Response to Reviewer1

We thank the reviewer for the thorough and constructive assessment of the manuscript. Following the editor's advice to change this brief communication into a full paper, we have been able to address the reviewer's comments, including expanding the introduction and adding recent citations. The comments have substantially improved clarity, framing, and methodological transparency. Below, we respond to each point individually. Reviewer comments are reproduced in full, followed by our responses in blue.

Major comments

1. [Introduction]:

We thank the reviewer for these constructive and detailed suggestions regarding the Introduction. We have substantially revised this introduction to (i) expand the broader scientific context of Antarctic surface melt, including firn-air depletion, melt ponding, and hydrofracture susceptibility; (ii) introduce the role of the surface energy budget in driving melt and motivating the AWS–SEB calibration used in this study; (iii) highlight the advantages of microwave remote sensing for year-round monitoring of surface and subsurface melt; and (iv) incorporate additional and more recent relevant literature, including optical-satellite estimates of melt volume. In addition, the paragraph on passive-microwave interpretation has been fully rephrased to avoid similarity with de Roda Husman et al. (2022). These revisions collectively strengthen the motivation, framing, and novelty of the study.

2. I have three main comments regarding Section 3.1 of the manuscript. First, I think the overall purpose of the method application and analysis could be made clearer by a slight adjustment to the terminology used. I originally thought that this section would assess whether the SSMIS pixels containing the AWSs correctly identified the presence of melt for the same days that the AWSs identified melt, as "accurately" implies comparison against a true value. Therefore, for this section I offer the following suggestions:

- Adjust L105 to something along the lines of "To assess whether the 6.25 km x 6.25 km resolution of an SSMIS pixel is sufficient to represent melt conditions at each AWS site…"
- So that it is clear which dataset (SSMIS or UMelt) is being used, adjust L107 to "For each station, an 11 x 11 grid of UMelt pixels was centred over each AWS location…"
- For greater clarity, adjust L112-114 to "These results indicate that around each AWS, the nature of melt conditions is highly homogenous at a scale similar to that of the SSMIS pixel footprint. Therefore, the 6.25 km x 6.25 km resolution of the SSMIS pixel is sufficient to represent local melt conditions and thus is appropriate for calibration purposes."

We thank the reviewer for these helpful suggestions. We have implemented the proposed wording changes in this section, namely: (i) clarifying that the purpose of the analysis is to assess whether the 6.25 km × 6.25 km resolution of an SSMIS pixel is sufficient to represent melt conditions at each AWS site; (ii) explicitly stating that a grid of UMelt pixels is centred over each AWS location; and (iii) rephrasing the conclusion to emphasise that melt conditions are highly homogeneous at the scale of the SSMIS footprint and that this resolution is appropriate for calibration.

Second, it is not clear how each UMelt grid was selected, nor how many were analysed through this process. Was this carried out for one grid per AWS or multiple grids per AWS? How was each respective grid chosen? I'm assuming they were only selected for days when melt was observed, but I think it would be good to state this. A short explanation addressing the above questions would be a good addition to this section.

Finally, I am interested to know why an 11 x 11 grid of UMelt pixels was chosen? If my interpretation of your methodology is correct, using a 13 x 13 grid of 500 m x 500 m pixels would produce an overall footprint of 42.25 km2 which: i) is much closer in size to the ~40 km2 footprint of an SSMIS pixel compared to the 30.25 km2 footprint of an 11 x 11 grid, and ii) has a footprint marginally larger than the SSMIS pixel, such that the SSMIS pixel is fully contained within the UMelt grid. Currently, the melt homogeneity evaluated over the 30.25 km2 (11 x 11) UMelt footprint is reflective of only ~75% of the SSMIS pixel, whereas using a 42.25 km2 UMelt footprint (13 x 13 grid) would enable an evaluation of homogeneity rate and local variability across 100% of the SSMIS footprint. If the homogeneity rate and local variability metrics were computed over a 13 x 13 grid, and produced very similar results, then the conclusion of L112-114 could be strengthened even further, e.g., "These results indicate that around each AWS, the nature of melt conditions is highly homogenous at a scale greater than that of the SSMIS pixel footprint. Therefore, the 6.25 km x 6.25 km resolution of the SSMIS pixel is sufficient to represent local melt conditions and thus is appropriate for calibration purposes." As the purpose of this section is to evaluate whether the enhanced resolution of the SSMIS pixel is sufficient, even if these metrics decrease, I think it is important to provide a quantification of melt homogeneity over the entire SSMIS pixel footprint.

Thank you for this helpful comment. We clarified in the manuscript [sec3.1] how the U-Melt grids were selected. For each AWS, a single U-Melt window centred on the station location was used, and melt homogeneity was evaluated for all days in the record (i.e., both melt and non-melt days), rather than only for melt days.

Following your suggestion, we repeated the analysis using a 13 × 13 U-Melt window to more closely match the SSMIS pixel footprint. The results changed only marginally (homogeneity >98%, variability <0.02), confirming that melt conditions are highly homogeneous at the scale of a full SSMIS pixel. The manuscript has been updated accordingly.

3. From my interpretation of the ROC analysis (Figure S1), it appears the two best-performing metrics stated on L132 have been incorrectly identified. Instead of diurnal amplitude, this should be winter anomaly. This error doesn't have subsequent implications for the following stages of analysis, but L132 should be corrected.

We thank the reviewer for catching this oversight. Indeed, the winter anomaly, rather than the diurnal amplitude, is the second-best performing metric in the ROC analysis (Fig. S2). This was a wording error only and does not affect any subsequent results or interpretation. The sentence on L160 has been corrected accordingly.

In addition, four of the 'Area Under the Curve' (AUC) statistics shown on Figure S1 are reported as negative, which is not possible (an area cannot be negative). This likely results from plotting the False Positive Rate (FPR) in descending rather than ascending order, causing the integration for AUC to yield a negative value. However, the magnitude of AUC will be correct, and therefore the analysis and interpretation of Figure S1 does not need to be repeated, but the relevant plots should be edited to remove the negative sign.

Thank you for pointing this out. We have corrected the plotting routine so that all AUC values are now reported as positive. This change affects only the sign of the plotted AUC values; the magnitudes remain unchanged. The updated figure has been included in the revised manuscript (Fig S2)

In light of the above, I would also suggest reordering the indicators in L141 and in the following lines (L142-147) so that winter anomaly comes before diurnal amplitude.

Changed accordingly

Minor comments

Throughout the manuscript, the RACMO2.4 dataset used within this study is sometimes referred to as 'RACMO', e.g., L8 or 'RACMO2.4p1' e.g., L197. For continuity and clarity, I would suggest either consistently using 'RACMO2.4' throughout, including within figures, or after the first use of 'RACMO2.4' follow with "hereafter referred to as RACMO".

L30: The acronym 'AWS' has already been established earlier in the manuscript and so "automatic weather station (AWS)" does not need to be written again in full here. Check this here and elsewhere in the manuscript.

Revised

L33-34: Would be good here to give spatial resolution and temporal coverage of Banwell et al.'s (2023) dataset.

Added

L35-44: The methodology of this study is introduced but there is no mention of the resulting dataset which I think should be included here.

Revised [L51]

L50: If AWS data availability is the limiting factor on the temporal coverage of this study's dataset, I think this
point needs to be made more explicitly. Surely these AWSs have data available to the present day?
Thank you for the helpful observation. We agree that AWS records extend beyond 2021, and AWS availability is not the limiting factor. The actual constraint is the temporal stability of DMSP-F17 SSMIS observations. We have now clarified this in the manuscript [L67/68]

L51: The manuscript states that the temporal coverage of the dataset is "2012-2021". However, here, the authors note June 2011 – May 2021 as the respective start and end dates of the ten hydrological year-period for which the dataset is produced. Given that the dataset offers annual surface melt rates for the hydrological year 2011-2012 (as shown in first panel of Figure S5), would reporting the temporal coverage of the dataset as "2011-2021" be more inclusive of the actual temporal range? It would also clearly show from a quick glance that it's a decadal dataset.

We agree with the reviewer and have updated the reported temporal coverage to "2011–2021", which accurately reflects the ten hydrological years included (2011–12 through 2020–21).[L66]

L56-58: As the SSMIS sensor offers both a standard and an enhanced resolution dataset at 19 GHz frequency, the manuscript should state that the enhanced resolution dataset is being utilised and should use the relevant citation for this (Brodzik et al. (2024): https://nsidc.org/data/nsidc-0630/versions/2)

Added [L75]

L57: In the interest of accessibility for all audiences, the manuscript could benefit from a clearer justification for the focused use of the 19 GHz H channel (assuming this was selected due to the general acknowledgement of this channel as having the lowest brightness temp over dry firn?)

Added [L76]

L58: As with the 19 GHz channel, the 37 and 91 GHz frequencies also offer standard (25 km) and enhanced resolutions (3.125 km), though it is not clear which resolution is utilised here? It would also be helpful to provide an estimate of penetration depth for each of the three channels. This could provide further context to the authors' statement that utilising the 37 and 91 GHz frequencies didn't notably enhance melt detection; i.e., that most of the detected melt occurs within the sub/near-surface layers of the snowpack. This edit could be addressed at this stage of the manuscript or earlier in the introduction (L16-17).

We thank the reviewer for this helpful comment. We have clarified in the manuscript which spatial resolution was used for the 37 and 91 GHz channels [L78/80]. As noted, higher-frequency channels are characterised by shallower penetration depths (Colliander et al., 2022), However, we emphasise that we are not able to attribute this outcome with certainty, as we did not perform an in-depth investigation of the physical or instrumental causes beyond the comparative skill assessment.

L57-59: Horizontal and vertically polarised channels are referred to a lot throughout manuscript and in equations. Introduce the acronyms 'H' and 'V' here and then use consistently throughout.
Revised

L66-68: For readers less familiar with AWSs, it would be helpful to give the region in which they are located either in their respective brackets, e.g., "AWS14 (northern Larsen C Ice Shelf, Antarctic Peninsula)" or introduce them by region, e.g., "Four AWSs from the Antarctic Peninsula (AWS14, ...) and three from Dronning Maud Land, East Antarctica (AWS11, AWS16, and Neumayer)...". A useful addition to this manuscript would be a supplementary Figure showing the exact locations of each station. This would give a reader a quick sense of their location and spatial distribution across the Peninsula and Dronning Maud Land.
A map of AWSs locations has been added in the supplementary document (Fig. S1)

L73: Adding "into the subsurface layers of the snowpack" here would increase clarity.
Revised L94

L76-78: In line with my above comment, give the location of Neumayer station (Ekström Ice Shelf, Dronning Maud Land, East Antarctica) or illustrate its location in a Figure. Here, the authors include Neumayer station with the purpose of broadening the geographic scope for their model calibration to ensure that it performs robustly across climatically distinct regions. Given that four of the AWSs are located on the Peninsula and the other two stations are also located in Dronning Maud Land, East Antarctica, would the inclusion of station data from further afield, such as the West Antarctic Ice Sheet, offer a more comprehensive addition of data to this study? If this is not possible, an explanation of how the inclusion of Neumayer station, in addition to AWS11 and AWS16 (also located in Dronning Maud Land), provides data from a broader geographic scope and climatically distinct region would support these statements.
We thank the reviewer for raising this point. Unfortunately, no additional AWS sites elsewhere in Antarctica provide the full set of SEB-quality observations required to compute melt consistently with our IMAU AWS stations. This precludes extending the calibration network into other major regions such as the West Antarctic Ice Sheet.
Regarding Neumayer station, although its melt regime is indeed similarly low to that of AWS11 and AWS16, the key distinction is data availability: AWS11 and AWS16 provide only a few usable years within our analysis period, whereas Neumayer offers a complete and continuous decade of SEB-quality observations. This longer and uninterrupted

record makes Neumayer essential for broadening the calibration beyond the climatology of the Antarctic Peninsula, adding a well-sampled coastal East Antarctic environment to the dataset. We have revised the manuscript to clarify this rationale [L97/101]

L89-92: I find this section a little confusing and hard to follow. Here are my suggested edits: "To constrain the uncertainty associated with the Then, the uncertainty due to the SEB model, settings and assumptions is estimated by separately varying one each of the five model settings were individually adjusted at the time: i) sensor using a constant height was fixed of the sensors to at 2 m above the surface instead of variable in time, ii) the use of a momentum roughness length was increased from 0.1 mm (typical for snow) to 1 mm for momentum of 1 mm instead of 0.1 mm for snow, iii) using a surface longwave emissivity was decreased from 1 to 0.97 of 0.97 instead of 1, iv) using an alternative snow thermal conductivity was used (Anderson, 1976), and finally v) letting the instead of nudging snow height with sonic height ranger observations, it was allowed to freely evolve in the model instead of prescribing snow height in time using the sonic height ranger observations." For point iv), I think this remains too vague -- what alternative was used? For the earlier list of measurement corrections (L85-87), I would recommend also using the i), ii), iii) ... notation, as above, for better readability.
Thanks for the suggested edits, readability of the section has been improved accordingly.

L95: Why were sensitivity tests not carried out for all stations? Pointing the reader here to section 3.3 where the sensitivity tests are incorporated and discussed would be beneficial.
Discussion added [L118/120]

L102-103 repeats line 105. Remove L102-103.
Removed

L117: Is 0.5 mm w.e. day-1 the threshold for identifying a "surface melt day" at the AWSs? If so, I think this could be explicitly stated and justified i.e., why is it not just any day where melt rate is > 0 mm w.e. day-1? Is ± 0.5 mm w.e. the resolution of the AWS measurements?
Thank you for the comment. Yes, 0.5 mm w.e. day $^{-1}$ is the threshold used to label an AWS day as "surface melt." We added this clarification to the manuscript [L141]. This value was selected to avoid classifying very small, near-zero SEB melt outputs as melt days, since melt is a model-diagnosed quantity rather than a directly measured variable. Near-zero melt values ($\lesssim$0.5 mm w.e. day $^{-1}$) can arise from energy-balance uncertainty, sensor corrections, or rounding in the SEB solution, and sensitivity tests showed that including such values increases noise without altering seasonal melt totals or calibration results. While no community-standard threshold exists for Antarctica, we chose 0.5 mm w.e. day $^{-1}$ that would provide a conservative lower bound that separates physically meaningful melt from numerical noise

L124: Is there an appropriate reference that could be cited for the Day-to-Day change? Maybe Wang et al.(2025) (https://tc.copernicus.org/articles/10/2589/2016/tc-10-2589-2016.pdf)? Though I appreciate they compare daily Tb values to a previous-3-day average.
We now cite Wang et al. (2016), who use short-term day-to-day variability in brightness temperature for melt detection, which is conceptually aligned with our "day-to-day change" indicator. The citation has been added accordingly.

L125: As Abdalati and Steffen (1997) use a cross polarised gradient ratio where both frequency and polarization is evaluated, could a study that uses the normalized polarization ratio be more appropriate? e.g., Mousavi et al. (2021) (https://dspace.mit.edu/handle/1721.1/148558)

We thank the reviewer for this helpful suggestion. We agree that citing a study explicitly using the normalized polarization ratio is more appropriate in this context.

L129: Over what time period was this comparison made and, in line with my earlier comment, is AWS-derived melt vs non-melt classified using 0.5 mm w.e. day-1 as an absolute threshold?
The comparison was performed over the full period of AWS-SEB melt availability used in our calibration (hydrological years 2011–2021). Consistent with the calibration procedure, AWS days were classified as melt when daily melt exceeded 0.5 mm w.e. day $^{-1}$, ensuring that both AWS and SSMIS evaluations use the same melt/non-melt threshold.

L157: As there are two sets of annual melt-day counts (SSMIS-derived and AWS-derived), for greater clarity be clear which set is being referred to – e.g., “SSMIS-derived annual melt-day counts are obtained by…”. Likewise, when referring to “observations” e.g., L81, make it clear these are AWS observations.
Revised

For Figure 2, it would be good to report an R2 value for both Figure 2a and 2b to quantify the fit of the data. I suggest labelling each respective y axis as “AWS/RACMO Annual Melt Volume [mm w.e./year]” for clarity.
Thank you for the helpful suggestion. We have now added the corresponding R^2 values for both panels directly in the caption of Figure 2. Axis labels have also been updated for clarity.

L203-205: It would be nice to visually see these results either as a closer view inset on Figure 3 or as an additional Figure in supplementary materials.
Thank you for the suggestion. We agree that regional visualisation can be informative, and this is now provided through the annual Antarctic Peninsula melt-flux maps in Fig. S6 of the Supplement. Because these figures already resolve the spatial patterns discussed in Sect. 4.2, adding an additional inset or figure would introduce redundancy without substantially improving interpretation. We therefore opted to retain the current figure layout for clarity and conciseness.

Here and throughout the results section, be clear about the exact statistics that are being reported, i.e., L204: “melt rates exceeded 350 mm w.e. yr-1… show their highest values on the western inlets…” – are these annual melt rates? decadal mean? highest mean values? For L210 (and elsewhere).“annual surface melt at Roi Baudouin”.be clear that these are surface melt rates.
L203-214: Consider presenting your results organised by region (Antarctic Peninsula, West Antarctica, East Antarctica). This will make them easier for the reader to digest. I think it would also be interesting to give regional decadal mean melt fluxes for the Peninsula, WAIS, and EAIS.
L212: Is this region of low-intensity melting a continent-wide minimum for Antarctica or just an area of low melt rates?
Thank you for the suggestions. We have reorganized the Results section into a clearer regional narrative (Peninsula, West Antarctica, East Antarctica) and clarified the temporal metrics throughout, explicitly distinguishing decadal-mean and annual melt values.

L224: It is not clear here which products the melt classification is being evaluated for, and therefore what is considered as a false positive?
We clarified that the misclassification analysis refers to the evaluation of the SSMIS melt classifier against AWS-derived SEB melt [L253]

L226-231: Surely these discussion points could be reframed with more confidence in the argument presented? "These findings suggest that our classifier is not only responding to surface melt events, but more generally detects the presence of liquid water near the surface..."

The ability of SSMIS to penetrate and detect near/subsurface melt in the snowpack is already acknowledged by the authors early on in the manuscript (L23-24).

"In this sense, SSMIS appears sensitive to a broader melt signal spectrum, including processes not directly measurable by AWS but captured by RACMO's subsurface hydrology." Is SSMIS not already acknowledged for having a broader melt signal given the ability for lower frequency channels to penetrate deeper into the snowpack? It would be interesting to comment on the implications of neglecting shortwave penetration into the snowpack during the AWS SEB modelling (L73).

We thank the reviewer for this helpful suggestion. We have revised the paragraph to clarify what constitutes a false positive, and we now describe the behaviour of the classifier in a more confident but still cautious way. The revised text emphasises that SSMIS is expected to detect liquid water within the upper firn, consistent with its penetration depth, and that such wetting may not always be diagnosed by AWS-SEB. These clarifications strengthen the interpretation while maintaining physical consistency.[255-263]

L241: This recent preprint by Zou et al. might be of interest here – https://doi.org/10.21203/rs.3.rs-7384193/v1

L245: Turner et al. (2016) identifies the cooling trend from 1998. I also think it would be worth mentioning the cooling trend was associated with decadal-scale natural variability.

Revised L281

L258: Here and elsewhere, the surface melt rate dataset is referred to as "satellite-only/ derived exclusively from SSMIS...". As this study relies not only on satellite-derived brightness temperatures but also AWS observations – as stated by the authors on L46 – I would consider rephrasing from "satellite-only".

We agree and replaced "satellite-only" with the more accurate wording "SSMIS-derived, AWS-calibrated melt dataset."

Figure S5: Include in figure caption that these are regional maps of the Antarctic Peninsula.

Revised

- *Technical corrections have been all revised.*

We sincerely thank the reviewer for the detailed, thoughtful, and constructive feedback. The manuscript has improved substantially in clarity, contextual framing, methodological transparency, and interpretive confidence. We believe the revised manuscript now more clearly communicates the novelty and robustness of the work.

Response to Reviewer2

We thank Reviewer2 for their thoughtful and constructive assessment of the manuscript. Following the editor's advice to change this brief communication into a full paper, we have been able to address the reviewers' comments, including revision and expansion of both introduction and discussions. We appreciate the careful reading and targeted suggestions, which have helped improve the clarity, transparency, and contextual framing of the study. All comments have been addressed in detail below (blue), and corresponding revisions have been implemented in the manuscript.

Main Points 1. SSMIS data cover the whole of Antarctica, making it possible to generate an Antarctic-wide melt product. However, calibration data are only available for the six AWSs and Neumayer Station. Furthermore, four of the AWSs are located on the Larsen Ice Shelf. The calibration data are thus quite geographically restricted, which begs the question of whether parametrisations derived using these data will be valid across all of the Antarctic melt zone. I think that there are indications that this may be true (e.g., figure S3(c), which, I think, provides model-based evidence that just using these sits for calibration does not introduce major biases) but I'd like to see a bit more discussion of the possible uncertainties resulting from limited calibration data.

We agree that the limited geographical distribution of AWS calibration data introduces potential uncertainty. We have added a dedicated discussion noting this limitation and clarifying that the strong agreement between the AWS-derived and RACMO-derived melt-day–melt-volume curves suggests good general applicability, while acknowledging remaining uncertainties in poorly sampled regions. The new text has been added in the Discussion (L261–270).

2. In section 4.2 you discuss the spatial structure of the melt field in the Antarctic Peninsula but it is very difficult to see at the scale of the pan-Antarctic maps presented in figure 3. Given that most melt occurs in the Peninsula region (and that the majority of your calibration data come from this region) I'd consider presenting separate larger-scale maps covering just the Peninsula region.

Thank you for this suggestion. We agree that regional detail over the Antarctic Peninsula is essential given that most melt occurs there. We note that the Supplementary Material includes annual Peninsula-scale maps (Fig. S5), and we have updated the figure caption to explicitly state that these are regional maps. We now also reference Fig. S5 directly in the Results section to guide readers to the higher-resolution regional view.(L242/243)

3. Not all readers will be familiar with the locations of the AWSs. I think that a location map would be useful.

A map of AWSs locations has been added in the supplementary document (Fig. S1)

Minor Points (technical suggestions) have been all revised.

We are grateful for the reviewer's insightful feedback, which strengthened the manuscript scientifically and editorially. We believe the revisions address all concerns raised. We hope the updated manuscript now meets the reviewer's expectations and we thank them again for their constructive contribution.