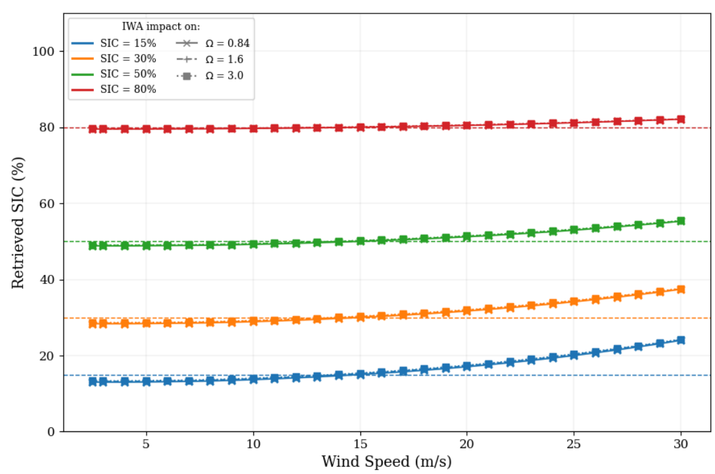
Finally, I was wondering whether you are also including any influence of the atmospheric water vapor or cloud liquid water (see, e.g., **Ivanova et al. 2015** or the paper by **Lavergne et al. 2019** which I mentioned earlier). The larger the open water fraction is the larger is the effect of these two parameters.

RC2.8 We have refined the description of the atmospheric and ocean surface component in PARMIO as suggested by the referee to assess the magnitude of the changes in the results when expanding the sensitivity analysis to take into consideration the parameters described in the following answer .

At present, PARMIO uses an analytical formulation for the atmospheric profiles that works worldwide, with the Sea Surface Temperature as the only input parameter rather than reanalysis-driven input. Note that we also complemented this with sensitivity tests using ERA5 atmospheric profiles for both the cold and warm seasons, over sea ice and open ocean (for more details, please see the answer RC1.3).

Regarding the ocean surface, we tested the impact of fetch in our simulation results. PARMIO incorporates the inverse wave age as a parameter controlling the ocean surface roughness state, which was held constant in our experiments. To evaluate its impact on brightness temperatures, we performed a sensitivity analysis varying the inverse wave age. As explained in Section 3.3 of the revised manuscript we now include these simulations The small waves are taken into account in the Donelan et al. 1993 description for the drag coefficient while the large waves impact through Elfouhaily 1997 sea spectrum model. We tested three values: Ω=0.84, representing a fully developed sea with wind and waves in equilibrium; Ω=1.6, representative of young seas as observed near the ice edge or in fetch-limited conditions such as the WISE campaign in the Mediterranean Sea (Dinnat et al., 2003); and Ω=3.0, representing an extreme young sea state typical of strongly fetch-limited conditions near land or ice (Janssen et al., 2004). The results below show that the inverse wave age, for the fetch conditions tested, have a negligible impact on the brightness temperature also for low sea ice concentrations and this impact decreases at increasing SIC.





Additionally, we include the results in the manuscript (Fig. 4) both with and without foam. While foam should be accounted for at high wind speeds (up to 30m/s) and low sea ice concentrations (SIC 15%), and its inclusion leads to increased TB, we also show results without foam to demonstrate that its impact becomes negligible at higher sea ice concentrations regardless.

References:

Dinnat, E., Boutin, J., Caudal, G., Etcheto, J., & Contardo, S. (2003). On the use of EUROSTARRS and WISE data for validating L-band emissivity models. Proceedings of the First Results Workshop on EuroSTARRS, WISE, LOSAC Campaigns, 4 - 6 November

2002, Toulouse, France. Ed.: Peter Fletcher. ESA SP-525, Noordwijk, Netherlands: ESA Publications Division, ISBN 92-9092-835-2, 117–124.

Janssen, P. (2004). The Interaction of Ocean Waves and Wind (1st ed.). Cambridge University Press. https://doi.org/10.1017/CBO9780511525018

L138-145: "estimated SIC" --> Please provide information about the algorithm that was used to generate this SIC estimate. Please specify the grid that is used (polarstereographic or EASE2.0 I assume).

RC2.9 The SIC is provided on a Southern Hemisphere polar stereographic grid (WGS 84 / Antarctic Polar Stereographic, EPSG:3031) at 12.5 km resolution, with a standard parallel at -71°S and central meridian at 0°. We added this information in the data description.

"date under analysis" --> this reads as if you are only considering a limited set of days. Please mention them and/or list them in a table.

RC2.10 The usage of the dates as representative for one season (cold or warm) has been clarified in the manuscript, as shown in answer RC1.11, RC2.14 ".

extending it 30 km into the ocean" --> it is not sufficiently clear what you aim with this 30 km wide margin. Do you want to include open ocean conditions? Then this margin is very likely far too small. Especially in the Southern Ocean there can be substantial regions that are covered with SIC < 15% but clearly > 0%. In addition to that the field-of-view (see one of my earlier comments) of the used AMSR2 channels is i) larger than the 12.5 km grid resolutions and ii) any strong gradients in the actual sea ice cover and hence the resulting TBs measured are blurred. Hence a compact sea ice cover in one grid cell can easily spill over to the next two grid cells adjacent to the ice edge - which is basically your 30 km margin; hence, in that case your margin does not allow you to have a "clean" open water signal. But as I stated above, the intention of using this 30km margin is not clearly enough stated.

RC2.11 To more clearly show the reliability of the model output under open-ocean conditions, we corrected the mask as described in comment RC1.17.

Table 1: You provide the reference, the minimum and the maximum value. Would you mind to also include the steps with you varied the parameters through the range given?

RC2.12 The parameter was varied in steps of `(max_val - min_val)/20` across the given range, resulting in 21 evenly spaced values from the minimum to the maximum. This is now clarified in the manuscript.

The "Fraction_Volume_Water_SP" ... is what: the volumetric liquid water fraction? What is the unit? Please make sure that this is a range similar to those used in other / previous studies, e.g. Ulaby et al., Microwave Remote Sensing –Active and Passive, Vol III, ArtechHouse Inc., 1986. and/or explain how the range used by you translates into the range used be these other studies; therein volume water contents of 10% are considered. But perhaps your maximum value of 0.2 translates into 20%...

RC2.13 The terminology of *liquid water fraction* is used consistently throughout the manuscript. The quantity Fraction_Volume_Water_SP refers to the volumetric liquid water fraction. The value of 0.2 corresponds to 20% liquid water fraction within the considered layer. The liquid water content in each layer has been adjusted and referenced according to literature (see RC 1.2) .

It is explicit in the manuscript, at the first introduction of this quantity: "The liquid water fraction is defined here as the volumetric fraction of liquid water within the considered ice layer, i.e. the ratio between the volume of liquid water and the total volume of the layer. Values therefore range between 0 and 1 and are dimensionless".

L150-155: This part is not sufficiently clear. You have n days (how many?) of AMSR2 observations (where exactly?). For these AMSR2 observations you have near 19 GHz and 36.5 GHz TB values AND sea-ice concentration values. Hence, from these observations you can select (within your region of interest) pixels where the SIC is exactly 100.0% and exactly 0.0% (see Kern et al., 2019, mentioned before, however, to understand what "exactly" 100% could mean with the kind of algorithms used, where SIC values above 100% are folded back into the range 0-100% and/or simply truncated). So, simply from the AMSR2 data set you can come up with a set of tie points, i.e. typical TB values (plus/minus a range) for the channels used and the two end members of the SIC distribution 0% and 100%. What do you then do with e model? This is not clear to me. You use the values given in Table 1 and tune them such that the distance between the simulated and the observed TB for 0% and 100% SIC matches the tie points you derived from the AMSR2 data? Why only "visually overlaps"? Don't you have a means to do that more accurately? What is not sufficiently clear how you "adjust" the parameters? Which parameter is given preference and why?

RC2.14 Here we clarify the role of AMSR2 observations, the tie point selection, and the model parameter adjustment in our analysis. In this study, our objective is not to perform an accurate physical reconstruction of the AMSR2 brightness temperatures for specific dates. Instead, the observational data are used as a background realism check to evaluate whether the range of brightness temperatures produced by the parameter variation choices and the model are consistent with the typical variability in the observations, as suggested in RC1.2. For this reason, we focus on identifying representative tie points. Regarding the selection of tie points, we used the background brightness temperatures from AMSR2 pixels within the region of interest, identifying the regions of the TB space corresponding to 0%

and 100% sea-ice concentration for a given date. As the referee notes, retrieved SIC values may involve truncation or folding depending on the algorithm. Our intention is not to treat these as absolute ground truth, but to verify that our modelling framework reproduces a realistic range of brightness temperatures for the two extreme surface conditions, which we then use to constrain our tie point. We clarified this approach in the revised manuscript . In addition, instead of showing only on date of observations for the ASMR2 background we included in the background 96 dates per season, showing that they yield a consistent background brightness temperature reference for the purpose of modelling a general circumpolar snowpack (answer RC1.11).

Concerning the adjustment of the model parameters: our goal is to explore the sensitivity of brightness temperatures to variations in key physical parameters and to investigate how this propagates into the uncertainty of SIC retrievals. Since the modelling is used in a sensitivity-analysis perspective, rather than for precise forward simulation of the observed AMSR2 values, we did not aim to optimise the model parameters to quantitatively reproduce the observational tie points. Instead, we adjusted the parameters qualitatively (process described in the MS as ‘visually overlap’) to ensure that the modelled TB range for open water and consolidated ice falls within the typical range of the observational tie points. This avoids introducing additional uncertainty associated with using observations-derived tie points.

This is now more clearly explained in Section 3.6 of the revised manuscript.

Figure 1 / caption: It is unclear how many days (and which days) worth of AMSR2 data are included in this figure. It is not sufficiently clear why the figures uses a point P that is representing a warm season snowpack. This implies that the AMSR2 TBs represent summer conditions.

RC2.15 Figure 1 serves as an example of how the Bootstrap algorithm computes SIC for a simulated TB19-37 combination. The simulation is the output of a forward model configuration based on one day of observations, chosen here as a summer season example. The figure caption and description has been improved and clarified in the revised manuscript, and an explanation for the date choice has been added as outlined in the response to RC2.14.

I note that the maximum 19V TBs are located at values > 270K. This suggests a surface emissivity of almost 1 - which is contradicting existing knowledge a bit (I know, you are only plotting observations here but seeing such high values near the 100% SIC line rings a warning bell).

I note that there is a light indication of an "extended" open water tie point in the AMSR2 TB data (the inclined light point cloud going from point O towards the top right.)

RC2.16 We believe that the values near the 100% SIC line with really high TB plotted as background observations from the ASMR2 product are representative of melt episodes. Except if open ocean or ponds are present within the footprint, low to moderate wet snow with a smooth surface has extremely high emissivity and a temperature of 273 K, which results in TB > 270K at 19V near the Brewster angle.

The ‘extended’ open water cluster is to be attributed to the representation in the V3719 space, where the ocean observations (impacted for instance from wind) are not on the OA line or to its left like in HV37 (Comiso 2012).

Finally, warm snow pack and dry depth hoar layer seems not to be a realistic combination.

RC2.17 We acknowledge the referee's comment regarding the non-physical representation in this sensitivity analysis case, which has now been removed in the revised manuscript.

Table 2: So you keep the temperature of the sea ice at a constant -1.8degC and do not allow any gradient within the sea ice? Is this realistic? I think it might be realistic for ice type grease ice but it is not realistic for sheet ice such as dark and light nilas.

RC2.18. In the current manuscript, we simplified the thin ice representation, as the Bootstrap algorithm in frequency mode, the configuration applied for this ice type, relies on vertically polarised channels that are inherently less sensitive to thin ice layering and polarization effects (Comiso et al., 1997). Following the referee's suggestion, the temperature gradient has been included in the revised manuscript, as also mentioned in comment RC2.7. Given that, in this thickness range (< 0.05 m), the microwave signal is primarily sensitive to the uppermost part of the ice column, and that newly formed ice can exhibit very high near-surface brine salinity (10–25 PSU; Naoki et al., 2007), the thin-ice emission is largely controlled by the properties of this top layer. Consequently, the uncertainties on SIC are comparable to those obtained in the sensitivity analysis presented in the first version of the manuscript (RC2.7).

Did you check what typical values of the density of thin ice are used in the literature?

RC2.19 Yes, we did verify the typical values of thin sea-ice density reported in the literature. (Timco and Frederking 1996, Cox and Weeks 1983, Timco and Weeks, 2010), however in the context of our simulations, we tested density values within this commonly used range and found that model outputs showed negligible sensitivity to realistic variations in thin-ice density.

L196-200: "LWC" --> In Table 1 you write "Fraction_Volume_Water". Is this the same as LWC here? It would be good to use the same name for one variable or quantity and not two or more

Yes, we improved the clarity throughout the text and the tables maintaining the same variable name, the Fraction Volume Water in Table 1 has been changed to Liquid Water Fraction (LWF) defined in the manuscript.

The response of the TB to the LWC of the snow pack should reveal some temperature dependence because the LWC is a strong function of the temperature. Also, if the topmost snow layer with a high LWC is thick enough the impact of any layer below should be rather small because all contributions being emitted from below the wet surface snow layer are attenuated therein and only the own emission of the wet snow surface layer determines the TB. And, you should see a difference between the 19 GHz and the 37 GHz channels in this regard because the lower frequency channels are known to penetrate (a bit) deeper into the snow pack.

RC2.20. The LWC sensitivity analysis is performed at 273 K strictly while LWC=0 at the lower temperatures. The penetration depth under wet conditions is extremely small even at 19 GHz so that only the top layer (here 5cm and 10cm for the two seasons) is emitting in these conditions.
Now this is specified in the manuscript: “ Note that the liquid water fraction sensitivity analysis is performed at a fixed temperature of 273 K.”

I suggest to state more clearly where you are referring to snow parameters and where you are referring to sea ice parameters. It is density, temperature and salinity of the snow that show negligible impact, right?

RC2.21 The distinction between snow and sea ice parameters is currently stated in the manuscript in a general way: density, temperature, and salinity refer to the snow layers, while thickness and temperature refer to the uppermost sea ice layer, as explicitly indicated in the text and in Figure 6. We revised the wording to make this distinction more immediately clear to the reader by listing the name of the snow and ice layers used in the configuration description.

L218/219: I cannot see this statement ("for instance, when the layer is absent (null thickness)") being supported by what is shown in Fig. 6 a, b)

RC2.22 The manuscript is revised to correct that, for this specific example, the reference is to Figure 6a only (now with new numbers and letters because of the modified figures (e.g. 9a, 10a)).

Figure 2: Clearly, there seem to be largest changes at the higher sea-ice concentration values. I was wondering whether it would be worth to come up with an additional set of figures where you zoom into the TB development for SIC values > 50% or similar. You could, if you want, focus on those parameters there which have the largest impact. That way one could see (and potentially discuss) how TB changes related to an increase in LWC are counteracting TB changes related to an increase in grain size.

RC2.23 As noted by the referee, variations in sea-ice physical properties produce larger brightness-temperature changes at higher sea-ice concentrations due to the linear combination of tie-point brightness temperatures. Given the focus of this study on low SIC conditions characteristic of the MIZ, we have added clearer figures highlighting the distinct signatures of different sea-ice types (e.g. thin ice, grease ice, and slush) to better illustrate the physical mechanisms driving variability at low concentrations and to avoid these effects being more difficult to isolate than the variations at high SIC. In addition, the text has been expanded to comment explicitly on the parameters with the largest impact at higher SIC.

Aren't you surprized that none of the simulations you showed so far stretch more along that limb of TB observations that is located approximately where one would draw the "ice line" (see Fig. 1). What is the explanation for that?

RC2.24 The focus of the present study is specifically on the marginal ice zone (MIZ) and low-concentration sea ice. Because our goal is to quantify how variations in surface parameters influence SIC retrieval uncertainties when SIC < 80%, we did not attempt to model the full range of radiometric signatures associated with high-concentration including deformed, snow-loaded, or multi-year ice. Representing this variability would require a dedicated modelling framework and parameter space beyond the scope of the present work.

As a result, our simulations were not designed to reproduce the entire distribution of the 100% SIC tie-point space, but rather to reproduce the variability around the oblique open-water–to–low-concentration ice line (line OA in Fig. 1) which is relevant to compute SIC on low-concentration conditions. This region of the TB19V–TB37V space is where the ocean contribution dominates and where the retrieval uncertainty changes most rapidly. We clarified this more explicitly in the revised manuscript.

Figure 3: Here my comment made earlier with respect to zooming in also makes a lot of sense because the colored data points are hard to see where you have a high density of AMSR2 TB observations.

RC2.25 We understand that the high density of the AMSR2 brightness temperature observations prevents to distinguish the colored data points in regions of high sea ice concentration, we made the plot, with a lighter background and, given the focus on lower sea ice concentration area of this work , we have introduced new focused figures focusing on the ice types characterizing the MIZ (RC2.7).

Figure 4: How do these simulation results compare to results published in the literature (e.g. **Andersen et al., 2006**, Improved retrieval of sea ice total concentration from spaceborne passive microwave observations using numerical weather prediction model fields: An intercomparison of nine algorithms, Remote Sensing of Environment; **Oelke, 1997**

mentioned earlier, in relation to sea-ice retrieval and other studies related to wind vector retrieval using, e.g. WindSat?). Did you cross check your results?

RC2.26 In our sensitivity analysis, wind speed is perturbed independently under clear-sky conditions, isolating its individual contribution to SIC uncertainty, while in a separate analysis we consider the combined effect of atmospheric profiles informed by ERA5 reanalysis (RC1.3). Nevertheless, comparisons of the general magnitude of SIC uncertainty from wind speed perturbation and from the full atmospheric profiles has been added to the discussion section of the revised manuscript.

This is now in a new paragraph into the discussion section.

Figure 5, panel a): What is your explanation for why all modeled TB combinations are located to the right of the line one would draw between the open water tie point and the ice line as shown in Fig. 1? How do these results compare to investigations done by others? There is some information about the impact of the thickness of thin ice on SIC retrieval accuracy in Ivanova et al., 2015, cited by you. In addition, there were several studies back in the 1990s or even 1980s where people investigated PM signatures of the different forms of thin ice - e.g. **Cavalieri, 1994**, A microwave technique for mapping thin sea ice, J. Geophys. Research - Oceans and references therein.

RC2.27 Concerning the signature of the thin ice located underneath the OA line we added a clarification in the manuscript as in RC1.8. New paragraphs have been added to the Discussion section to contextualise our results within the existing literature. In particular, we discuss the consistency of our findings with previous studies regarding the impact of newly formed ice types (thickness <0.30m) on the microwave signature, which is known to lead to systematic underestimation of sea ice concentration across all retrieval algorithms -therefore also BF (Tonboe et. al, 2022).

L243-248: --> GC5, "In Comiso (2013), ... of SIC." --> The error you are reporting here is - if I am not mistaken - for 100% SIC. I don't think Comiso reported on errors at different SIC values. Please also check whether what Comiso (2013) reports as error is comparable to what your results are representing. Your results are - to my understanding rather an estimate of the uncertainty of the retrieved SIC as a function of the actual snow and sea ice properties - hence how well the tie points chosen are meeting the actually encountered conditions. Uncertainty and error are two different things; uncertainties are ante-processing while an error values is obtained post-processing.

RC2.28 We corrected the manuscript accordingly, reframing the discussion in terms of uncertainty rather than error, and clarifying that our results represent an estimate of the uncertainty in retrieved SIC as a function of the actual snow and sea ice properties.

Your write about 2-3% of LWC (which is 0.2-0.3 in your notation of "FRACTION_SNOW_VOLUME"?). If you check the work of Garrity, 1992 (a chapter of the

Book entitled "Microwave Remote Sensing of Sea ice" edited by Carsey) and also the book of Ulaby et al. (1986) you will see that penetration depths into snow and with that changes in the microwave emissivity begin to have a notable impact already at considerably smaller LWC values.

RC2.29 At line 246 of the manuscript 2%-3% is the 0.02, 0.03 liquid water volume considered in the snowpack. We deleted this sentence, avoiding the comparison with Comiso (RC2.28). In addition, in the revised version of the manuscript we adjusted separately the LWF intervals for the cold and the warm season, based on literature-derived parameter ranges as in Table 1.

L255: "This sensitivity ... development stage." --> While this is a true statement your investigations do not back up this notion. You did not take into account sufficiently well the different development stages of young / thin sea ice in your work. See my previous comments about the set up of your model with respect to thin ice.

RC2.30 In the revised version of the manuscript we included a new sensitivity analysis for the thin ice, with respect to salinity, temperature and brine volume fraction This allowed us to better capture the evolution of some of the physical properties associated with the aging of dark nilas (ice thickness < 0.1 m). In addition, we extended the analysis to newly formed ice, including grease ice, in order to more comprehensively represent the early stages of sea-ice development and to strengthen the support for the conclusions drawn (RC2.7).

We included a clearer statement on this topic in the revised discussion: ‘In this work, the thin ice development is approximated through the variability of the properties of grease ice or the top dark nilas layer, which affects the most the microwave signature at the frequencies used (Naoki et al., 2008b).’

L259: "Worby, 2004)" --> My recollection of this work might be wrong but I think the main thing this work is referring to is the fact that the sea ice near the ice edge is often wet and overwashed by waves quite often, resulting in a microwave signature that is much closer to that of open water than that of sea ice and that this is the main reason why sea ice concentration maps often show too little sea ice in the MIZ / near the ice edge as demonstrated against ship-based observations. It is not necessarily the increased open water surface emissivity due to wind. But maybe I am wrong.

RC2.31 The wind and weather effects are only briefly mentioned in Worby (2004). A more suitable reference for the wind effect, alongside those already in the manuscript, would be Oelke (1997), suggested by the reviewer in a previous comment.

L260-263: "In addition ... AMSR2 observations." --> Please take into account / discuss here the issue I brought up earlier with respect to the limited fetch in the sea ice covered area which leads to a completely different ocean surface wave spectrum here than in the open, sea-ice free ocean.

RC2.32 We performed the sensitivity analysis to fetch parameters as in answer RC2.8.

L264-268: --> GC5, In this paragraph you again need to be more careful with the expressions used. You are mixing error, uncertainties and bias. Again, what you are investigating is contributing to a better understanding of the sea ice concentration retrieval uncertainty. You are not comparing sea ice concentrations retrieved from AMSR2 observations with independent SIC estimates in, e.g. thin sea ice areas. Then your results would be about the actually observed errors or biases in the retrieved product.

RC2.33 We changed 'errors' to 'uncertainties'. We have left the terminology 'bias' just for defining the bias, systematic underestimation of 5K of the PARMIO model.

You chose to cite Andersen et al. (2007) here. I believe citing the work from 2006 (I mentioned that to you earlier) would be more to the point).

RC2.34 We changed this to the suggested better reference in the revised version.

I don't think that the two references "Notz and Worster" and "Gough et al" add much to the main context of this manuscript because they are not specifically referring to the surface physical properties relevant to the remote sensing with the aim to retrieve SIC.

RC2.35 We removed them in the revised version of the manuscript.

L269-271: "a mask ... characteristics" --> Only a very careful reader would so far have guessed that the map you are using is circumpolar. I strongly recommend to add considerably more information about this part of your work earlier into your manuscript. What I would love to see is why you chose only ONE day per season; why you chose these particular two days and also how the SIC looks like. I recommend that you show a figure of 6 maps, 3 per season, with the actual sea ice concentration and the TBs of the 19V and 37V channels.

RC2.36 We added a description of the mask characteristics as in the paragraph in answer RC 1.17 and an explanation for the updated plots with 96 dates per season as described as in answers RC1.11, RC2.1.

L283/284: This is certainly a very good idea. I made this comment earlier - but I indeed think that it would be very good to check whether, for instance, high LWC values are associated with particularly large or low snow grain radii. This is just one example.

RC2.37 In this work, wide variation ranges were chosen for each parameter to assess their order-of-magnitude impact on the retrieved SIC. The correlations between parameters are not investigated, as the complexity of their physical interactions would require a dedicated study with different tools. Such detailed analysis would be difficult in the MIZ given the current state of the art in terms of snow and sea-ice properties, and their representation in microwave emission models.

The discussion would further benefit from a more in depth discussion of the effect in the different snow layers. What I am missing is a discussion of the penetration depth into / opacity of the snow as a function of LWC and, perhaps, also thickness. The observation that during winter the LWC in the depth hoar layer plays a considerably larger role than during summer is not sufficiently well discussed. I am, in that context also wondering, how realistic a high LWC in the depth hoar layer is. Also here, your manuscript would benefit from a more indepth discussion of the correctness the assumptions made for the model investigations. --> GC4

RC2.38 In the revised manuscript, we stated more clearly that these simulations are intended to represent a broad range of snow and sea-ice conditions that may occur in the marginal ice zone. The discussion has been placed more explicitly in context by reassessing the conclusions with respect to the likelihood that such conditions occur in reality, integrating the results more closely with previously published studies, and adding references where possible to document the range of parameters that were tested. The ranges for the liquid water fraction in the depth hoar have been changed according to the references in Table 1.

In addition to that, what is missing so far in your discussion is the fact that while your results show the sensitivity of two channels 19V and 37V to your selection of snow and sea ice physical parameters, the results about the resulting uncertainty in the SIC are based on the combined effect. You might want to discuss whether it isn't quite logical that the SIC change due to TBs reflecting different snow LWC is lower than expected when the changes in the two TBs due to LWC happen to be into the same direction. This is something that has been discussed to some degree in the **Andersen et al. paper from 2006** illustrating that substantial changes in the TB at different frequencies due to various (atmospheric) parameters might not have an effect on the retrieved sea ice concentration because the effects cancel out each other.

RC2.39 We added a reflection on this point in the discussion: ‘In addition, this analysis treats parameters independently: extensions of this work should consider potential simultaneous variations and their propagation on the SIC retrieval, providing further accuracy to its estimate in the MIZ. This also signifies concealing potential compensating effects contributing to the SIC uncertainty (Andersen et al., 2006). For example, the LWF sensitivity analysis conducted with clear sky atmospheric conditions can have an opposite effect on TB yielding smaller SIC uncertainties, but that were here tested independently.’

Editoral Comments / Typos

L31/32: "pixel ... the spatial footprint of the sensor acquiring the measurement" --> This formulation is not correct. A "pixel" is something we know from digital images taken by our smartphones. In Earth System Sciences / the field of satellite remote sensing one only speaks of a pixel when the satellite images provide very fine spatial sampling, i.e. several tens of meters or less. It is more common to speak about a "grid cell" which then has a "grid cell area" or a "grid cell size" or a "grid resolution". These so-called grids on which satellite data, e.g. PM TBs are provided are the result of projecting the originally obtained TB measurements onto a regular grid representing the Earth Surface.

Now, a "spatial footprint" is something different. PM sensors measuring TBs have a frequency dependent footprint or "field-of-view". This is the projection of the antenna lobe at the surface and very often one uses the 3dB with of that cone intersecting the surface. Hence for a PM sensor of the kind used for sea ice concentration retrieval using incidence angles around 53 degrees the field-of-view is of elliptical shape. Individual measurements are obtained for every field-of-view and the resulting TBs are then sampled / projected into a regular grid.

In short: "pixel" and "spatial footprint" or "field-of-view" shall not be mixed. I strongly recommend to change any mentioning of "pixel" throughout the manuscript towards a more suitable expression.

RC2.40 We revised the terminology throughout the manuscript, replacing 'pixel' with the more appropriate terms 'grid cell' and 'field-of-view' where relevant.

L37/38: "continuous ... otherwise" --> I suggest to be more specific and state explicitly that PM observations are daylight independent and also - widely - independent of cloud coverage.

RC2.41 We added this in the manuscript.

L42/43: "seawater flooding ... sea ice overload" --> The three issues you are mentioning here all inter-connected and it might make sense to stress that. Seawater flooding is the result of a negative sea-ice freeboard (the ice-snow interface is located below the sea surface) due to a heavy snow load and snow ice formation is the result of such a flooding event (provided that temperatures stay below the freezing point).

RC2.42 We agree that the snow-ice deserves more attention. We expanded the description of snow ice formation and incorporated it into the model setup.

L148: "pixel" --> "grid cell"

RC2.43 We changed this in the revised manuscript.

L179: What is the "prescribed" SIC?

RC2.44 The prescribed SIC is the SIC considered as the true value, therefore the mixing factor used for the reference value to find the brightness temperature of the mixed grid cell. Now this is explicit in Sect. 3.7.1.

L185: "1cm" --> Table 2 states 3 cm. What is correct?

RC2.45 The correct value was 1cm in the table. In the revised manuscript the thin ice sensitivity analysis was changed with a thin ice composed of multiple layers as described in Table 2.

L202: What do you mean by "contained impact"?

RC2.46 We clarified this in the revised manuscript, we mean that the changes in the parameters lead to smaller variations in resulting brightness temperature compared to the warm season.

L213-219: Please make clear the reader understands that you are referring to the warm conditions example here.

RC2.47 We clarified this in the text.

L236: "their impact on the radiometric signal" --> Since you have been working with TBs and were not yet talking about the emissivities I suggest that you get back and write "on the measured brightness temperatures"

RC2.48 We changed this in the revised manuscript.

L237: "the T_B signature" --> I suggest to either write "the microwave signature" or "the T_B"

RC2.49 We applied this modification.

L240: "snow optical radius" --> I note that you (again) use different expressions for the same thing. In L235/236 you wrote "snow radius". I recommend to use one expression and make sure that the reader understands that here you are in fact referring to the snow grain radius which translates into the snow specific area (SSA) which then, in your paper, is the snow optical radius, is that correct?

RC2.50 We corrected the nomenclature and used the term ‘snow grain size’ to refer to the 'optical radius' consistently throughout the manuscript, to make the description more intuitive. This is now stated in the SMRT snowpack model description. This quantity is used as input to compute the specific surface area (SSA), from which the Porod length is derived. Combined with the polydispersity, this yields the effective microwave grain size in the model as introduced in Picard et al. (2022). To our knowledge, accurate measurements of SSA for Antarctic MIZ snow are currently not available in literature; Picard et al. (2022) investigated optimal values for ice sheet and seasonal snow with rounded grains and depth hoar, but not for sea ice.

L242: "Fig. 8" --> You are talking about the warm season in this sentence and hence you might not want to refer to Fig. 8.

RC2.51 Both seasonal figures are referred to for seasonal comparison, even when only one season is the primary subject of discussion.

L273-275: In Lavergne et al. (2019) you will find a different realization of the ice line which you could as well mention here.

RC2.52 We included this in the revised manuscript.

L278: "the Antarctic continent" --> better: "Antarctic sea ice".

RC2.53 We changed this.

L286-288: This reads like a very elaborated and complicated sentence. Wouldn't it make sense to begin a bit more simple, stating that you investigated the sensitivity of sea ice concentration retrieval to a set of environmental parameters, among them snow and sea ice physical properties....? In particular, I don't see any "parameterization" I cannot see how your contribution will effectively improve sea-ice concentration retrieval other than better understanding the retrieval uncertainties.

RC2.53 We rephrased this sentence: This study investigated the sensitivity of passive microwave sea-ice concentration retrieval to a set of environmental parameters, including snow, sea-ice and ocean physical properties, characterising the Antarctic MIZ, with the aim to better understand and quantify the sources of retrieval uncertainty.

L290: Please relate "optical radius" to the snow grain size for better understanding.

RC2.54 The optical radius is used to compute the SSA, input in equation (2), now we use everywhere the nomenclature 'snow grain size' introduced the first time 'optical radius' is mentioned, for better understanding.

L298-300: "Importantly, ... models." --> Where did you show this in your manuscript?

RC2.55 In the current version, this comparison is limited (Lines 242-244, 253, 257-259, 265); in the revised manuscript, we extended it to include additional studies that analyze SIC retrieval algorithms applied to observations. This is now described in the revised Discussion section.

L438-441: --> Two times the same reference.

RC2.56 We removed one of them.

— REFEREE 3 —

Review of "**Uncertainty of Antarctic sea ice concentration using passive microwave retrievals in the marginal ice zone**" by Stenella et al., 2025

Summary:

This manuscript investigates how variations in the physical properties of the snow-sea ice-ocean system contribute to errors in satellite-derived sea-ice concentration (SIC), with a focus on the Antarctic Marginal Ice Zone (MIZ), defined here as the region with SIC between 15% and 80%. Because the MIZ is highly dynamic, satellite-based SIC retrievals are known to be less reliable in this region. To explore this issue, the authors simulate microwave brightness temperatures (TBs) over snow-covered sea ice using the Snow Microwave Radiative Transfer Model (SMRT) and over open ocean using the Passive and Active Reference Microwave to Infrared Ocean (PARMIO) model. By tuning model forcing parameters, they generate TBs corresponding to prescribed "true" SIC values. These simulated TBs are then input into the core of the Bootstrap retrieval algorithm, enabling an assessment of how physical-property variability affects retrieved SIC relative to the prescribed SIC.

The authors thank the referee for their detailed review. The suggestions improved the rigour of the methodology proposed and therefore the clarity of the presentation of the results in the revised manuscript.

General Comments (GCs):

Overall, the manuscript addresses an important topic for the polar remote-sensing community. The approach is promising, but several areas require strengthening for clarity, rigor, and contextual grounding in a revised version.

GC1: Although the introduction and background sections provide context on microwave SIC retrievals, they do not discuss documented retrieval biases, algorithm performance, and limitations of commonly used SIC products/algorithms (e.g. **Kern et al 2019**). Currently, the literature review appears incomplete and the motivation of the work would seem more applicable to the Arctic rather than the Antarctic. The most common type of ice found in the MIZ is pancake ice from autumn to mid-spring, and brash fragmented ice in summer. It is more difficult to define the MIZ mask in summertime given the fragmentation, as reported in the papers cited by the authors. The presence of frazil ice, slush and other interstitial types of ice is very common, and the use of a model that linearly combines open ocean with consolidated ice may not appear fully justified unless motivated (see also the next point on thin ice).

RC3.1 Regarding the literature review, we incorporated the relevant studies suggested by the referee, as in responses RC2.2 and RC2.3, in order to better document known retrieval biases, algorithm performance, and limitations of existing SIC products. These changes are now in the Introduction, Background and Discussion revised sections of the manuscript.

As discussed in RC2.7, and following the referee's recommendation, we have expanded the analysis to give more importance to the characteristics of the Antarctic MIZ. We included more ice types typical of Antarctic snowpacks: the snow ice (in the first-year snowpack), the grease, the slush and the dark nilas.

Concerning the SIC mask, we corrected its structure as explained in RC1.17. The objective of the mask is not to provide a highly accurate delineation of the MIZ, but rather to ensure inclusion of the entire ice pack required to define the 100% SIC sea-ice tie point, together with the adjacent open-ocean region used to define the ocean tie point. This, together with including all the intermediate SIC snow and ice surface types typical of the circumpolar MIZ.

GC2: Several assumptions are presented without sufficient justification, citation or explanation. For instance, the choice of the warm or cold days is very arbitrary, and it is not explained nor justified (the dates are only known when reading the captions). Other points that would require further clarity are the definition of thin ice fraction and how it is accounted for in the calculations, the SIC mask and the sensitivity analysis done for SIC values of 10% and 90%, parameter ranges, and reference dataset)

RC3.2 We addressed these different concerns in the revised manuscript as follows:

- We included multiple dates (96 per season as explained in RC1.11) to include brightness temperature corresponding to a wider variety of snow and ocean surface characteristics to show that the forward modelling approach enables the inclusion of the observations background. More details on this choice are in answer RC2.14.
- We revised the sentence at Line 181 for clarifying the thin ice sensitivity analysis:

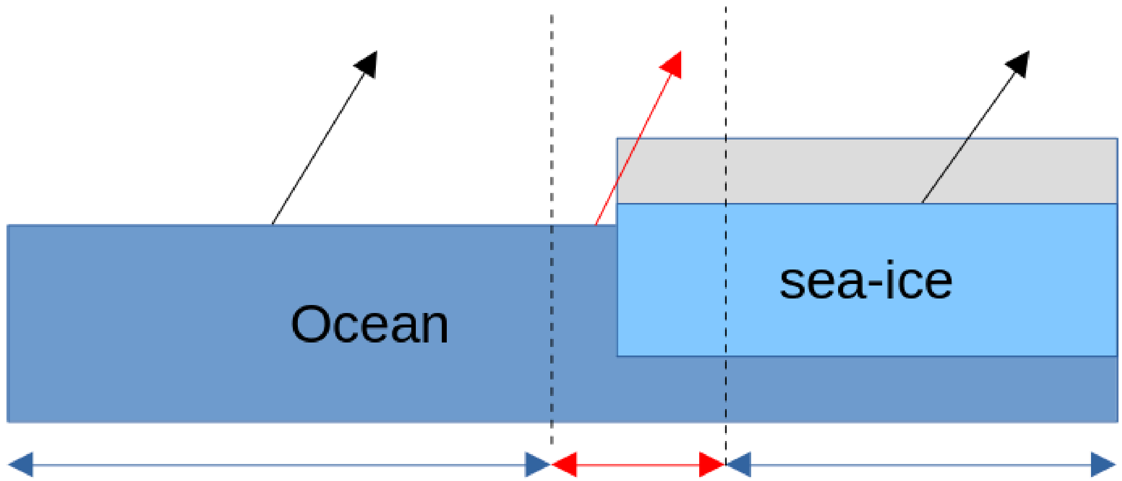
'For each surface type, the tie point XA of snow-covered first-year sea ice is compared to TB values obtained by replacing a fraction *f* of the total sea ice with thin ice at representative values (0%, 15%, 30%, 50%, 80%, 100%), with the remaining fraction ($1 - f$) being first-year ice. The resulting TB for the observation (XP) is computed as a linear combination of the contributions from first-year ice, thin ice, and open ocean, weighted by their respective area fractions. "

- The SIC values ranging from 10% to 90% are chosen for representational purposes, in order to show that the linear combination of tie-point brightness temperatures is able to reproduce the background satellite observations across the full range of sea-ice concentrations. This is now described in the figure caption only, to make clear that it is not a methodological choice.

GC3: The manuscript does not explain how point-based radiative transfer model outputs relate to the stated 12.5 km satellite resolution, nor whether any spatial aggregation or assumptions are applied. Furthermore, the known high spatial heterogeneity of Antarctic sea ice is not sufficiently accounted for in the analysis. The main issue is related to the choice of arbitrary days from the extensive AMSR2 record. The 15-80% mask can be very different in different days, especially in summertime, which would affect the swarm of Tb points that the model is expected to replicate.

RC3.3 In the revised version of the manuscript we clarified that the grid cell brightness temperature is the linear mixing of the two point-model outputs which follows directly from the additivity property of radiance (energy conservation). This requires that the vertical extent of the medium (here, the freeboard) is negligible compared to the horizontal extent, so that lateral radiance flux between sea-ice and ocean are negligible. In the figure below, the problematic area is between the dashed lines, where 3D radiative effects can occur and would require a 3D emission model. As long as this area (in horizontal 2D) in a footprint is small compared to the pure ocean areas and the pure sea-ice areas, the linear mixing is correct. It is worth noting that even though pancakes, nilas and other thin ice are small in extent (tens of cm), they are also thin vertically (a few cm). Brash ice is certainly the most problematic in this respect.

In the revised manuscript (Sect. 3.6) it is explicitly stated as : “The field-of-view brightness temperature is obtained as the area-weighted linear combination of the point-model outputs (SMRT, Sect. 3.1.1 and PARMIO, Sect. 3.3), which follows directly from the additivity of radiance. Each TB derives from different surface types, and the weights are given by the SIC. This mixing of two 1D model outputs assumes that the vertical extent of the medium (in this context, the sea ice freeboard) is negligible compared to its horizontal extent, and that microwave wavelengths are short relative to any three-dimensional structures present. These assumptions imply that lateral and three-dimensional radiative effects, such as those caused by ice deformation or ridging, are not addressed in this work.”



Regarding spatial heterogeneity, while the analysis does not resolve spatial sectors or sea ice type variations explicitly, the wide range of parameters included in the simulations is designed to represent the diversity of sea ice scenarios encountered across the circumpolar Marginal Ice Zone. To further demonstrate that the simulations can replicate the observed spread also in summer, the AMSR2 observations mask has been expanded to include a broader range of dates, as suggested by the referee.

In the revised version of the manuscript this has been more explicitly stated as mentioned in comment RC1.11

GC4: In the Results, the evaluation framework lacks clarity. Terms such as error, bias, and uncertainty are used inconsistently, and the metric itself is not clearly defined. This creates conceptual ambiguity that affects both the interpretation of results and the framing of the study (i.e. the title). The metric should be clearly defined in the introduction and presented in the methodology.

RC3.4 We acknowledge that the terminology for the evaluation framework requires clarification. The scope of this study is to explore the sensitivity of brightness temperatures to variations in key surface physical parameters and to assess how this sensitivity propagates into uncertainty in sea-ice concentration retrievals.

We changed the title to: "Sensitivity of the Bootstrap sea ice concentration to surface parameters in the Antarctic marginal ice zone using passive microwave retrievals" and adapted the language throughout the manuscript to consistently frame the results in terms of SIC uncertainty, rather than error or bias. A reference snow and ice configuration (which structure is based on literature and AMSR2 reference via forward modelling) is used to simulate the tie points and therefore the brightness temperature corresponding to the 'true SIC'. The uncertainty associated with surface parameter variability is then quantified by perturbing each physical parameter individually in the model, computing the resulting brightness temperatures, and applying the Bootstrap algorithm in frequency mode to retrieve SIC. The deviation of these retrieved SIC values from the reference ‘true SIC’ defines the uncertainty metric used in this study. We decided, in this case, to use the word ‘uncertainty’ and not the word ‘error’ because the ‘true SIC’ is not the one derived from AMSR2 observations.

GC5: Structurally, most of the result figures currently appear in the discussion rather than in the Results section, reducing clarity. Figure 2 and Figure 6 appear to show the same results from the sensitivity analyses, but in a different way. This is rather unclear and should be improved. Furthermore, the sensitivity analysis workflow is complex and scattered between Sec. 3.4 and 3.5.1. The steps are explained twice in two different ways, and this would benefit from a schematic diagram to guide readers.

RC3.5 The placement of the figures has been revised in the manuscript. Regarding Figures 2 and 6, these serve distinct purposes: Figure 2 illustrates the variability of the simulated TB signal in the 37V-19V space, providing an overview of parameter sensitivity without quantifying the impact on SIC retrieval, while Figure 6 represents the actual SIC uncertainty computed using the Bootstrap frequency algorithm for each parameter. The caption of Figure 2 has been updated to clarify that the 10% SIC increments shown are purely a visualisation choice for displaying the brightness temperature signature in the 37V-19V space. The description of the sensitivity analysis methodology has been made more concise: the first two steps, which build on the variability analysis, have been removed from the SIC analysis section. In addition, the section describing the TB variability has been reorganised for improved clarity.

Specific Comments:

Line 50: The manuscript refers to “errors,” while the study is framed as uncertainty based. This inconsistency should be resolved.

RC3.6 The wording has been revised here and throughout the manuscript to consistently use 'uncertainty' in place of 'errors'.

Line 52: Justify the choice of the bootstrap algorithm as opposed to alternative algorithms such as the enhanced NASA Team (NT2).

RC3.7 A justification on the choice of the Bootstrap algorithm in frequency mode (BF) has been added in the text as outlined in comment RC1.1, RC2.3.

Line 55: Vichi. (2022) challenges this “classical” MIZ definition, is this the correct citation for this?

RC3.8 We changed to a more suitable review : Strong & Rigor (2013).

Line 78: Using T_B for both pure ocean and pure sea ice is confusing, use the specific symbols for each. Also, briefly define “pure” presumably used for “only’’.

RC3.9 This has been clarified in the revised manuscript by adding subscripts to TB for the open ocean (OO) and the sea ice (SI) respectively.

Lines 80- 81: Without using dedicated symbols for the pure ocean and sea ice, the symbols here seem to refer a different ocean and sea ice.

RC3.10 The notation here has been made coherent with the one specified in answer RC3.9.

Section 3.1-2: Consider including a simple model architecture diagram with the complete model layering to aid the reader (e.g. Figure 2 in Picard et al. 2018). Importantly, explain the spatiotemporal scales within which the models are commonly applied. They are point-based models 1D models and this needs to be clarified.

RC3.11 To help the reader visualize the snowpack layer configuration we made explicit that the order of the layer is the same as the one shown in Table 1. We clarified in the text that we are employing a 1D model and that this assumption is valid for sea ice because the microwave wavelengths are short relative to the 3D structures in the scene, and sea ice is shallow relative to its horizontal extent, making lateral and 3D effects negligibly small. Multiple 1D simulations are therefore sufficient to represent the scene.
We added this paragraph in Sect.3.6 as in RC3.3.

Lines 123-124: Please include a citation or justification for the definition of thin ice.

RC3.12 We included a clarification in the manuscript as in RC2.7, Section 3.2.

Section 3.3: Please explain why you choose the AMSR-2 product and briefly mention limitations or later in the discussion.

RC3.13 We chose AMSR-2 as it represents a recent and well-established passive microwave sensor. In this study, AMSR-2 brightness temperatures were used only as a background consistency check (see response to comment RC2.14), and the AMSR-2 SIC product was used solely to generate a mask broadly including sea-ice covered areas and the region of open ocean surrounding it. Given this limited and qualitative use (now described in the revised Sect. 3.6), where no accurate absolute SIC values are required, we have not included a detailed discussion of its limitations. Instead, we include a discussion comparing the order of magnitude of the product uncertainty with our simulation results.

Line 142: Include the temporal resolution of the dataset.

RC3.14 We stated the use of the ascending orbit in the revised manuscript: AMSR products provide observed brightness temperatures and estimated sea-ice concentration for two satellite overpasses per day, corresponding to ascending and descending orbits. In this study, we use data from the ascending orbit, which corresponds to an overpass time of approximately 13:30 local solar time in Antarctica.

Lines 143-145: This is confusing. From what boundary is the 30 km measured? The masked boundary? If yes, then areas with <15% SIC are still included despite the initial mask.

RC3.15 We are interested in the ocean-zone brightness temperatures adjacent to the ice pack. In the revised manuscript, the mask has been corrected and updated as described in RC1.17 and RC3.1.

Table 1: Parameter ranges are not clearly linked to their sources. Table 1 should include explicit citations for each parameter, ideally in a dedicated column.

RC3.16 We included the table with the parameters references where possible as shown in RC1.2, RC2.50.

Lines 150-152: Which dates are selected and what is guiding the choice?

RC3.17 The date choice has been specified as in answer RC1.11.

Lines 159-160: The 10% SIC intervals contrasts **Lines 168-170**, where 15 % increments are considered. Clarify why this difference is necessary.

RC3.18 The difference between the two intervals is purely a visualization choice and does not reflect a methodological difference. The 10% SIC intervals in the TB space figure are chosen to best visualize the spread of model outputs in terms of brightness temperature variability. The 15% increments in the SIC retrieval figure are instead selected to represent characteristic SIC values across the MIZ, from the ice edge (SIC–15%) to the consolidated ice pack (SIC–80%), allowing the sensitivity to be assessed across the different MIZ regimes. To clarify that this is a visualization choice, we have removed this detail from the Methods section and clarified it directly in the respective figure captions.

Lines 175-180: The evaluation metric appears to represent an error (difference from a true value), but this is not clearly defined as the target metric beforehand.

RC3.19 In the current manuscript, we define the uncertainty in SIC introduced by each parameter as the deviation of the sensitivity analysis results from the reference simulation, treated as the true SIC (lines 53–54). However, this definition is subsequently applied inconsistently, with the same quantity referred to as 'error'. In the revised manuscript, we corrected this throughout, using 'uncertainty' exclusively, and clarified that we chose this term since we do not compare retrieved SIC against any independent measurement. Further explanation about this choice is in comment RC3.4.

Lines 180-185: Please explain how this partitioning is applied to vary the proportion of thin ice. Since the SMRT is 1D layer model, what does proportion/fraction mean because it is not fraction of a grid cell for example?

RC3.20 The relationship between the 1D layered model outputs and the grid cell observation follows directly from the additivity of radiance. The brightness temperature measured by the radiometer over a grid cell containing a mixture of surface types is the area-weighted combination of the brightness temperatures emitted by each surface type, where the weights are given by the SIC over the grid cell and, in the case of the thin ice sensitivity analysis, by the respective fractions of thin ice within the total SIC. This only neglects the area subject to 3D effects as depicted in the figure above. This has been stated more clearly in the revised manuscript (RC3.11).

Lines 210-220: The same metric described earlier as error is here referred to as bias. This inconsistency in terminology must be corrected.

RC3.21 In the revised manuscript, we adopted consistent terminology throughout, using 'uncertainty' as the standard framing (RC2.1, RC3.19).

Figure 5: This figure is used to conclude that thin ice has some of the largest contribution to the error as stated in the abstract but in the same abstract it is stated that salinity had an unexpected low contribution (**Lines 15-20**), which is not true according to this figure. Please clarify the abstract that salinity in first year ice has low contribution and that the influence of thin ice increases with increasing salinity.

RC3.22 We corrected the abstract accordingly.

Lines 268-271: This is a critical point and must be clarified in the method section because it is relevant to interpreting the results and its omission creates significant confusion. Please see earlier comments about the spatial scales of the models, without this information it is difficult to know how the evaluation is done given the use of point-based models and a gridded satellite product.

Furthermore, it is insufficient to acknowledge the drawback from doing a circumpolar aggregation when there is literature such as Raphael and Hobbs 2014 that highlight the significant Antarctic inter and intra-sectoral variability. Without specific treatment of sectors, the work remains exploratory. A better conceptual framework would be selecting specific points at given locations with desired contrasting conditions and demonstrating the novel evaluation approach for the specific SIC algorithm. Also, the impact of aggregating the satellite dataset is not addressed.

RC3.23 Concerning the spatial scales of the model and their suitability for representing the gridded satellite product, these aspects are addressed in responses RC3.3 and RC3.20.

Despite using a circumpolar aggregation, our methodology accounts for spatial variability through the broad parameter ranges sampled, which were shown to cover the observed TB spread across the MIZ. This approach is consistent with the Bootstrap algorithm, which also determines SIC with circumpolar rather than sector-specific tie points. In addition, the inclusion of multiple ice types characteristic of the Antarctic MIZ – dark nilas, grease ice, and slush – better represents the diversity of surface conditions around the Antarctic sea ice. While our results do not resolve sector-by-sector variability, they capture the range of conditions encountered across the Antarctic MIZ.

References:

Kern, S., Lavergne, T., Notz, D., Pedersen, L.T., Tonboe, R.T., Saldo, R. and Sørensen, A.M., 2019. Satellite passive microwave sea-ice concentration data set intercomparison: closed ice and ship-based observations. *The Cryosphere*, *13*(12), pp.3261-3307.

Picard, G., Sandells, M. and Löwe, H., 2018. SMRT: An active–passive microwave radiative transfer model for snow with multiple microstructure and scattering formulations (v1. 0). *Geoscientific Model Development*, *11*(7), pp.2763-2788.

Raphael, M.N. and Hobbs, W., 2014. The influence of the large-scale atmospheric circulation on Antarctic sea ice during ice advance and retreat seasons. *Geophysical Research Letters*, *41*(14), pp.5037-5045.

Vichi, M., 2022. An indicator of sea ice variability for the Antarctic marginal ice zone. *The Cryosphere*, *16*(10), pp.4087-4106.