Dear Dr. Tuckett,

Thank you for your positive assessment and recommendation. We greatly appreciate the time you have taken to review the manuscript. Your feedback, together with that of the second reviewer, has helped us significantly strengthen our manuscript.

Kind regards,

Dr. Emily Glen
on behalf of all co-authors

Dear Dr. Vandecrux,

Thank you for your second review. We respond to each comment in full below, with your original text reproduced in black and our responses in blue. All sections and line numbers refer to the revised version of the manuscript.

Thank you for your revision of the manuscript, and apologies for my delayed response. I appreciate the efforts put into the revision. The manuscript has significantly improved, particularly in the presentation of the slush area partition between bare ice, ice slabs, firn aquifers, and other firn areas, and in the first-order quantification of the radiative impact of slush fields.

Thank you for recognising the improvements made to the manuscript. We greatly appreciate the time and guidance you have provided throughout the revision process. Your feedback, together with that of the first reviewer, has helped us significantly strengthen the manuscript.

However, I feel that one of my core concerns was not properly addressed: how slush is identified and labelled in the training data. Figure S1 is illustrative, but it confirms what I had suspected.

The clustering algorithm classifies pixels into clusters, yet it remains unclear what the distinct physical meaning of each cluster is, and on what basis the expert decided to label one cluster as slush and another as non-slush. For instance, in Figure S1d,e,f (reproduced below), the upper- left part of the scene appears to be a large slush field with varying shades of blue. More specifically, the area within the black rectangle appears to me as predominantly slush. Yet the clustering algorithm allocates many pixels in that region (shown in red and orange in S1e) to clusters later categorized as non-slush (turquoise pixels in S1f). I understand that some clusters are unambiguously white snow and others are clearly open water, but the number of non-slush pixels in the upper-left area of Figure S1d,e,f raises questions about the uncertainty introduced by the inclusion or exclusion of certain clusters from the "slush" label. In other words, the manuscript still does not clearly illustrate where, and on what quantitative basis, the expert observer draws the line between slush and non-slush.

I recognize that this cannot be easily fixed, as the labelling was done long ago and the RF classifier has since been trained on these labels. I therefore suggest the following additions:

- A zoomed-in version of Figure S1d,e,f that illustrates this limitation explicitly.
- A statement — potentially in the discussion — acknowledging that due to the single-expert labelling approach and the absence of a "transitional slush/non-slush" label that would reflect the natural ambiguity an observer faces when categorizing a continuous process, the training labels carry an inherent and unquantified uncertainty.
- An ad hoc analysis of the training labels to improve reproducibility: specifically, what range of "blueness" ended up being classified as slush (if the observer assignmed clusters based on visual inspection of the RGB imagery). Blueness could be quantified using standard metrics based on saturation and hue, yielding threshold values (± some measure of spread) separating dry snow from slush and slush from open water. This would make the labelling process more transparent and reproducible.

Thank you for raising this point and for your guidance on how to address it. In response, we have made the three additions you suggested. We agree that these changes make the training labelling process more transparent and reproducible.

First, we have updated Supplementary Figure S1 to include a zoomed-in view of the image, showing the true-colour imagery, the corresponding spectral clusters, and their final assignment to the slush and non-slush classes.

Second, we have added a statement to Lines 205–210 acknowledging that the training labels were assigned by a single expert using only binary slush and non-slush classes, and therefore contain an inherent and unquantified uncertainty, particularly for transitional surfaces.

Third, we have carried out the suggested ad hoc analysis to quantify the spectral characteristics of the training labels. We refer to this analysis in the main manuscript at Lines 210–213 and provide the full methods and results in a new Supplementary Section S1 and Table S2.

Additionally, the uncertainty in cluster assignment highlighted by this comment prompted us to revisit the classifier evaluation and our broader treatment of uncertainty in mapped slush area. We have updated the reported performance statistics and replaced the previous confusion-matrix-based area uncertainty estimate with a more robust, independent assessment of uncertainty in the mapped outputs, now in Section 2.5.1.

In addition to these concerns, I have a few minor comments listed below. Please correct me if any of these points are already addressed in the text.

I am confident that addressing the comments above would bring the manuscript to the highest standards and make it suitable for publication, at which point I would have no further objections to acceptance.

Thank you for your positive assessment of the manuscript. We have addressed all of the comments raised above, together with the minor comments listed below, in the revised version of the manuscript.

page 12 l.*73 "slush occurring outside the firn zone": this phrasing appears in several places. I would suggest replacing it with "slush occurring in the bare ice area," which directly states where it occurs rather than where it does not.

We have replaced "slush occurring outside the firn zone" and similar phrasing throughout the manuscript with "slush occurring in the bare ice area".

page 14 l.*07 "Fig 4b" should it be 4d ?

Yes – this has been corrected.

page 19 l.*73-*81 This would be better in the methods section.

We have moved this material to the Methods section (Section 2.7).

Figure S6 and S9 — If FAC is taken as the basin-average across all pixels up to the ice divide, its value will be dominated by the large number of pixels in the upper accumulation area, where FAC changes relatively slowly. This likely dampens the apparent interannual variability and weakens the correlation. The correlation would probably strengthen if FAC were computed over an area more representative of potential slush formation. This analysis also does not reveal whether local

FAC values either promote or inhibit slush formation above or below a certain threshold. An alternative, pixel-level framing may be more informative: for all slush pixels, what are the spring-time FAC values and accumulated snowmelt that precede slush formation?

Similarly, I would expect stronger correlations between slush extent and snowmelt, snowfall, or air temperature when these variables are evaluated within the area that can potentially turn into slush, rather than basin-wide. Beyond improving correlations, such an approach could allow you to establish a quantitative relationship between melt (or temperature) and slush extent.

Combined with your new radiative impact estimates, an exponential response of slush extent to future warming would make for a compelling result.

We agree that averaging predictor variables across entire drainage basins may obscure variability within the areas in which slush can form. We therefore repeated the interannual and intra-annual analyses using FAC and climatic variables averaged only across the potential slush zone within each basin, defined as the union of all mapped slush between 2016 and 2024.

We also added a RACMO-grid-cell analysis comparing March FAC and accumulated April-to-June snowmelt between grid-cell years with and without mapped slush, together with a standardised binary logistic regression. Slush occurrence was associated with lower March FAC and greater spring snowmelt, but the distributions overlapped and no distinct threshold was identified. These results are presented at lines 455–463.

We did not fit an exponential relationship with temperature because the observational record contains only nine annual values per basin and does not provide sufficient evidence to constrain a future warming response.

page 25 l.*43-*49: Please move the description from the supplementary material into the main text and present it concisely. Put the 134 PJ d^{-1} figure in context: if this energy were used to melt ice, how much additional melt would it represent? What fraction of the ice sheet's total mass deficit does this correspond to?

We have moved a concise description of the radiative calculation from the Supplementary Material into the Methods (Section 2.8), Results (Section 3.5), and Discussion (Section 4.5) of the main manuscript. We have also placed the estimated additional energy absorption in the context of its potential contribution to ice melt and the total GrIS mass deficit (Lines 629–639).

page 25 l.*52-*57: I found this analysis difficult to interpret as currently presented. As I understand it, the RACMO pixels located within the mapped slush area account for 28% of modelled runoff while representing only 5% of all ice sheet pixels. However, this seems like a direct consequence of runoff being naturally concentrated in the low-elevation marginal band: the slush areas you map are by definition within the runoff zone, which is where melt is highest. I would suggest condensing this analysis and integrating it with the one at l.*43–*49, along the following lines: "Within RACMO's runoff area, slush covers X% of pixels. The runoff currently modelled from these pixels is Y Gt, but could increase to Z Gt if the additional energy absorbed by slush fields due to surface darkening were fully used for ice melt." It should also be noted that regional climate models currently struggle to accurately represent the spatial extent of the runoff zone (https://doi.org/10.5194/tc-20-427-2026).

We have condensed this analysis, restricted the comparison to RACMO's modelled runoff area, and integrated it with the radiative analysis. The condensed runoff-conversion result is reported at Lines 468–472 (results) and discussed at Lines 629–632 (Section 4.5). We have also cited Machguth et al. (2026).

Kind regards,

Dr. Emily Glen
on behalf of all co-authors

References added:

Machguth, H., Tedstone, A., Kuipers Munneke, P., Brils, M., Noël, B., Clerx, N., Jullien, N., Fettweis, X., and van den Broeke, M.: Runoff from Greenland's firn area – why do MODIS, RCMs and a firn model disagree?, The Cryosphere, 20, 427–452, https://doi.org/10.5194/tc-20-427-2026, 2026.