Dear Dr. Vandecrux,

Thank you for your review. We respond to each comment in full below, with your original text reproduced in black and our responses in blue.

The manuscript presents and analyses nine years of slush mapping based on annual and monthly (May to September) Sentinel-2 mosaics. The resulting dataset is very valuable, and the manuscript is clearly written, well illustrated, and suitable for publication in The Cryosphere. My only major concerns are as follows.

We thank the reviewer for this positive assessment of our dataset and the manuscript.

1. Although the uncertainty of the classification algorithm is currently provided, it is unclear how this uncertainty scales when the algorithm is applied to the entire ice sheet. Beyond the uncertainty quantification itself, all results are presented without uncertainty ranges, making it difficult to assess whether reported trends and interannual differences exceed the algorithm's intrinsic uncertainty. I wonder whether the random forest (RF) classifier outputs class probabilities that could be used to derive upper and lower bounds on slush extent, and potentially an uncertainty metric to be distributed with the dataset.

   We agree that the presentation of uncertainty alongside the reported results is important for interpreting interannual variability and trends. We will incorporate uncertainty estimates derived from the RF classifier. Specifically, mapped slush area will be stated to have an associated estimated uncertainty of +1.3/−0.2%, based on classifier performance metrics. Uncertainty ranges will be reported alongside all slush area values in the revised Results.

   I am also not fully convinced by the use of a K-means clustering algorithm to segment slush and non-slush areas during the training and validation process. To my understanding, unsupervised classification groups pixels based on spectral characteristics rather than physical meaning. Depending on the scene, the sharpness of the spectral difference (and transition) between slush and non-slush, and the image-specific number of classes, there is no guarantee that clusters will align with physical boundaries. I assume that the manual labeling step also revealed some ambiguity, with some slush clusters matching the visible slush extent better than others depending on the image. These ambiguities in the labeling and the manual curation process that follows therefore appear insufficiently documented: for example, where is the boundary drawn between slush and channels within slush areas? This combination of unsupervised classification and undocumented manual correction currently limits confidence in the uncertainty assessment. Providing illustrative examples of the workflow, including class maps and labeled images in the supplement, would help build that confidence.

While we agree that unsupervised clustering does not inherently correspond to physically meaningful classes, we note that the use of K-means clustering as an intermediate step to support manual labeling is consistent with previous studies, including Dell et al. (2022, 2024) and Halberstadt et al. (2020). We will revise the Methods section to clarify that K-means clustering is used to guide the identification of spectrally distinct regions, rather than to directly define physical classes. In practice, clusters are interpreted in the context of the underlying imagery and subsequently grouped into physically meaningful classes (e.g., slush and non-slush) through manual inspection.

We will also revise the manuscript to clarify that this stage was conducted by a single expert analyst, consistent with Dell et al. (2022). We acknowledge that the distinction between slush and ponded water is inherently subjective, as boundaries between these classes are not always well defined, and this will be explicitly recognised in the revised manuscript. To further improve transparency, we will include illustrative examples of the workflow in the Supplement to demonstrate how ambiguous cases are handled in practice.

> Finally, comparing expert delineations of “uncertain slush” areas with the RF-derived uncertainty metric would help demonstrate that the proposed uncertainty quantification is realistic. These points are intended as suggestions, and alternative solutions consistent with the authors’ methodology are of course welcome.

We will consider incorporating an additional supplementary figure, or similar supporting material, consistent with our existing methodology.

2. The analysis of the slush maps is limited to their spatio-temporal patterns (with some repetition between regional and ice-sheet-wide behavior) and rather superficial comparison with RACMO estimates of melt and air temperature. Deriving additional insights on the formation of slush and/or impact of the slush on the energy balance, would greatly increase the manuscript's relevance. For instance, a potentially valuable analysis would be to study the role of surface topography and annual snowfall in controlling slush formation and its persistence from year to year. It could also support more informed discussion of future evolution: for example, whether slush can form everywhere on the ice sheet, and whether increased melt could shift its occurrence to higher elevations. Additionally, although the impact of slush on surface albedo and energy uptake is not directly quantified, it could be discussed using previously published work scaled to the slush extents derived here.

We agree that, in its current form, the analysis is primarily descriptive and that further process-based interpretation would strengthen the manuscript and its broader relevance.

We will revise the manuscript to incorporate additional analyses aimed at improving mechanistic understanding of slush formation and persistence. In particular, we will assess the role of firn air content from IMAU FDM, snowfall accumulation and using RACMO outputs, and consider how previously published albedo-energy balance frameworks may be applied to the slush extents derived here. We will also consider topographic controls using the ArcticDEM. These analyses will allow us to better constrain the factors governing where slush forms and persists across the ice sheet, and to move beyond purely spatio-temporal description. Please also see our response to a similar comment made by Reviewer 1.

Addressing these points, together with the minor comments below, would improve the robustness and depth of the study and make it a valuable contribution to The Cryosphere.

We thank the reviewer for recognising the value of our work, and will address the points raised.

l.19 "maximum" -> "minimum"?

Maximum is correct, as maximum slush areal extent is the metric used (which we will ensure is clearer)

l.46 missing parenthesis after "Machguth et al., 2022" / l.47 missing period after "laterally"

Both typographical errors will be corrected.

l.127–130 How does the use of the "high-end" ice slab extent impact the statistics presented later on? Why use this instead of a more reasonable mid-range estimate?

We will revise the manuscript to include both high- and low- end estimates of ice slab extent.

l.253 "trend": do you mean temporal trend? Please check for other instances where "temporal" might be missing before "trend".

We confirm that ‘trend’ refers to temporal trend, and this will be clarified throughout the manuscript.

l.319–320 This statement seems counterintuitive. One would expect the slush area to be located at lower elevation in the northern regions. Could you elaborate?

We agree that this statement is unclear and does not add substantial value. It will be removed from the revised manuscript.

l.365–370 SW is also the region where ice flow, melt rates, and surface topography lead to the longest bare ice duration, which favors bare ice darkening (Feng et al., 2024). Do these same settings also shape the slush area above the bare ice zone? Reading this paragraph, I am also wondering about areas that may be mapped as slush early in the melt season and later turn into bare ice after depletion of the snowpack. This could be briefly discussed, as it may introduce noise in the slush extent versus elevation relationship: when slush reaches its maximum elevation, low-elevation slush areas may already have transitioned to bare ice.

We agree that the same controls that promote prolonged bare ice exposure in the SW (i.e. high melt rates, ice flow, and surface topography) are also likely to influence slush distribution, particularly by limiting its persistence at lower elevations. This will be clarified in the revised manuscript, with reference to Feng et al. (2024).

We also clarify that our elevation analysis is based on the maximum annual elevation of slush, rather than its simultaneous distribution, and therefore captures the highest inland extent reached during the melt season.

l.374 "30% of Greenland's slush area lies above ice slabs" Here, as in many other places, I miss the uncertainty range associated with this value, both from your slush maps and from the ice slab maps.

We agree that uncertainty should be reported and we will revise the manuscript to include uncertainty ranges for this (and other similar) value(s), incorporating uncertainties from both the slush classification and the ice slab dataset.

l.378 "regions lacking underlying low-permeability layers" There are several issues with this framing. (1) There is substantial slush mapped in the bare ice area in Fig. 3. These should be counted separately and definitely have an underlying low-permeability layer: the ice surface. (2) Firn aquifer regions are known for deep meltwater infiltration enabled by temperate firn and the absence of refreezing. Even if these areas do present ice layers (e.g., Miller et al., 2020, Fig. 6), they should not be grouped with ice slabs, which are defined by their low permeability. Finally, there is evidence of physical limits to meltwater infiltration in Greenland firn in the absence of massive ice layers, driven instead by contrasts in grain size and density (Humphrey et al., 2021). By documenting slush outside bare ice and ice slab areas, you may be providing the first observation of meltwater ponding due to minor ice lenses combined with density and grain-size contrasts, as studied experimentally by Humphrey et al. (2021).

We agree that this framing requires revision. We will separate slush occurring over bare ice (i.e. a low-permeability surface) from slush occurring in firn areas, using the firn area as delineated by Vandecrux et al. (2019), to distinguish these zones explicitly. We additionally agree that firn aquifer regions should not be grouped with ice slab areas given their fundamentally different hydrological behaviour, and will ensure this distinction is clearly made in the revised text. Finally, we agree that slush documented outside bare ice and ice slab areas may represent observational evidence of meltwater ponding driven by minor ice lenses, as studied experimentally by Humphrey et al. (2021), and will discuss this possibility in the revised manuscript.

l.382–384 Could this paragraph be more quantitative? If there is topographic control on slush formation, slush areas should be largely persistent for years with similar melt. I find non-persistent slush areas in unexpected zones such as the firn aquifers suspicious at first glance. It would be helpful to provide either an RF-derived uncertainty estimate for these cases or at least an expert assessment of the reliability of these non-persistent slush events.

This section will be substantively revised in response to your related comment above and will include a more quantitative treatment of these patterns.

References:

All following references have been added to the manuscript.

Feng, S., Cook, J. M., Naegeli, K., Anesio, A. M., Benning, L. G., & Tranter, M. (2024). The impact of bare ice duration and geo-topographical factors on the darkening of the Greenland Ice Sheet. Geophysical Research Letters, 51, e2023GL104894. https://doi.org/10.1029/2023GL104894

Humphrey, Neil F., Joel T. Harper, and Toby W. Meierbachtol. “Physical Limits to Meltwater Penetration in Firn.” Journal of Glaciology 67, no. 265 (2021): 952–60. https://doi.org/10.1017/jog.2021.44.

Miller, O., Solomon, D. K., Miège, C., Koenig, L., Forster, R., Schmerr, N., et al. (2020). Hydrology of a perennial firn aquifer in southeast Greenland: An overview driven by field data. Water Resources Research, 56, e2019WR026348. https://doi.org/10.1029/2019WR026348