Dear Dr. Tuckett,

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.

General Comment

Glen et al. provide a detailed analysis of surface slush extent across the Greenland Ice Sheet between 2016-2024. The dataset is novel and demonstrates the impressive scale of data processing achievable through cloud-computing and machine learning approaches. While the base method is not novel (the study builds upon the methodological framework of Dell et al.2022, 2024) and similar work has been conducted in Antarctica, this is the first ice-sheet wide study of slush in Greenland that I am aware of. The dataset itself is the strongest contribution of the work, providing a baseline for future process-based studies.

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

The manuscript is well-written, thoroughly referenced, and has a clear and logical structure. The methodological approach appears sound, although I think more clarity is needed regarding the modifications made to the Dell et al. (2022, 2024) workflow – in particular, the reason for using Sentinel-2 data instead of Landsat, and the methodological implications of this switch.

We agree that greater clarity is needed regarding our methodological modifications and will revise the methods section accordingly.

The results are well described and are supported by clear and well-designed figures. While the results are original due to the novelty of the dataset, they are not particularly surprising; they largely show the same interannual and seasonal patterns that have already been documented for supraglacial meltwater features (i.e. supraglacial lakes and streams) around Greenland. Similarly, the reported relationships between air temperature, snowmelt and slush extent are useful to show, but they offer limited new physical insight. The most interesting result is the relative area comparison between slush and surface lakes/streams, highlighting the abundance of slush in comparison to the more widely studied supraglacial meltwater features.

We acknowledge and agree with this point. We anticipate that the additional RACMO analyses described in response to '*Suggestion 1*' (below) will provide the deeper mechanistic insight that is currently lacking.

The main conclusions of the manuscript are that slush is widespread, significantly more abundant than other surface meltwater features (lakes and streams), and that slush could become an increasingly important factor for future ice sheet mass balance. The first two points are well evidenced by the data presented, but I think the manuscript is lacking in evidence to support this last point. The comparisons between slush area and RACMO snowmelt and air temperature do not provide much process-based insight, and there is no quantitative analysis linking slush to albedo, firn air content or surface energy absorption (as done in Dell et al., 2024).

We are pleased that the reviewer has acknowledged that our conclusions are well supported by the data presented. We agree that the third conclusion is currently underdeveloped relative to the evidence presented. We will revise the manuscript to either provide stronger supporting analysis for this claim or to moderate it appropriately where needed. We address the reviewer's specific suggestion regarding additional RACMO variables and process-based analysis with our response to '*Suggestion 1*' below. In terms of albedo and surface energy uptake, we will also consider whether this can be more effectively discussed using existing published frameworks scaled to the slush extents derived here, suggested by Reviewer 2.

Linked to this, I think the authors should compare their results with the results of Dell et al. (2024), highlighting differences in slush extent/variability etc. between Greenland and Antarctica, and the likely reasons for this. The 'Implications' section has the potential to be the most impactful part of the paper, but it is currently the shortest and weakest part of the discussion.

We agree that the '*Implications'* section requires development and that a comparison with Dell et al. (2024) would strengthen the manuscript. We address both points in response to '*Suggestions 2 and 3*' below.

Overall, I do not have any major issues with what the authors have submitted, and believe the dataset is a valuable contribution to The Cryosphere. My main criticism is that the data analyses and subsequent discussion are rather surface-level, meaning the paper currently lacks particularly insightful results or discussion. I think some further analysis of different RACMO datasets (e.g. snowfall precipitation & albedo), consideration of lags, and a more detailed discussion of the broader implications of the work would help to strengthen the impact of the paper.

We thank the reviewer for recognising the value of our work. We acknowledge that the current analysis is primarily descriptive and will address this by incorporating additional analyses and extending the discussion, as suggested.

Suggestions:

1) Compare your slush data with other RACMO variables, such as snowfall precipitation, albedo and firn air content (from a coupled firn densification model). Also consider temporal lags as part of your analyses and discussion.

We agree that incorporating additional RACMO variables would substantially strengthen the mechanistic interpretation of our results. Specifically, we will examine spatial and temporal relationships between slush extent and RACMO snowfall accumulation (as a control on snowpack water storage capacity) and firn air content from IMAU FDM (as a key determinant of the capacity of the firn column to absorb meltwater before saturation occurs). We will also explicitly examine potential temporal lags in these analyses (for example, examine whether antecedent snowfall or melt conditions in preceding months are more strongly correlated with slush extent than same-period values).

2) Compare your results with those from Dell et al. (2024), and discuss the potential glaciological and climatological reasons for the differences in slush proportions between the two ice sheets - e.g. why does slush in Antarctica account for ~50% of the total meltwater area, whereas in Greenland this percentage is much higher?

We intend to add a comparison with the results of Dell et al. (2024) to the revised Discussion, noting the contrast in slush proportions between the two ice sheets and offering qualitative discussion of the likely glaciological and climatological reasons for this difference, for instance varying climate, firn air content, surface mass balance, and slope.

3) Broaden your discussion section to incorporate new analyses, and provide a more nuanced discussion of the wider implications of your results.

We agree that the Discussion requires expansion and intend to address this through the addition of a new section, provisionally titled *'Controls on slush formation and persistence'*, as well as a revised '*Implications'* section. This new section will draw on the analyses described under '*Suggestion 1'* to move the paper beyond a purely descriptive account of slush distribution and towards a process-informed understanding of the climatic and glaciological factors governing where and when slush forms across the GrIS.

Specific comments:

Line 53: It is misleading to suggest that meltwater routing to the bed will always promote dynamic ice loss. Meltwater injection can cause both ice speed-up and slow-downs depending on the configuration of the subglacial drainage system. Suggest changing to 'influencing ice dynamics'.

The text will be revised in the revised manuscript.

Line 72: Given that you go on to show that slush area in Greenland can be 4-9 times greater than the area of surface meltwater features, this 50% statistic from Antarctica seems surprisingly low. You should come back to this in the discussion – why is there so much more slush area relative to surface meltwater area in Greenland than Antarctica (ice shelves)?

We agree this discrepancy warrants explicit discussion, and will address it as outlined in response to *'Suggestion 2'.*

Line 74: 'Total meltwater storage' implies Dell et al. (2024) comment on water volumes, whereas I believe they just look at slush/surface meltwater area.

This will be corrected to '*area*' in the revised manuscript.

Line 75: Be specific about what these climatological similarities are, as there are some key differences between the regions that should also be mentioned – e.g. elevation, moisture availability, FAC etc. (specifically, if comparing Greenland vs Antarctica slush as suggested, it is important to state these differences).

We will revise this sentence.

Line 101: I think in general, you need to be more specific about which parts of the method of Dell et al. (2022) you follow directly, and which elements you change. One key difference is that Dell et al. (2022, 2024) use Landsat 8 imagery, whereas this study uses Sentinel-2 – this difference is not explicitly stated. What is your reasoning for using Sentinel-2 and not Landsat- 8? What difference does this have on the method, especially the selection of thresholds during the masking and NDWI stages? Given the number of different thresholds (for both L8 and S2) used in both Moussavi et al. (2020) and Corr et al. (2022), I think you need to explicitly state the threshold values you applied to avoid confusion.

The methods section will be revised to clearly describe which elements of the Dell et al. (2022) workflow were followed directly and which were modified. We will explicitly state that this study uses Sentinel-2 rather than Landsat-8 and provide justification for this choice. All threshold values applied during the masking and NDWI stages will be explicitly stated, with the reader directed to Glen et al. (2025) where the Sentinel-2-specific thresholds are described in full.

Line 120: Why remove images with >25% cloud cover if you mask out cloud anyway? Could you not be missing lots of useful imagery?

This threshold is consistent with approaches adopted in comparable supraglacial meltwater mapping studies (e.g., Corr et al. 2022; Glen et al. 2025; Tuckett et al., 2025). We nonetheless acknowledge the reviewer's concern and will add a brief justification to the revised methods.

Line 172: How was this 'manual' review stage conducted? Was it one individual or did several people do this, with an average then taken? Distinguishing slush from other surface meltwater features is very subjective, and two people could classify an image in quite different ways.

The manuscript will be updated to clarify that this stage was conducted by a single expert analyst, consistent with the approach of Dell et al. (2022). We acknowledge that the distinction between slush and ponded water is inherently subjective, as the boundary between these classes is not always well defined, and this will be explicitly noted in the revised manuscript, with the addition of a supplementary figure to improve transparency around the classification workflow.

Line 203: I suggest you provide data in the Supplement to validate the statement that 'no year was disproportionately affected by cloud or temporal gaps'. Presumably some sectors had more frequent S2 images that others, and similarly, some sectors will have been more influenced by cloud cover than others. It would be good to show metrics for this, such as average cloud cover per sector and the average number of images used within monthly/annual mosaics, per sector.

We will add supplementary material presenting image availability and cloud cover statistics per sector, including the average number of images contributing to monthly and annual mosaics, allowing readers to assess whether any sectors or time periods were disproportionately affected by data gaps.

Line 221: These slush area values need some kind of uncertainty values reporting.

Uncertainty estimates derived from the RF classifier will be reported in the revised manuscript, with mapped slush area carrying an estimated uncertainty of ~0.2-1.3% based on classifier performance metrics, and will be reported alongside all slush area values in the revised Results.

Line 223: Why do you think this is? You don't really explain this in the discussion – given its southerly latitude and the high number of firn aquifers, it is slightly surprising that the SE sector has the least slush. Is there a climatological reason (e.g. more snowfall) for this?

We agree this is currently unexplained and will address it in the revised Discussion. We anticipate that higher snowfall accumulation in the SE sector may maintain a deeper snowpack that is less susceptible to full saturation, and will draw on our RACMO snowfall data and IMAU FDM firn air content to examine this more rigorously.

Line 226: I'm confused by the result here compared to the one stated in the next paragraph…. In Figure 3, there are dark orange areas in the NE showing slush recurrence in all 9 years, as stated in the following paragraph. So why here do you say that it only reappeared in the same location in four out of nine years?

The statement at line 226 refers to the mean persistence across each sector, whereas the following paragraph refers to the maximum pixel-level persistence, specifically the area in which slush was present in all nine melt seasons. We will clarify this distinction in the revised manuscript to ensure the two statements are not misleading when read in conjunction.

Line 248: What happens if you account for temporal lags with your correlations? There is likely a lag effect with higher air temperatures translating to increased surface slush, due to the time taken for a snowpack to saturate with melt. This likely explains the low R2 for this result. This section would be much stronger if you could quantify these time lags (e.g. do you get a stronger correlation if you correlate air temperature with slush the following month?). At a minimum, the potential for lag effects should be incorporated more into the discussion.

We agree and will explicitly test lagged correlations in the revised manuscript, examining whether antecedent temperature and snowmelt conditions yield stronger associations with slush extent than same-period comparisons.

Line 305: Again, there is no mention of lag effects here. Furthermore, you suggest that air temperature is not the sole driver of meltwater production (which is certainly true), but you do not explore this with other datasets. In particular, spatial and temporal variations in snowfall accumulation are not considered, which is likely crucial in determining meltwater storage capacity (and hence how likely it is to become fully saturated, resulting in slush). Your results would be much more impactful if you included some other variables provided by RACMO within your analyses.

This section will be substantively revised in conjunction with the additional analyses described under *'Suggestion 1'.*

Line 330: Some of the colours for the six sectors in Figure 7 are quite hard to distinguish. I suggest using more distinct colours.

The colour scheme in Figure 7 will be revised.

Line 332: It would be better to report the actual p-values (here and elsewhere), rather than simply stating p<0.05.

We report statistical significance using standard thresholds (e.g., $p < 0.05$), as the exact p-values do not affect the interpretation of the results.

Line 341: I think this whole section is missing a 'so what?' element to it. A greater surface area of the ice sheet is slush than lakes/stream, but why is this important? Is slush important for ice dynamics as with lake drainage events? Do lakes and streams still account for the majority of surface water volume? I know you can't necessarily answer this last question with your data, but some discussion of it is required.

We agree that the broader implications of slush dominance need to be more clearly articulated in the Discussion. We will revise this section to better address why the greater areal extent of slush matters, including its potential implications for meltwater storage, runoff generation, firn evolution, and possible links to ice dynamics, while also acknowledging the current limitations of our dataset in resolving relative water volumes.

Line 357: I don't think this statement is particularly helpful, because the same could be said for lakes and streams – e.g. sub-surface lakes, englacial channels. These comparisons also assume that surface area extent is the most important aspect, but there is no mention of water volume.

This statement will be removed from the revised manuscript.

Line 382: Can you quantify these links with ice slabs and firn aquifers? I think more discussion of specific examples related to this is required - E.g. Why are some ice slabs (e.g. in the NW sector) not associated with slush? Why is there such high persistence in some parts of the NE and NO where there isn't an ice slab?

This section will be revised to distinguish between slush occurring over bare ice, ice slabs, and firn aquifer regions, in response to comments raised by Reviewer 2. This revised framing will allow us to more directly address specific spatial patterns, including the limited slush associated with ice slabs in the NW sector and the high slush persistence observed in parts of the NE and NO in the apparent absence of ice slabs.

Line 402: Do you mean September 2022? You state in the methods that mapping was only conducted between May-September each year.

We will change this to '*late-September*' in the revised manuscript

Line 411: Could you not estimate long-term trends in slush based on the relationships you have derived with RACMO snowmelt? Or do you not think these relationships would hold going back in time?

We will consider the feasibility of using the derived relationships to estimate historical slush trends in the revised manuscript. However, key caveats would limit such an approach, including the potential non-stationarity of relationships over longer timescales as ice surface properties and firn structure evolve. We will propose this as a direction for future work.

Line 426: This final sentence is too sensationalist, and isn't supported by any of the data or results you have presented.

This sentence will be removed from the revised manuscript.

Line 431: Why haven't you done some of these steps? Wouldn't they be relatively simple to implement given you already have the RACMO data?

We will revise the manuscript to incorporate additional analyses of key RACMO variables, including snowfall accumulation, firn air content, as outlined under '*Suggestion 1',* and expand the Discussion accordingly. We note, however, that a fully comprehensive treatment of all controls on slush formation remains beyond the scope of the present study and represents an important direction for future work.

Technical corrections:

Line 46: Close bracket.

Line 47: Fullstop needed after 'laterally'.

Line 228: Grammar – 'one' not 'once'

All technical corrections will be made in the revised manuscript.