**REPORT#1**

Review of

Sensitivity of the Bootstrap sea ice concentration to surface parameters in the Antarctic marginal ice zone using passive microwave retrievals
- Revision 1 -

by M. Stentella et al.

I had the pleasure to read the first version of this manuscript. I applaud the authors for doing such a thorough job in revising the manuscript and taking care of the suggestions made. As a reviewer one cannot get a better reward. Thank you.

*We thank the reviewer for their extensive and constructive engagement with our manuscript throughout the review process, which has substantially improved its quality, and for their positive assessment of the revisions.*

I have nothing to add, no major concerns or specific comments. Remaining issues I list below.

Remaining editoral comments / Typos:
L169: "The windpacked here" --> "The windpacked layer here". See also L183.

We changed this in the manuscript.

Table 1: "Winded snowpack" under column Parameter is "Windpacked snow"?

Yes, we corrected this in the revised manuscript.

Figure 1 caption Line 4: "winded snowpack layer" I suggest to write "windpacked snow layer" to remain consistent. I also believe that "to wind" has a different meaning, no?

We corrected this in the manuscript.

L306: Further up you wrote about the 19V-37V TB-space. Any reason why you switched the order of frequencies here or does this not have any particular meaning.

We made this coherent throughout the manuscript (change in 19V-37V space).

L312: "define" --> "defined"

Changed.

L324: "Despite the slightly wet windpacked" ... This half sentence seems not to be complete ... please check.

We added ‘layer’ → despite the slightly wet windpacked layer.

Figure 2 and 3: Maybe my eyes are quite bad in the meantime but, frankly speaking, when I read your paper and want to see something in the panels of Figs. 2 and 3 I need to zoom to at least 200%. Perhaps it would be worth considering to splitting these figures with many panels such that one grasp much better what is actually displayed.

We have improved the visibility of Figures 2 and 3 by splitting each into two separate figures: one for the SP and DH snowpack layers, and one for the SI and SI_1 layers. We also switched to common axes and moved the subplot labels into the subplot margins, which allowed us to increase the size of the individual subplots.

Caption: Here and in the caption for Fig. 4, 7 and perhaps also other figures: "AMSR-2" --> "AMSR2"

We changed this coherently to AMSR2 throughout the manuscript.

Figure 4, panel b): Again I need at least 200% zoom to be able to distinguish circles from squares.

We have improved the visibility of panel b) by doubling the size of the scatter markers (circles and squares).

L344: "37V-19V plane" --> in the caption of Figure 6 you write "19V-37V space"; I suggest you stick to that name, i.e. change it here in the text.

We changed this coherently to AMSR2 throughout the manuscript.

L365-368: I can understand that you put this paragraph here because it is here where you write about the SIC retrieval ... But refering back to Figure 4 from here seems a bit sub-optimal …

We have changed the placement specifier so that the figure can be positioned more flexibly, closer to the relevant discussion, in the final version of the paper.

L401: "perform" --> "performs"

Corrected.

L463-469: The uncertainty of the NOAA/NSIDC product for one grid cell is the standard deviation of two 3x3 grid-cell boxes computed from the two algorithms that enter the product. It is basically a measure of i) the spatial variability than a physically based uncertainty and ii) a

measure of the bias between the two algorithms that enter the product. I am not so sure I would add this paragraph here.

We removed this paragraph.

L495: I note that you do not get back specifically to the Bootstrap algorithm here but remain more general. Is this on purpose?

This was not intentional. It is indeed a necessary addition to the conclusion, we now added: "This study investigated the sensitivity of the Bootstrap passive microwave sea-ice concentration retrieval to a set of environmental parameters."

**REPORT #2**
Response to rebuttal:

General response:

I would like to commend the authors for their thorough efforts in revising the manuscript and for the detailed responses provided to each referee comment. The manuscript has benefited substantially from these revisions. My primary concerns during the initial review centred on three key areas: (1) the study's focus on SIC uncertainty and the use of inconsistent terminology, (2) the treatment of spatial heterogeneity, and (3) a lack of clarity regarding certain methodological choices. I am pleased to see that these issues, along with several minor concerns, have been addressed satisfactorily.

In light of the revisions made, I recommend the manuscript for publication, subject to minor revisions. Below, I provide a small number of remaining comments for the authors' and editor's consideration. These include both new observations and follow-up remarks related to my original concerns, which are identified using the corresponding rebuttal numbering for ease of reference.

We would like to thank the reviewer once again for their thorough evaluation of our revisions. We are glad that our responses have satisfactorily addressed the concerns raised in the initial review, and we appreciate the reviewer's continued engagement with this manuscript.

**Title:** refocusing the study to a sensitivity study specifically using the bootstrap algorithm has significantly improved the clarity of the study and avoids many of the initial methodological concerns. Kindly consider revising the title to "Sensitivity of the Bootstrap sea ice concentration algorithm to surface parameters in the Antarctic marginal ice zone using passive microwave retrievals" since you're testing the algorithm's sensitivity and not a concentration product derived with the bootstrap.

We changed the title.

**RC3.2:** Thank you for the clarification regarding the increments, I reserve my response to another comment below. On a different note, I find that having different ranges for figures 6-10 for “Retrieved SIC” is confusing, some are 0-100%, some less and some more, why not fix it to 0- 100% ? If it is a visual issue, consider for figures 6-8 for example, setting the legend columns to 4, that way they will likely occupy less vertical space, allowing you to fix the range, if that fails, you can place the legend outside the figure using insert_axes in python. But I'll leave this to the authors to decide. The same trick can be used for figure 10. I'm not sure what restricts figure 9. But regardless of the increments, the range should be the same throughout.

The varying axis ranges were indeed a leftover from an earlier version of the code, where the scale was extended beyond 100% to show possible SIC overestimation without truncating the retrievals. Since the tested range shows this is not the case, we have corrected the representations and fixed the range to 0–100% for Figures 6–10, and adjusted the legend to reduce vertical space, as suggested.

**RC3.3:** Thank you for this clarification. I appreciate the use of a schematic to explain this and fully agree with the authors about the additivity property of the radiance. Indeed, the relative vertical and horizontal extends mean that the lateral fluxes are negligible.

While I am now convinced that the spatial heterogeneity is not a major factor for the study, I believe the authors should acknowledge that spatially aggregating the sectors creates somewhat pseudo conditions that may not be representative of the true Antarctic conditions, I say it is not major because I still believe that the major contribution of this work is providing a novel approach to testing the sensitivity of the SIC retrieval algorithms and not to truly assess it against “real” conditions, doing so requires significant work that can be refined in later studies but these shortcomings should be acknowledged. At the very least it should be said somewhere that Antarctic sea ice varies significantly within the sectors and aggregating them may not represent the true sensitivity. This does not take away from how informative the results are about the probable sensitivity to the broad characteristics in my opinion.

This is now acknowledged in the Discussion section that reads: “ A second limitation of our forward modelling approach is that we assume a single observations background across the entire MIZ, thereby not accounting for regional-scale heterogeneity in the snow, ocean, and sea ice. The tie points are similarly determined circumpolarly, as in the Bootstrap algorithm. This circumpolar aggregation allows us to include a wide range of parameters describing the conditions encountered in the Antarctic MIZ; however, it also means that aggregated conditions do not capture the sector-specific characteristics of Antarctic sea ice (Raphael and Hobbs, 2014). Future development of this work should focus on a finer-scale analysis to capture the changes in the physical properties that could otherwise be concealed or averaged, preventing the actual estimation of SIC. This could include a comparison against true, sector-specific observations.”

**RC3.18:** Alright, this makes sense. I assume that a 15% increment in the TB space is not sufficient then? If yes, then okay, otherwise I still do not understand why for example for figure 4 it would not be better to have the same increments on the left and right columns (i.e. 15%), unless indeed a 15% is insufficient to show the impact due to wind speed on the left column.

We think that using 10% increments in the left column better captures the full variability of Tb along the SIC observation line throughout our simulations, which is why we chose a finer increment there than in the right column.

**RC3.23:** I concede that the inclusion of multiple ice types does represent the diversity of conditions around the Antarctic sea ice. However, I am not convinced that a circumpolar aggregation informs us about the "true" nature of Antarctic sea ice, the focus on the broad characteristics and sensitivity of the bootstrap to these broad features is sufficient for an initial research pathway, extrapolations can be made in later studies so I no longer deem this a major concern, it only needs acknowledgement. I also note that the tie points are done circumpolarly and not sector specific, perhaps commenting this somewhere in the manuscript may be better. Either way, I leave this decision to the authors and editor.

A clarification concerning this point has been added in the Discussion section, as shown in answer RC3.3 above. A further limitation concerning the representation of the 'true' nature of Antarctic sea ice is currently described in the first limitation in the discussion section as well "A first limitation of our analysis is that it treats parameters independently. This conceals potential compensating effects contributing to the SIC uncertainty [...]."

Lastly, I have a general recommendation for figures 2 and 3. Please consider using shared x and y axis, since the subplots display the same variables, only differing in the shaded variable.

We have improved the visibility of Figures 2 and 3 by splitting each into two separate figures: one for the SP and DH snowpack layers, and one for the SI and SI_1 layers. We also switched to common axes and moved the subplot labels into the subplot margins, which allowed us to increase the size of the individual subplots.

For Figures 9 and 11, Please explain why the sea ice does not have liquid water fraction. I would have also expected that the algorithm is sensitive to ice salinity given that it is sensitive to the liquid water fraction, at least as shown in Figures 6 to 8. This is because the salinity is somewhat indicative of liquid water fraction (i.e. high LWF, higher bulk salinity).

In sea ice, liquid water content is physically represented by the brine volume fraction rather than an independent liquid water fraction parameter. Following the Leppäranta & Manninen (1988) formulation, brine volume fraction depends only on temperature and salinity This is why we selected these two variables for the sensitivity tests shown for the sea ice layers, as they fully determine the brine volume.

This is now explicit in the manuscript Sect. 3.1: “Both layers of sea ice use the type "first-year ice" defined in SMRT. The brine volume fraction of the sea ice is not prescribed directly but is computed internally from temperature and salinity following the Leppäranta & Manninen (1988) formulation, based on the Cox & Weeks (1983) coefficients.”

Regarding the limited sensitivity to salinity visible in Figs. 9 and 11: in both the snowpack and the sea ice configurations, the resulting brightness temperature changes only at the first decimal place, which is why it appears in our figures as essentially unvaried. For the snowpack layers and for the sea ice layer this is due to two different reasons.

For the sea ice layers (SI_1), salinity does not affect density, which is prescribed independently and held constant. Its only physical role is to set the brine volume fraction (the scatterer amount), via the empirical Leppäranta & Manninen (1988) relationship, itself based on the Cox & Weeks (1983) coefficients. This calculation does yield different brine volume fraction values (0.07–0.13) across the tested salinity range. However, the sea ice layer is already optically thick over this range, so the resulting brightness temperature is not very affected by this change in brine volume fraction and the corresponding sensitivity of the retrieval to salinity is correspondingly limited within the tested range. On the other hand, for thin ice, the salinity is indeed one of the major sources of uncertainties (Sect. 4.1).

For the snow layers, the limited sensitivity is instead a consequence of the specific reference configuration used. Salinity there only affects the permittivity of the saline water phase; once this is mixed via the Polder–van Santen formula at the reference liquid water fraction of these layers (<3.5%), the resulting change in bulk permittivity is only visible at decimal places further down. In this case, making the effect of salinity more visible would require varying it jointly with the liquid water fraction. However, this would depart from the one-parameter-at-a-time approach used consistently throughout the rest of our sensitivity analysis.

I commend the authors once more for their dedication to improving the manuscript and thank them for the detailed responses and actions taken.