

## Reviewer 1

The authors have clearly put in a lot of effort and consideration into the manuscript since the initial submission. The revised manuscript is vastly improved (scientifically) and addressed the reviewer comments sufficiently—thank you to the authors for the hard work in implementing these changes. In particular, the study now focuses on dry firn and hydrological transition zones in the accumulation area instead of attempting to characterize 5 zones across the entire glacier elevation. The processing methodology has also been improved to assess changes on a per-glacier basis, instead of a domain-wide analysis. The paper is (still) written at a very high quality. I can recommend the study for publication, with or without some specific/technical suggestions outlined below.

Line numbers below refer to the track-changed document:

L29: This is a little redundant, repeating the “delayed runoff” statement twice.

*We have removed the 2nd delayed runoff statement*

L31: “longer timescales”: Maybe rephrase to be a bit more precise. I think the statement is indicating that this can change the fundamental firn structure and thereby hydrological response of the glacier. This could be a bit clearer.

*Rephrased to: Conversely, near-surface refreezing of meltwater into ice layers can reduce firn permeability and increase runoff as a result of this fundamental change to firn structure*

L50: remove “adjacent”

*Done*

L57-63: This is an important paragraph setting up the study—reword to avoid shifting focus away from what you’ve done. Maybe something like, “While regional assessments have linked temperature to rising snowlines and melt extents (Wells et al., 2026; Bevington and Menounos, 2025), the spatial extent of firn transitioning from dry to wet conditions in Wrangell/St. Elias remains poorly constrained. Here, we use...” I would also add a concluding sentence to the end of this paragraph to wrap it up nicely.

*Rephrased to: While recent observations from Wrangell/St. Elias have shown evidence of firn warming and increased surface melt production and percolation (Ochwat2021, Kindstedt2025) and regional assessments have linked temperature to rising snowlines and melt extents (Bevington2025, Wells2026), the spatial extent of firn transitioning from dry to wet conditions in Wrangell/St. Elias remains poorly constrained.*

L89-92: Rephrase to stronger sentence. Maybe something like (you could probably write this better than I do, but just for an idea): “We identify melt onset and freeze up dates for scenes from each Sentinel-1 path/frame independently, to avoid inconsistencies in backscatter returns

between path/frames that are associated with the satellite angle of incidence. For pixels covered by multiple path/frames, we select the earliest date as the melt onset or freeze-up.”

*Rephrased to: To avoid inconsistencies in backscatter associated with differences in satellite incidence angle among paths/frames, we identify melt onset and freeze-up dates independently for each path/frame. For pixels covered by multiple paths/frames, we assign the earliest detected date as the melt onset or freeze-up date.*

L100: delete “below”

Done

L109-110: I’m not actually fully convinced by this statement, that it is most effective in the accumulation area because there is tens of meters of dry snow. My understanding of SAR, particularly on mountain glaciers with very little dry firn zone (which has low backscatter return in winter due to volume scattering), ice lenses in firn of accumulation areas cause high backscatter in winter. The amount of dry snow above the firn is then sort of irrelevant. Ultimately, this signal is highly distinguishable from wet snow during the melt season, but this wet snow signal is generally quite distinguishable across the entire glacier since it’s such a stark drop in backscatter compared to reflections off of ice, debris, or ice lenses.

*We have removed this statement to avoid confusion*

L113-114: This is a reasonable assumption across most of the glacier, but could be susceptible to winter snowpack quickly melting out near glacier termini, particularly if one or a few Sentinel scenes happen to be missing. There are ways to filter this based on the assumption that melt at higher elevations on a glacier implies melt is occurring at lower elevations. However, since you don’t focus on ablation areas anyway, this is likely okay to keep and not make any adjustments. Just wanted to note this, though.

*Yes, agreed. Thanks for the input*

L119: SAR penetration statement in dry snow is redundant from earlier—suggest consolidating to only state once.

*The earlier statement has been removed*

Fig. 2 caption: suggest adding “Sentinel-1 (S1)” and “melt onset and freeze-up day of year (DOY)” to align with terminology in the figure

*Good catch – done*

L135: Elaboration is likely not needed, can just say “...are excluded from analysis. We also...”

We left this is as since the elaboration was in response to another review

L141-149: Is this necessary here? I would move this to the results as a results section, or part of a section. Alternatively, you could also state something like "...and the Copernicus DEM (ESA, 2014), although the methodology is robust to the choice of DEM used to remove steep terrain (Appendix X)." and then move this to the appendix/supplementary material. This choice is up to the authors: to keep this here, move it to results, or to the supplement.

We have moved this section to an appendix

Table 1 caption: Is all the technical information on the accuracy and operational ranges of the equipment needed in the caption? I would move this to the appendix or supplement. Again, however, this is not a critical point and up to the authors discretion.

We have left the technical information in the table caption, as it was in response to another review

L274: rephrase to "pixels that experience melt"

Done

L294: Is this just not the snowline response? That is, that the surface returns to ice or debris during the summer, which is the same as the winter signal (which penetrates dry snow to reflect of underlying ice or debris). If so, it's probably worth explicitly stating that this the ablation zone, when the snowline has migrated above it. - Note: I see that this is elaborated on in section 4.2. I'd still include something very brief here as well.

Rephrased to: *Freeze-up does not mirror the downward migration of the  $0^{\circ}\text{C}$  isotherm in the fall (Fig. 4). Below 1,400 m a.s.l.,  $\Delta\text{freeze} < 0$  days, meaning winter firm conditions are reached as the snowline migrates to higher elevations before air temperatures drop below  $0^{\circ}\text{C}$ .*

L298: suggest removing "our" and just refer to fig. 5: "...five terrain characteristics (Fig. 5)..."

Done

Fig. 6 and Fig. 7: Suggest removing the hillshade because it makes the figure look a bit foggy/blurry in the middle, and thus tougher to see the dry firn and transition zone classifications. Possibly have the glaciers shaded a very light grey instead.

After testing out removing the hillshade, we decided to keep it since it provides helpful context (particularly in Fig. 7) about where the transition zones are relative to the tall peaks in the region.

L346: Noting this reference placeholder to be updated

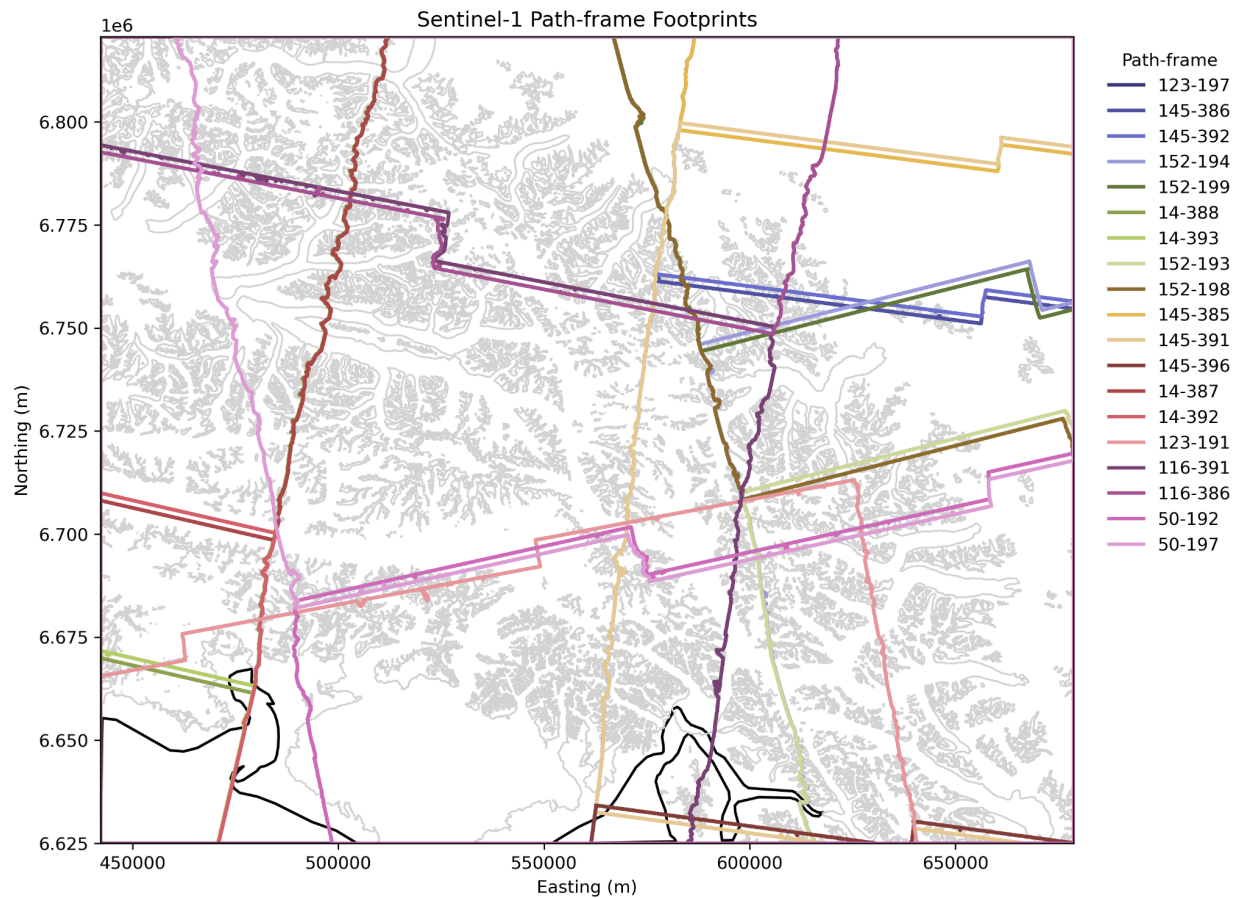
Done

L394-395: possibly typo in this sentence? Maybe something like “...and subsequently deriving hydrological transition zones...”

Rephased to: *While the approach presented here is limited to resolving the timing of melt onset and freeze-up, and subsequently deriving hydrological transition zones...*

Figure A1: Some basemap or coastline and glacier polygon layer in the background would make this figure much easier place and interpret where the scenes are.

Coastline and glacier outlines have been added:



## Reviewer 2

The generic use of pixel is confusing and needs further explanation in places. After a kernel size 5 median filter is applied the grid cell is no longer the same size as the original input. Please review the manuscript and explicitly define the differing sizes and manipulations as they are invoked.

We have added the following text clarifying our use of the term pixel:

*We use the term pixel as it is commonly used in SAR literature to refer to a single unit of gridded remote sensing data, rather than a picture element on a computer screen.*

Applying a 5x5 median filter does not actually change the grid cell size. Instead, it changes the value to be the median of the 5x5 neighborhood centered on that cell. To that end, we have edited the text as follows:

*To reduce speckling in our results, we apply a median filter with a kernel size of 5 to each scene in the dry-snow winter average and in the melt season timeseries, replacing each pixel value with the median of the 5x5 neighborhood centered on that pixel.*

It is still a mystery to me why a generic melt model is included to determine if melt can occur below air temperatures of 0C. The inclusion of a more realistic melt model would have added considerable value to this work.

We agree that developing a more sophisticated melt model would be a significant and valuable undertaking. However, we do not think that a more complex model would better serve the specific purpose of this analysis: evaluating whether melt under below-freezing air temperatures is physically plausible. For this purpose, the simple model we use is sufficient to address the question without introducing additional complexity that is beyond the scope of this study. A comprehensive and realistic melt model for the Wrangell/St. Elias region would itself warrant a dedicated study, with model development and evaluation as the central focus. We therefore chose to retain the simpler approach so that the analysis remains focused on the primary objective of this work: understanding firn hydrology.