Reviewer #1

Major Comments

Summary: This paper uses repeat firn cores collected at four sites on the Juneau Ice Field, together with climate reanalysis and numerical firn modeling to investigate changing firn properties on the JIF and their impacts on runoff from the accumulation zone. Given the dearth of data on firn structure and firn hydrology on Alaskan glaciers, this paper provides a valuable first look at these questions.

We thank the reviewer for taking the time to review this paper. Their comments have helped revise the paper by some restructuring, clarification, additional quantification, and technical changes.

[1] The research questions articulated in lines 105-107 are much broader than this study can answer, given the limited number of study sites. I would suggest that the authors tighten these questions to the scope of the study. Perhaps something along the lines of:

> (1) How have firn properties changed on seasonal to decadal time scales at 4 sites in the Juneau Ice Field wet snow zone?
>
> (2) How have these firn property changes affected runoff at these sites? Alternatively, the introduction and discussion would need to make a stronger case for why the inferences from these four sites can be extrapolated to Alaskan glacier hydrology more generally.

We thank the reviewer for pointing out this concern. We have followed their suggestions and changed the paragraph that includes the motivating questions to:

"Here, we motivate this study through the following questions: (1) How have melt and firn properties changed over seasonal to decadal time scales at four sites on the Juneau Icefield? (2) How have these firn-property changes impacted refreeze and runoff?"

[2] Structurally it might be helpful to present the results in the same order you present the data collection (cores -> reanalysis -> model).

We agree that a similar order would be beneficial to readers. We changed the order of the methods instead, as climate reanalysis -> cores -> model, to reflect the order of the subsections of the Results section.

[3] The paper could be strengthened by using multiple climate reanalysis or regional climate hindcasts in the climate analysis (Sections 2.2 and 3.1). Some discussion of how these reanalysis products have been validated for Alaska and their limitations would also be appropriate. If reported trends are consistent across multiple reanalysis products, this would make a stronger case for the results of this paper. If only one

reanalysis product is used, I would encourage the authors to very clearly justify why that particular product was selected.

We thank the reviewer for highlighting this concern. We use the particular reanalyzed surface-mass balance product from Ing et al. (2025) for a few reasons: (1) the reanalysis model CFSR (Saha et al., 2010) was chosen by Lader et al. (2016) because it was one of the top well-performing reanalysis models for southeast Alaska; (2) the output from the dynamically downscaled CFSR model shows good agreement with automatic weather-station observations at the Juneau airport (Lader et al., 2016); and (3) the two other models that Ing et al.(2025) use (i.e., CCSM and GFDL) are higher and lower sensitivity than CFSR output while producing similar means (Ing et al., 2025).

We agree that an analysis featuring multiple climate products would be an interesting study. However, we argue that it is outside the context of the present study, in that our focus is to use multiple lines of evidence to understand the evolution of firn on the icefield. Such a model intercomparison, as the reviewer suggests, would shift the focus of the paper too much towards asking questions regarding regional climate model (RCM) performance and variability, which is not the focus here. Such a model intercomparison doubtless deserves a standalone study.

We added to and clarified the text in subsection “2.2 Climate” (Line 130) to better and more clearly justify why the particular product was selected:

“We analyze climate variables (including temperature, surface snow melt, snow accumulation, and rainfall) at the four study sites covering the dates between 1980 to 2019 using a reanalyzed downscaled surface mass-balance (SMB) product from Ing et al. (2025). Ing et al. (2025) simulated historical and future SMB for the Juneau Icefield by forcing the COupled Snowpack and Ice surface energy and mass-balance model in Python (COSIPY) with three different climate reanalyses that were originally dynamically downscaled to southeast Alaska by Lader et al. (2020). Ing et al. (2025) tuned the model to the longstanding and rich history of measurements recorded on the Juneau Icefield.

Of the climate products produced by Ing et al. (2025), we select model results using The Climate Forecast System Reanalysis (CFSR) (Saha et al., 2010) due to several reasons: (1) the dynamically downscaled reanalysis model CFSR was chosen by Lader et al. (2020) because it was one of the top well-performing reanalysis models for southeast Alaska; (2) the output from the CFSR model shows good agreement with automatic weather-station observations at the Juneau airport (Lader et al., 2020); and (3) the two other reanalyses that Ing et al. (2025) use (i.e., CCSM and GFDL) have higher and lower climate sensitivity than the CFSR model, respectively, and thus span the results from CFSR while producing similar means to CFSR (Ing et al., 2025). We use the CFSR climate output from Ing et al. (2025) to determine interannual and daily variability and trends in climate variables, and as input to the Community Firn Model. Spatial resolution is 0.01◦ latitude and longitude, and temporal resolution is daily.”

We also title the Section 2.2 to more accurately reflect the product as a whole: “Section 2.2 Climate.”

Further, we added text in Section 4.6 “Future Work” (Line 528) to discuss considerations regarding our choice of climate product and firn model:

"There are inherent uncertainties and limitations in how firn-model results can be interpreted, and further studies investigating how uncertainties and variability among climate products manifest in firn-model output would help quantify the uncertainty in firn-model results on the Juneau Icefield and similar glacier systems. We hypothesize that different climate forcings would produce varied firn profiles but that on decadal time scales the trends would resemble our results. In this study, we assumed that using a simple bucket scheme simulates meltwater processes accurately enough to assess the decade-scale trends in firn properties, following other studies in similar regions (e.g., Ochwat et al., 2021; Kindstedt et al., 2025). However, due to their instantaneous nature, they may not accurately simulate the timing of meltwater runoff or percolation depths because of uncertainties in ice-layer permeability and lack of preferential-flow processes. While we hypothesize that changing the parameterizations in the bucket scheme would have limited effects on decadal trends in our study because, for example, the firn at our sites is temperate and changing the threshold for an impermeable ice lens would not likely change the percent of meltwater that runs off substantially (i.e., ice lenses do not block access to cold porosity). We suggest that future work should explore detailed model intercomparisons, which would be required to fully understand the effects of climate forcing and model physics, but are beyond the scope of the present work.”

[4] For all analyses where you report rates of change, please give the p value and confidence intervals on the slope from linear regression results so the reader can evaluate how robust the relationship is.

Thanks. For each linear least squares regression, we include the linear regression fit, the R squared value, the p value, and confidence intervals in the figures discussed in Section 3.1 “Changes in Climate” and Section 3.3 “Modeling Results” (see below). We also include these statistics on the figures and summarized in the Appendix Table A1 (not shown here).

Please note that the rain panels were moved to the appendix, since there were no significant trends. Please note we also omitted the melt volume panels in Figure 7 because melt is shown in Figure 2. We also separated each site into separate panels so readers can more easily see each dataset and the corresponding linear regression.

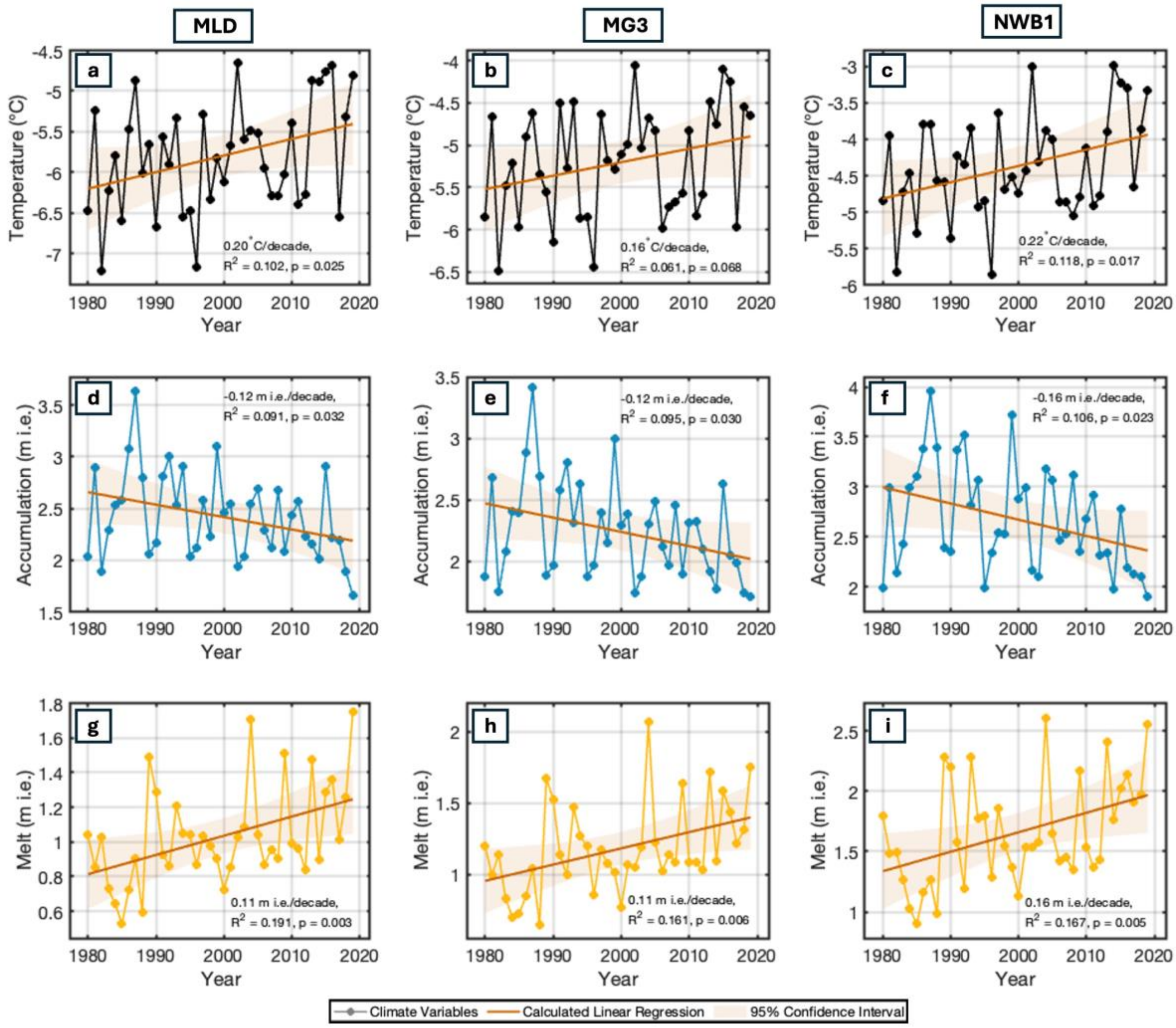

Figure 2. Annual climate at each site (MLD, MG3, NWB1) in the accumulation zone from the product by Ing et al. (2024): mean annual skin temperature (a-c), cumulative annual snow-accumulation (d-f), cumulative surface melt (g-i).

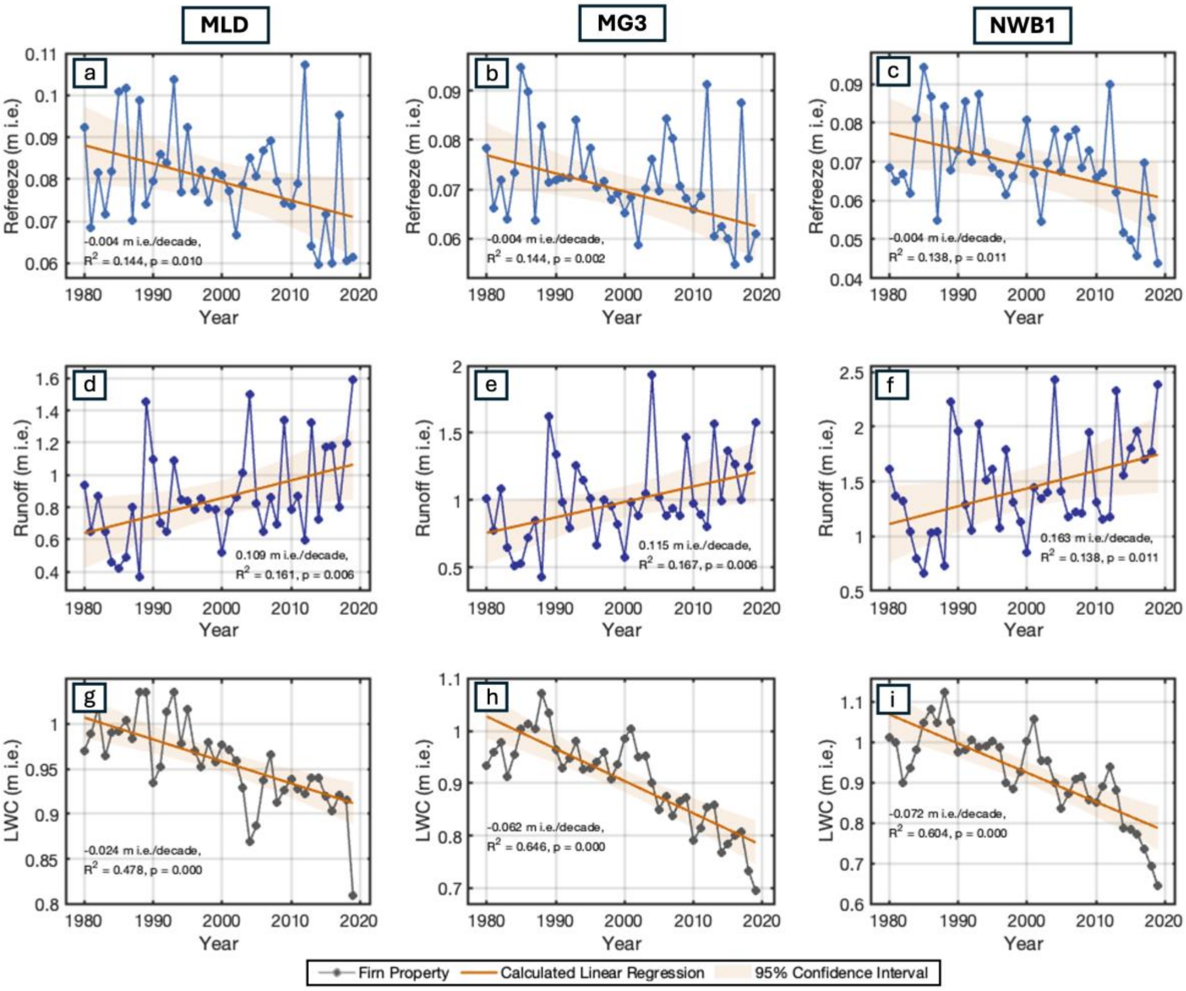

Figure 7. Modeled cumulative annual refreeze in the firn (a-c), modeled cumulative annual runoff (d-f), and modeled annual depth-averaged liquid-water content (LWC) in the firn (g-i).

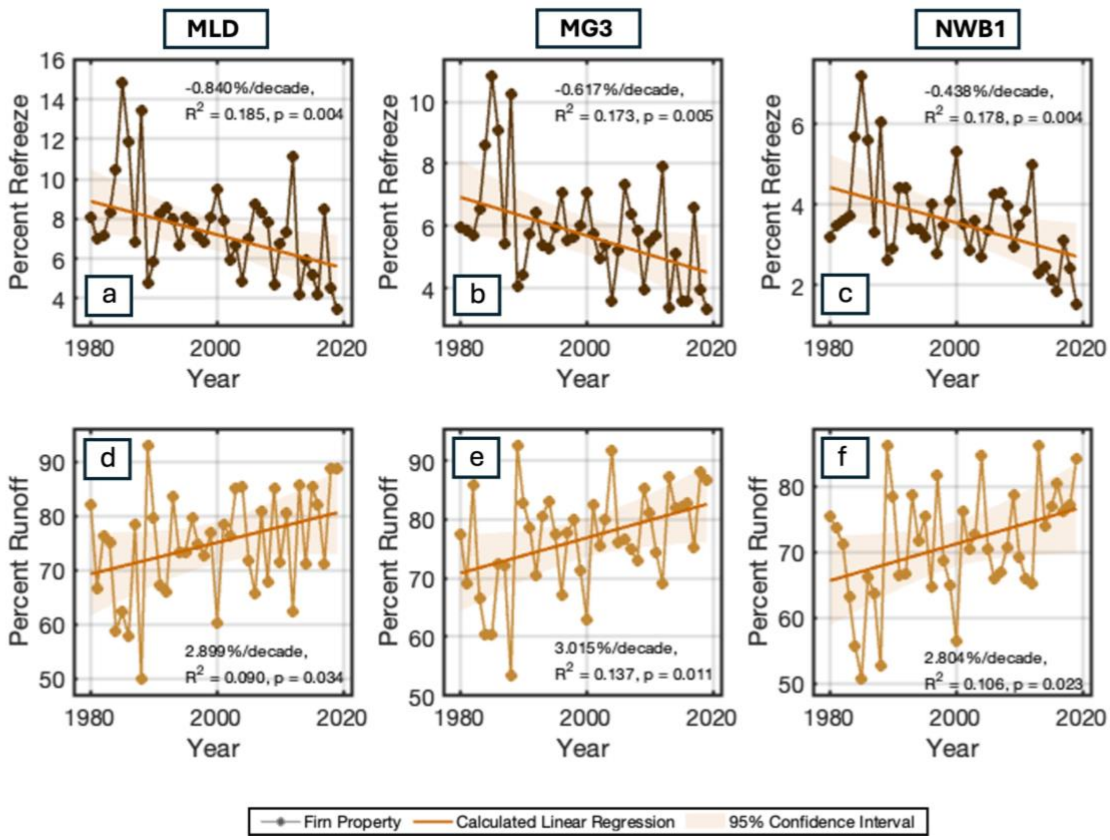

Figure 8. Percent of modeled annual melt and rain that refroze in the firn (a-c), and percent of modeled annual melt and rain that ran off from the firn (d-f)

[5] Line 237 – 244: How are these absolute changes in the date of melt onset being reported? Is this the date in 1980 vs. the date in 2019? I assume that the change in not perfectly monotonic from year to year, so it is important to explain how these differences are calculated. If you want to report absolute change rather than a rate of change from a linear regression, comparing decadal averages might be most appropriate. I have the same questions/concerns about the discussion on the length of the melt season, the number of annual melt days, and the timing of runoff.

The values in the paper were originally reported as absolute change and we agree that more clarity is necessary. We include changes to reflect the reviewer's concerns. We calculate the linear least squares regression for the onset of the melt season, melt-season length, and melt intensity. We include the rates of change in the main text, and report R squared, p value, and 95% confidence interval in the appendix Table A1.

We decide to only include changes for onset of melt season and melt-season length for the NWB1 site, as the R squared values for MG3 and MLD are very low (R^2<0.05). We include all sites for melt intensity.

[6] The paper would be strengthened with some quantitative comparison between the CFM model results and the field data, as well as with a discussion of model performance and limitations. The CFM results are really central to the conclusions about changing refreeze and runoff proportions, so it would be good to justify to the reader that the model setup captures at least the general observations of density, etc.

We had qualitatively compared modeled results with observations: (1) We compared the modeled and observed depth-density profiles, shown in Figure 4, which is standard practice. (2) We also analyzed dry and wet density through the season (Figure A5), which shows, with a prescribed boundary condition, surface density of 350 kg m^{-3}, that the low-density snow melts away and densifies during the beginning of the melt season and surface snow in July agrees with our measured surface densities (~400 kg m^{-3}). (3) We note that our measured LWC is generally in line with modeled results (> 10%).

To address the reviewer's concerns, we also calculate the Normalized Root Mean Square Deviation (NRMSD) and include these in Table 3:

Table 3. Normalized Root Mean Square Deviation (NRMSD) calculated for modeled depth-density profile for 1 June 2019 and the measured depth-density profiles shown (Figure 4).

Core	MLD	MG3	NWB1
C1	0.2180	0.1907	0.2637
C2	0.2408	0.2361	0.2933
C3	0.2562	0.2355	0.1890

We also edited the text in section "2.4 Community Firn Model" to add more detail about how we configured the CFM for the model runs in this study. We have also added additional text to section "4.6 Future Work" to highlight that a full model intercomparison is beyond the scope of this study, but that it is worthy of future work. We specify that our expectation is that while model runs with different forcings and/or model configurations would likely change the shorter-term variability in the model outputs but would not significantly change the decadal-scale trends

[7] The discussion of the GPR data seems underdeveloped. Where exactly was the data collected (a map inset on Figure 1 might be helpful)? Can the data be shown in comparison to the cores it was collected near in a figure? The results discuss apparent changes in attenuation/reflectivity over time, but in the discussion the GPR data is mostly referenced to suggest that some ice layer horizons may be continuous, but then that observation is almost largely discounted. Are the "unstacked radar profiles"

mentioned in the discussion from this same data set? If so, that comparison should probably be mentioned in the methods. Overall, it is not totally clear to me what the GPR data adds to the interpretation or conclusions.

Due to similar concerns from the second reviewer, we decided to delete the GPR section. While the GPR measurements were an important part of our proposed work and field season, the results are not integral to the conclusions of this study.

Minor Comments

Line 27: if you choose to define firn, do so the first time it is mentioned on line 24.

Thanks. Fixed.

Lines 75 – 81: might want to include a short discussion of the role of microstructure and thermal properties since the topic sentence for the paragraph includes these topics.

The paper mainly focuses on thermal and macrostructural properties of the firn. We therefore exclude discussion of the role of microstructure, which we now make clear. We also have outlined ways in which the thermal properties of the firn impact firn hydrology in previous paragraphs. We clarify that this paragraph discusses briefly the *interplay* of these properties:

"Runoff efficiency and timing is influenced by the interplay of thermal, macrostructural, and microstructural properties of the snow and firn. In this paper, we focus on the former two. Water exceeding the irreducible water content that cannot percolate downward is available for runoff. The irreducible water content is the residual portion of water that is retained in the pore space of the firn due to capillary forces, and is approximately 6-8% water saturation or 2-3% of unit volume (Schneider and Jansson, 2004; Colbeck, 1974), though some studies define irreducible water content as a density-dependent relation (Coléou and Lesaffre, 1998). Further, runoff of meltwater from the firn may be promoted by refreezing processes such as ice-layer formation, which may reduce or impede vertical percolation, increase lateral flow along the ice-layer surface into crevasses, and reduce the retention capacity of the firn (Machguth et al., 2016; Culberg et al., 2021)."

Line 97: spell out "ELA" the first time you use it

Thanks, fixed.

Line 97: can you quantify "significant mass loss"?

We elaborate on this: "The icefield is projected to lose significant mass in the coming decades, with a volume decrease of at least 58% possible by 2100 (as compared with 2010; Ziemen et al. (2016))."

Line 122: “These include different climatic regimes and their history of previous repeat mass balance and snow density measurements (McNeil et al., 2020).” I don’t really understand what the second half of this sentence is trying to convey and how it is related to the first half of the sentence.

We clarified this sentence: “These sites include different climatic regimes (e.g., different altitudes, proximity to the coast, and surface mass balance) (Figure 1; Table 1) and have a history of previous repeat mass-balance and snow-density measurements (McNeil et al., 2020).”

Lines 124-126: It would be helpful to have these statistics in Table 1 if you’re gong to discuss them here (could be from past publications or measurements if you want to save your measurements for later in the paper).

Thanks. While we agree that it could be helpful to include these statistics in Table 1, we choose to keep the climate results in Table 2 and the other site specific information in Table 1. We did rearrange the sections and placed all site description in the first section of the Methods so that readers can understand which sites this paper focuses on at the outset.

Line 127: It would be helpful to provide some information on how far apart in time and space the repeat cores were collected at each site.

We have already specified how far apart the cores were taken in this section. We add how often they were collected at the end of this sentence on line 152: “Each repeat firn core at a site was taken within 1-2 m of each other and retrieved one week to one month a part (Table 4).” Table 4, in the results section, illustrates the exact dates that the cores were retrieved.

Line 150: For the reader who is not intimately familiar with the Ochwat et al. (2021) paper, it would be helpful to briefly comment on the source of all these numbers. Without context they seem very subjective, so if there is a quick (1-2 sentence) way to justify them, I think that would be valuable.

We added more description of the variables and an additional citation if readers would like more information. While it is largely subjective, we try to follow previous published studies that have attempted to invoke as much quantifiable characterization of the cores as possible (e.g., Ochwat et al (2021) and Kindstedt et al. (2025)).

One line 169, we add: “…where 1 represents an intact core and numbers between 0 and 1 represent the fraction of the core’s volume that is missing.”

On line 175, we add a description of *dm*: “…which accounts for residual snow and liquid water on the core surface and the scale accuracy of 0.1g.”

We also added a citation to the end of the section: Kindstedt et al. (2025).

Line 155: Might be helpful to include (potentially in an appendix or supplement) a table of exactly what measurements were made at each site and on each core on which dates.

We appreciate the suggestion and feel that Table 4 summarizes what measurements were taken on the core well enough.

Equation 4: this is kind of nitpicky, but k is more commonly used for relative permeability (e.g. the flow parameter) and ϵ for relative permittivity (the electrical property). You are clear about the definition, so you don't have to change it, but I do think it could be clearer at first glance to use ϵ since in this context you could totally be interested in calculating relative permeability.

We appreciate the suggestion and acknowledge that the use of these specific variables may be confused with other commonly associated physics. However, our intent is to keep the variables congruent with Webb et al. (2021), from which this equation is from.

Section 2.3: not totally clear from the text, but I assume that you are just running CFM at the 3 core sites in the accumulation zone? Please clarify in text.

Yes, we clarify on line 201: "We use the Community Firn Model (CFM) to simulate firn evolution at our three sites in the accumulation zone of the Juneau Icefield."

Equation 5: change $rhoj$ to ρj

Thanks, fixed.

Line 229: broken reference link to appendix figure.

Fixed.

Figure 2: font size is really hard to read at 100% zoom – can you increase?

Fixed.

Line 309: Should this be a reference to Figure 5?

Yes, thanks. We fixed this to reference Figure 5.

Lines 316 – 322: I don't totally understand the difference between "annual refreeze in the firn" and the "percent liquid water that is introduced into the firn that is refrozen".

We tried to clarify the latter in more detail on line 348 as: "the percent of annual melt and rain that refroze in the firn" The annual refreeze in the firn is the total amount of refreeze in the firn, described in meters (for a 1 m x 1 m section of firn column). The percent of liquid water that is introduced into the firn that is refrozen is a percent to describe how much meltwater and rain water is refrozen (vs runoff vs stored), as a percent.

Lines 342 – 349: Need to be very clear in this section what is modeled results and what is measurements.

Thanks. We acknowledge the need to differentiate between modeled and measured results. While we already have designated the sections as "our measurements suggest …" and "model results suggest …" we added clarifications throughout the entire text.

Line 389: even greater spatial variability than what? What is the point of comparison?

We deleted all sections regarding GPR in response to the reviewer's previous concern. Therefore, this sentence is omitted.

Lines 460-468: it would be at least worth mentioning that the next step from this idealized calculation would be to run CFM across the entire Taku Glacier accumulation zone. Also, can you contextualize these numbers with total runoff from Taku? How does the amount of runoff from the firn/accumulation zone compare to runoff from the ablation zone?

Thanks. We revise and mention that a potential future direction would be to run the CFM across the accumulation zone of the Taku Glacier and Juneau Icefield as a whole:

"he next step from this calculation would be to run a firn model across the entire accumulation zone of the Taku Glacier, and even the entire accumulation zone of the Juneau Icefield."

Reviewer #2

This is a comprehensive paper looking at the firn conditions on the Juneau Icefield from both in situ measurements and a firn model. The measurements are repeat cores from the 2024 summer season. The firn model is forced by climate reanalysis data between 1980 and 2019. The paper finds decreasing firn thickness and cold content and increasing melt, runoff, and firn temperatures during the modeling period, and variable LWC and refreezing features within the cores.

Overall, I think this is a useful contribution to the firn measurement and modeling community. Wet firn is extremely difficult to measure and monitor, and this paper includes some of the first measurements of LWC in firn.

Most of my comments are inline in the PDF, but I have summarised some of the broader comments here.

We thank the reviewer for taking the time to review this paper. Their comments have helped revise the paper by some restructuring, clarification, additional quantification, and technical changes.

Please note that we illustrate our LWC content results more clearly by additionally including box-and-whisker plots of LWC in the appendix.

Moderate notes

- The paper is quite long, so it may be useful to look for areas where concepts/results are repeated or not needed because they don't relate directly to the reported results. For example, you may be able to reduce or remove some sections in Introduction/Firn Hydrology, e.g., firn aquifers are not discussed much in the results or discussion, but are discussed quite a bit in the introduction.

Thanks. We acknowledge that the paper is long. We have eliminated all sections regarding the GPR, in response to both reviewer's concerns, which has shortened the length. However, we believe that we need to keep the aquifer section in the text, as we do discuss firn aquifers and their presence on the icefield in the discussion section ("4.4 Aquifers").

- The GPR is not discussed at all in the results. Either a section should be added to the results about tracking melt features in the GPR and/or the variability of melt features seen in the GPR, or there should be some mention in the methods & discussion about why this data should be included in this paper and/or why it wasn't analysed.

We deleted the GPR sections in the methods, results, and discussion, based on both reviewer's concerns. The GPR results were an integral component to the proposal that motivated this work and to the fieldwork, but are not to the interpretations in this paper.

- Figure 3 makes it very difficult to see how or if ablation was taken into account because all the cores are shown starting at the same surface height. I would recommend plotting these so that they are shown at depths relative to the snow surface of the earliest season core at each location. Otherwise, please clarify how ablation was taken into account when determining the relative depth of the cores & layers.

Thanks. All cores are plotted with reference to the same surface, the snow surface at the time of the earliest season core. We make it clearer in the figure and have clarified previously in the figure caption:

"Figure 3. Stratigraphy of the repeat firn cores at sites MLD, MG3, NWB1, and TKG4. All surfaces are corrected for ablation, except MG3, where no ablation measurements exist. Repeat cores were taken within 1-3 m of each other. The differences in stratigraphy show that even great local variability exists in high-melt and highly temperate snow and firn. Large volumes of liquid water were found at the base of the TKG4_C1 core."

While we already state this within the Methods section text, we add a reference to ablation in Figure 3:

"We report all layer depths to 1-cm accuracy and relative to the first core's summer snow surface at the site (i.e., accounting for ablation of the surface), as other reference depths such as the previous summer surface were difficult to discern within the cores and because the firn-ice transition was not reached for all cores (Figure 3)."

and

"We report all depths associated with the density measurements relative to the first core's snow surface at the site."

- The "flashier" hydrology in the Discussion section isn't sufficiently motivated or supported by citations. While it seems plausible, this could be better supported by looking at work looking at seasonal snow or rain-on-snow events, but in general would require looking at downstream hydrology or looking at how runoff vs. melt events is changing.

Thanks. We clarify and restructure this paragraph to reflect the reviewer's concerns. We more clearly explain that increased runoff and decreased storage (liquid water and ice) in the firn may propagate surface melt and rainfall into the downstream hydrological system faster. We eliminate the term 'flashy' as we follow the reviewer's concerns for caution in connecting these impacts.

We then introduce recent results from Young et al. (2021), which found that general total glacier runoff is shifting earlier in the season and changes in the firn may complicate these findings.

"Because the firn layer modulates the timing and volume of water flow through a glacier (Fountain, 1996), we suggest that increased runoff from the firn and decreased refreezing, in tandem with long-term firn thinning, may increase the propagation rate of surface melt and rainfall into downstream hydrology. While some liquid water input is accommodated by refreezing and transient liquid water storage (both of which decreased), the percent that becomes runoff from the firn increased across all sites. As a result, melt and/or rain events may be more promptly propagated downstream, possibly contributing to a higher likelihood for downstream flooding or highly variable stream flow. Depending on melt and rainfall variability, these firn changes may contribute to broader hydrological changes on the Juneau Icefield. For example, earlier shifts of total glacier runoff into downstream environments may contribute to changes in stream temperature and clarity, and consequently may impact species, such as salmon, that prefer certain freshwater conditions (Young et al., 2021)."

- I am not completely convinced still that the decrease in the number of days with high cold content is solely responsible for the increase in runoff and earlier regime transition --- decrease in FAC and thinning firn packs (resulting in less pore space for water) likely also play a role. If this is your argument, I think it needs to be strengthened by showing correlations between cold content and runoff in the results or discussion and/or more explicitly working the reader through the physical premise for your argument.

Thanks. We acknowledge that the amount of pore space available for water storage within the firn column, the firn-air content (FAC), can also be important in determining how much runoff and refreezing occurs from the firn.

Our primary expectation regarding refreezing was initially stated in the paper: “While firn volumes and therefore available porosity is decreasing on the icefield, we expect that the primary limiting factor on the volume of melt that can refreeze in the firn is the cold content (i.e., the energy required to warm the firn to the melting point).” And this is partly confirmed by correlation calculations (though correlation does not necessarily mean causation), though we describe some considerations below.

As for runoff: the melt rate (for how much melt is introduced), the cold content (for how much melt refreezes), and the firn-air content (how much pore space is available for liquid or air to occupy) may all determine runoff.

However, in our case, the importance of each can be determined based off of volume: the pore space available is > 8-10 m (Figure 6) whereas the volume of melt produced is much smaller than that (0.5-2 m). Therefore, the amount of pore space is likely not the most important limiting factor and we then deduce that cold content is likely the most important.

We performed correlation calculations to additionally address the reviewer’s concerns and found that the correlation between cold content and runoff is higher (R = -0.35 (MLD), -037 (MG3), and -0.46 (NWB1)) than the correlation between the firn-air content and runoff is R = -0.24 (MLD); -0.19 (MG3); -0.21 (NWB1). Further, we found that the correlation between annual cold content and first day of runoff is R = 0.37 (MLD), 0.38 (MG3), 0.62 (NWB1).

We note that while a correlation can constrain some of these relationships, the runoff from the firn is directly linked to how much melt is introduced into the system on a given day while firn properties will contribute what occurs after the surface melt occurs. Refreeze, infiltration, and other firn processes are non-linear and therefore may be lagged non-linearly from the imposed climate forcing; thus, we cannot completely constrain the relationships of these variables through a simple correlation calculation.

We have clarified the physical reasoning in the discussion section of the paper:

“On the Juneau Icefield, the cold content in the firn governs the capacity of the firn to refreeze meltwater, due to its near-isothermal nature. The amount of pore space is another factor that generally may contribute to capacity for runoff and refreeze;

however, it is likely not the most important factor here because the available pore space is >8-10 m, whereas the volume of melt produced is much smaller (0.5 - 2 m)."

We also realize that some of statements in the paper may not have clearly delineated these relationships, and we therefore clarify our reasoning throughout the paper.

We also note that we have three figures related to cold content in the paper: the number of days above the threshold; the actual cold content for a 1 x 1 m firn column; and the winter cold content." In the main text, we had shown the number of days above the threshold of cold content for the reader's ease of understanding but it is the cold content itself that is the limiting factor for refreezing. We therefore switch the figures and put the figure with actual cold content values in the main text.

Minor notes

- Check for consistency in the verb tense in the methods/results - the writing often switches between past and present tense, which can make it difficult for the reader to follow along. For example, in the paragraph starting at line 295, the first sentence uses past tense ("were") and the subsequent paragraphs use present tense ("are"). Elsewhere in the stratigraphy section, past tense is used; in the GPR section, the tense switches to present.

Thanks. We addressed these concerns throughout the paper and ensured that we generally follow present tense, with certain exceptions in the fieldwork section (2.3) when the verbs refer to our actions in the field. We also designate verbs that refer to the cores as past tense, since the cores no longer exist.

- Differentiate between latent and sensible heat. Only latent heat is mentioned throughout; meltwater (and rain) also introduce sensible heat, and in some locations latent heat is mixed up with sensible heat (latent heat -> phase change; sensible heat -> warming or cooling without phase change)

Thanks. We follow this suggestion, along with a similar suggestion by first reviewer, and include a more complete description on Line 67:

"Firn may warm through meltwater infiltration and through refreezing, which may generate latent and sensible heat exchange."

- Watch for language that attributes cause or implies causality where none has been proved. For example, the abstract says "decreasing firn cold content caused increasing meltwater runoff from the firn", but in the results, no causality (e.g., statistical analysis; physical argument) is put forth.

Thanks. We make sure to be more precise in our language that may imply causality.

In the Methods section, add more detail about the model (e.g., clarify what kind of bucket scheme was used), how the spinup was performed, and how the surface density was chosen.

The other reviewer had a similar request, and we have added additional text in Section 2.3 to provide more detail (please see above response to other reviewer).

- In general, the lightest colors in each plot, and particularly the lightest color dotted lines, are very difficult to see. I would recommend either making the lines thicker or use a darker color.

Fixed.

- There are some (though few) LWC measurements in firn, including Samimi et al. 2020 who measured change in dielectric permittivity in the firn and Heilig et al. 2018 who used upward looking GPR to measure liquid water content in the firn.

While dielectric permittivity is a great proxy for liquid-water content, Samimi et al. published only dielectric permittivity values for the Greenland Ice Sheet, and did not publish calculated liquid-water content. However, we note that Heilig et al. 2018 did publish volumetric liquid-water content values for the Greenland Ice Sheet.

We adjust our text in the discussion section to reflect the reviewer's concerns. We delete the sentence previously on Line 369. We then also add the LWC values from Heilig et al. 2018 to our list of comparisons. We also add another data point that we found from Wilson et al. (2019):

"Our measurements of LWC in the snow and firn are similar to LWC observations from previous studies for snow on the Juneau Icefield (e.g., volumetric LWC was 1.7%-5.7% in 2012, and 2.1%-16% in 2021 (Mannello et al., 2025); and a single measurement of 3.5% in 2018 Clayton (2022)). Seasonal snowpack, and snow and firn studies at other locations, such as Colorado and Idaho, US; the Greenland Ice Sheet; and Switzerland find similar magnitudes of LWC, including from geophysical observations (0-19% vol., Webb et al. (2018); 0-10 % vol. Heilig et al. (2018); 4-5 % vol., Heilig et al. (2015); and over 10 % vol., Bonnell et al. (2021)), and from similar measurement techniques to this study (i.e., Denoth meter and Snow Fork) (between 0-10 % vol.; Techel and Pielmeier (2011))."

In-text comments:

Line 5 – "during" instead of "through"

Thanks, fixed.

Line 9 - in the methods/results the cause of the increase in meltwater isn't directly attributed to decreasing firn cold content. It could be useful to partition the increase in runoff to a) increased melt b) decreased FAC (pore space) and c) decreased cold content

See previous response addressing this concern.

Line 13 – "capacity and storage"

Thanks, changed to: "Our results suggest that firn on the Juneau Icefield and other temperate alpine glaciers of Alaska will continue to lose long-term meltwater refreezing capacity and meltwater storage."

Line 20 – "melted (i.e, transformed into water that can raise sea level)instead of "vanished"

Thanks, we fixed to just "melted," as we reference global sea level promptly afterward and including it again we feel would be repetitive.

Line 29 - unclear what the which refers to (firn hydrology or glacier mass balance?

Changed to: "On a broader level, understanding firn hydrology is fundamental to characterizing glacier dynamics and glacier-mass balance, and thus may potentially impact the glacier's contribution to sea-level rise, downstream ecology, and regional water resources."

Line 31 – "on" not "in"

Thanks, fixed.

Section 1.2 - This is a good review, but adds length to an already lengthy paper. I would recommend keeping only the text in this section that relates or directly contextualizes the measurements/reported results. For example, firn aquifers are hardly mentioned in the results, but discussed at length in here.

Thanks. We acknowledge that the paper is long. We have eliminated all sections regarding the GPR, which has shortened the length. However, we believe that we need to keep the aquifer section in, as we do discuss firn aquifers and their presence on the icefield in the discussion ("4.4 Aquifers").

Line 40 – "understanding" not "paradigm

Changed to understanding.

Line 44 – "into not to"

Thanks, fixed.

Line 45 – “discharge as runoff”

Thanks, added this clarification.

Line 51 - meltwater can introduce sensible or latent heat -- sensible being heat that doesn't cause a phase change, and latent being heat that does cause a phase change; refreezing releases latent heat

Thanks for catching this. We added sensible heat to the sentence.

Line 53 – omit “that has been introduced”

Fixed.

Line 56 – omit latent and held

Thanks, fixed.

Line 57 – change “lowering” to “decreasing”

Changed to “decreasing.”

Line 59 – “reconfigure?” instead of “reform”

This word was part of the portion of the paragraph that we deleted in response to the comment regarding line 63.

Line 60 - invert these two, since first the layers have to form and then they can partition off pore space lower in the firn column

Reversed these two sections of the sentence.

Line 63 - confused by the sentence structure here. I think it can be simplified by combining it with the previous sentence, since they say very similar things

Changed to be more concise: “In addition, following high-melt seasons, ice layers and ice slabs may release latent heat during formation and/or section off portions of the deeper firn. These thermal and hydrological changes may influence subsequent interactions and responses of the firn to surface melting and densification processes, limiting deeper pore space from melt infiltration, funneling meltwater along gradients, and/or priming firn for firn-aquifer formation through latent-heat release (e.g., Culberg et al. (2021); Machguth et al. (2016)).”

Line 65 - This indicates a continuation of an argument, but this paragraph is giving us new context about firn aquifers

Thanks. Changed to: “On many alpine glaciers and on parts of the polar ice sheets, firn aquifers form in regions with high summer surface melt and high winter snow accumulation.”

Line 67 – omit “either” by “through”; replace “or” with “and”; replace “promotes” with “generates”

Fixed.

Line 68 - Firn aquifer requires thermal equilibrium and mass recharge if/as water is lost

Changed to clarify this: “Firn-aquifer formation may occur if both thermal energy and mass from local meltwater recharge balance or exceed the thermal energy and mass lost from the aquifer (i.e., from winter sub-freezing temperatures or drainage into crevasses (Miller et al., 2020)).”

Line 70 – omit “due”, insert “and attributed”

Changed to “attributed.”

Line 72 – omit aquifers

We are referring specifically to firn aquifers and therefore chose to keep this text in.

Line 73 – omit promote; indicate? "promote" implies that firn aquifers have some kind of feedback that causes more meltwater to be retained the larger the aquifer---this could be what you are arguing, but if so the argument should be clarified

Changed to: “because they retain meltwater in the firn and prevent immediate runoff.”

Line 76 – cannot

Thanks, fixed.

Line 78 - potentially useful here to also report values from Coleou and Lesaffre since those are the numbers used in the modeling

Coleou and Lesaffre use a density-dependent relation to define the irreducible water content, so it is not a single number. However, we add a sentence to clarify and address the reviewer’s concerns:

“The irreducible water content is the residual portion of water that is retained in the pore space of the firn due to capillary forces, and is approximately 6-8% water saturation or

2-3% of unit volume (Schneider and Jansson, 2004; Colbeck, 1974), though some studies define irreducible water content as a density-dependent relation (Coléou and Lesaffre, 1998)."

Line 79 - not sure what promoted means here - maybe rephrase to say that ice lens/layers can cause runoff to occur even if there is still available pore space in the firn column?

Promoted by is another phrase for "fostered by" or "supported by." We are referencing the fact that some refreezing processes like ice-layer formation actually may lead to runoff processes. We chose to keep this one word in.

Line 84 – "or"

We have a list of three, not two, so chose to not insert "or" between (1) and (2).

Line 87 - It would be useful to motivate with a reason for why this is important

To broaden the motivation, we add sea-level rise at the end of the sentence:

"Recent studies call for increased research on these Alaskan glaciers to understand firn hydrology, as well as other glaciological applications such as the improvement of firn-densification models and geodetic mass-balance calculations (Stevens et al., 2024), for more accurate sea-level rise estimates."

Line 140 - This is a good place to add additional information about the data shown in Figure 3. In that figure, it looks like all the cores are plotted at the same starting surface elevation, which would mean that ablation was ignored. On the other hand, if ablation was taken into account, it would help to plot those results so that the surface of each subsequent core is shown starting at the appropriate depth relative to the earliest-core's snow surface.

The reviewer had commented on this in the major comments section. We have addressed this concern previously in the document (please see above response). under the reviewer's previous comment and copy our response here:

Line 154 - This could be rephrased to make it clearer what you do, e.g., We calculate mean density using core sections that span the same depth relative to the surface of the first core drilled at each location (the core drilled earliest in the summer season).

Thanks, we took your suggestion.

Line 167 - This is confusing, can you rephrase? I think you mean that density wasn't measured before melt onset, so there may be liquid water in the density measurements, which could skew them higher than the true "dry" density.

Yes, that is correct. We rephrase to try to clarify: "or these calculations, we use measured density for ρ_s because of the availability of concurrent permittivity and density measurements and because of our inability to measure dry density prior to melt onset. We do this with the understanding that this may positively bias our LWC calculations."

Line 171 – "do not"

Thanks, fixed.

Section 2.1.5 - The GPR is not discussed in the results, and could be added (e.g., how continuous were the layers?) It is mentioned briefly in the discussion, but I recommend at least either mentioning them in the results, or indicating here why they aren't reported

We deleted the GPR sections in the methods, results, and discussion, based on the concerns of both reviewers. The GPR results were an integral component to the NSF proposal and fieldwork, but not to the interpretations in this paper.

Line 190 - Can you add a sentence explaining why you chose this reanalyses?

The other reviewer had a similar request, and we have added additional text in Section 2.2 to provide more detail (please see above response to other reviewer).

Line 194 - Is it correct that the model was only run at the core locations? Or was it run across the whole icefield but only reported at the core locations?

Yes, the model was run at the three core locations in the accumulation zone. We clarified this as: "We use the Community Firn Model (CFM) to simulate firn evolution at our three sites in the accumulation zone of the Juneau Icefield."

Line 198 - Would suggest using just "input" here and you can choose whether or not to describe the models boundary conditions (e.g., what happens to ice at the bottom of the model as firn accumulates and densifies, how is temperature set there, etc)

Thanks, fixed.

Line 200 - elsewhere you say you run the model between 1980 and 2019, can you clarify here?

Thanks, fixed to 1980-2019.

Line 203 - A "bucket" scheme generally just refers to the fact that percolation is instantaneous and different schemes have varying ways of handling impermeable layers. Some (like MAR) force some of the meltwater to become runoff when any ice layer is encountered. Others (like IMAU-FDM) ignore impermeable layers. How was it handled here? This seems very important to specify for this study and will impact the results

We have added additional detail in Section 2.4 “The Community Firn Model” specifying how we have configured the CFM and its bucket scheme:

“We use the CFM’s bucket scheme, one of many treatments of meltwater retention and flow within water-saturated firn layers. Bucket schemes implemented in firn-densification models are one of many treatments of meltwater retention and flow within water-saturated firn layers. In bucket schemes, water may percolate downward when the water exceeds the capillary capacity of the firn parcel and will stop either when the water reaches a firn parcel that has enough cold content and pore space for refreezing, or when there is an impermeable layer such as an ice layer and the meltwater runs off (The Firn Symposium Team, 2024). The remaining water from each parcel will continue to percolate to the subsequently deeper parcel until all water is refrozen or runoff. While there are limitations with using bucket meltwater percolation schemes, currently, more complex models do not reliably model firn meltwater processes better than approaches that use a simple bucket scheme (Vandecrux et al., 2020) and recent studies have used bucket schemes successfully in environments similar to our study area (e.g., Ochwat et al., 2021; Kindstedt et al., 2025).

We configure the model to treat ice layers with a density of 830 kg m−3 or greater and at least 10 cm thickness as impermeable, so that water that encounters such layers immediately became runoff. We set the irreducible water content to be dependent on the dry density, following Coléou and Lesaffre (1998). The CFM uses an enthalpy scheme to simulate heat transfer due to meltwater refreezing.”

Line 209 - useful to add this parametrization in, since LWC plays a large role in this analysis

Thank you for the suggestion; however, we decide not to include the parametrization as we feel its inclusion is unlikely to provide substantial insights to readers who are not already familiar with it and its use in models.

Line 213 – parenthesis around Figure 2

Thanks, fixed.

Line 213 - How was this value chosen? Were other values tested (e.g., sensitivity test), or was this value determined from surface measurements?

We added text in section 2.3 specifying that we chose it based on qualitative analyses. We use 350 kg m^{-3} for several reasons: (1) this surface density produced the best fit between the modeled depth-density profile and our observed depth-density profiles; and (2) through the melt season, the upper snow layer is removed and what is left at the surface is slight denser snow (~400 kg m^{-3}) (Figure A5) which is in line with our measured surface densities from the firn cores (Figure 4) and from published snow-pit density measurements (Mannello, 2023).

“We force the CFM with the CFSR surface mass-balance product from Ing et al. (2025) from 1980 to 2019 (Figure 2) and surface density of 350 kg m3. Qualitative analysis testing surface densities showed that 350 kg m3 produced the best fit between the modeled depth-density profile and our observed depth-density profiles. Normalized Root Mean Square Deviation (NRMSD) values between the modeled and measured depth-density profiles are shown in Table 3.”

Line 214 - A bit confused, is the spinup 150 years long using the average values of the 1981-1990 period? Could you clarify how the spinup is performed?

We have rephrased the text in section 2.3 to clarify how we perform the spinup.

Line 214 - the daily cycle can influence FAC and refreezing/runoff. Did you look at 3-hourly or 6-hourly output to see if these changed either value significantly, or is this constrained by the input data?

The time step is indeed constrained by the temporal resolution of the climate product, which is one day.

Line 228 - specify little - does that mean no significant long term change?

Yes, we meant no significant long-term change. We changed the text to reflect this.

Line 229 – omit “also”

Thanks, fixed.

Line 230 – omit “we find that”

Thanks, fixed.

Line 230 – melt-to-snow

Thanks, fixed this and inserted hyphens in similar compound modifiers in this paragraph.

Line 230 – “increased”

Thanks, fixed this and changed tense of other verbs in paragraph to reflect past tense.

Line 230 – at a rate of

Thanks, fixed this and added “at a rate of” to the last result as well.

Line 235 – omit “ubiquitous” across the and include “common to all”

Thanks, fixed.

Figure 2 - Color hard to distinguish from dotted/dashed lines, and dotted line is difficult to see. Also consider adding a legend for the locations

Fixed.

Figure 3 - was there a TKG4-C2 core? Add unit for depth. Can you clarify how these surfaces were corrected for ablation? Do they all start at the lowest surface depth (e.g., from latest core in the melt season)? Otherwise it seems to me that core profiles from later in the season should start below the surface of earlier cores, assuming only ablation occurred.

We address the concern regarding the TKG4_C2 core by adding clarification at the end of Figure 3 caption:

"Stratigraphy of the repeat firn cores at sites MLD, MG3, NWB1, and TKG4. All surfaces are corrected for ablation, except MG3, where no ablation measurements exist. Repeat cores were taken within 1-3 m of each other. The differences in stratigraphy show that even great local variability exists in snow and firn in high-melt environments. Large volumes of liquid water were found at the base of the TKG4_C1 core. Note that there is no TKG4_C2 core, due drill malfunction."

We have already addressed the ablation correction concern (please see previous responses).

Figure 4 - Missing modeled depth-density profile for (a)? If no modeled density at this location, mention in the caption

The TKG4 site is the in the ablation zone, and we only model firn in the accumulation zone (MLD, MG3, NWB1). We add clarification at the end of the Figure 4 caption:

"Measured density (blue lines), modeled depth-density profile for 1 June 2019 (dotted black line for accumulation zone sites), measured liquid water content (blue circles), and uncertainty estimates (shaded region) at the four sites. a) Site TKG4; b) Site NWB1; c) Site MG3; d) Site MLD."

Line 242 - Perhaps instead of events, use "days", since this makes it clearer what is being reported

We decide to keep "melt events" instead of "melt days" to differentiate between days that have melt and high-melt days ("events"), as many days have nonzero melt that is not substantial (> 0.5 mm per year).

Line 253 – omit "similar"

Thanks, fixed.

Line 254 – omit in and insert between; omit while

Thanks, fixed.

Line 256 – ranges

Thanks, fixed.

Line 257 – “for each” instead of “in the”

Thanks, fixed.

Line 260 - I am confused by this sentence. First, I am not sure what is meant by "mean" here -- could you clarify which measurements are averaged - were there multiple LWC measurements per core section? Second, by "for the core section we compared", do you mean between subsequent (later season) core sections at the same depth span? Could this be rephrased as "Mean LWC generally, but not always, increased at all depths as the summer season progressed"

We clarify that the mean LWC is the depth-averaged LWC and also take the reviewer’s suggestion:

“Depth-averaged LWC generally, but not always, increases with the summer season (Table 4): 27% increase in LWC at MLD; 71% increase in LWC at NWB; and 46.4% increase in LWC at TKG4. Site TKG4 had generally the highest LWC of all sites (>17 % vol. mean LWC for the core, with highest measurements at 34 % vol.).”

Line 263 – in Figure 4

Thanks, fixed.

Line 264 - It is a bit hard to understand what happened here, and may be worth expanding on in the methods section. It looks like from the plot that LWC was measured to 4m for the first core at this site, and then not measured for the subsequent cores. I am not clear about how the drill (presumably, for the core drill) being stuck affects the ability for LWC to be measured.

The instrument from which we collect LWC is called a WISe sensor, which can be inserted into the recovered core. The drill is not the limiting factor here, but the ability of the WISe sensor is, as it cannot be inserted into high-density firn. We explicitly state this in Section 3.2.2, but add “of the core sample” in case this clarifies the methods here more:

"LWC measurements were collected approximately every other sample for each core (approximately 40 cm depth spacing) and were collected until the density of the core sample became too high (approximately 550 kg m−3) …"

Line 271 – "was only intermittently and qualitatively noted"

Thanks, fixed.

Line 281 – before it ends around 13 m. Omit next sentence.

Thanks, fixed.

Line 283 - just above or the whole section above the snow-firn transition?

The large grains were observed in the 2-4 m above the firn-ice transition. We clarified this.

Line 284 - were present/ were identified

Thanks, fixed to "were identified."

Line 292 - of each sample? of the auger?

We clarified "at the base of the drill."

Line 300 – identified

We deleted the GPR section in which this comment was focused on.

Line 313 - The reported numbers seem to indicate that the *rate* of firn thinning is increasing, in addition to an overall thinning -- useful to make clear, and if so could be good to report overall rates first, and then comparison between early in the model run and later (e.g., 1980-1989 vs 2010-2019). Was there a specific reason 2012 was chosen as a cutoff?

We clarified that we report percent change, following other firn studies. We also decide to calculate relative changes in the last decade (2010-2019) as well as the full analysis period (1980-2019).

Figure 5 - I can only identify two colors in the temperature plots. Label or mention the white line (presumably the firn/ice transition) in the caption. It also could help to add a column labels on the left hand side of the figure indicating which core the plots belnog to

We describe in the caption where the red color on the temperature plot is. To further clarify, we more explicitly delineate that it is plotted along the 0 degree C isotherm. We clarify that the white line is the bubble close-off depth:

“Modeled profiles of density and modeled firn temperature at 3-m depth (light orange), 5-m depth (dark orange), and 8-m depth (red - plotted along the 0 °C isotherm) through time from Kuipers Munneke (2015) firn-densification model at the MLD site (a and d); MG3 site (b and e); and NWB1 site (c and f). Bubble close-off depth (i.e., 830 kg m−3) is delineated as the white line on plots a-c.”

Figure 6 – “integrated”; The lightest color (MLD) is a bit hard for me to see; perhaps instead of different colors for each plot, you could use the the darkest or middle color from each plot and keep the color for each of the sites the same

Fixed.

Line 338 – omit less than fewer

Thanks, fixed.

Line 340 - You could remove this first section, which summarises the motivation for the paper and the results. Instead, you could use this to discuss any relationship between the results, or introduce the main points in your discussion

We thank the reviewer for the suggestion but we value summarizing the motivation for the reader’s clarity as they begin the discussion section. Following the revisitation of questions, we introduce the main results and interpretations which are expanded on in the rest of the discussion.

Line 350 - cold content decrease was given a minus sign, but here decrease in meltwater refreeze was not; check that the use of minus signs to indicate negative trends is consistent.

Fixed.

Line 351 - the rise in ELA doesn't "cause" a deceased mass balance, but is indicative of it (i'd attribute the cause here to the rising temperatures)

Thanks, fixed to clarify this: “rising atmospheric temperatures are causing increasing ELAs and decreased mass balance.”

Line 259 – omit “with little long term changes in rainfall”

Deleted.

Line 364 – was

Thanks, fixed.

Line 366 - which locations? alaska? other alpine glaciers?

We clarify which locations and what kind of studies:

"Our measurements of LWC in the snow and firn are similar to LWC observations from previous studies for snow on the Juneau Icefield (e.g., volumetric LWC was 1.7%-5.7% in 2012, and 2.1%-16% in 2021 (Mannello et al., 2025); and a single measurement of 3.5% in 2018 Clayton (2022)). Seasonal snowpack, and snow and firn studies at other locations, such as Colorado and Idaho, US; the Greenland Ice Sheet; and Switzerland find similar magnitudes of LWC, including from geophysical observations (0-19% vol., Webb et al. (2018); 0-10 % vol. Heilig et al. (2018); 4-5 % vol., Heilig et al. (2015); and over 10 % vol., Bonnell et al. (2021)), and from similar measurement techniques to this study (i.e., Denoth meter and Snow Fork) (between 0-10 % vol.; Techel and Pielmeier (2011))."

Line 369 - Samimi et al. 2020 measured change in dielectric permitivity in the firn Heilig et al. 2018 uses upward looking GPR to measure liquid water content in the firn

Included.

Line 371 – indicates

Thanks, fixed.

Line 373 – drains

Thanks, fixed.

Line 376 - this can be higher in higher density firn

Yes, irreducible saturation may increase with increasing density. But we measure high saturation even in the relatively low-density snow and upper firn.

Line 380 – modulate

Thanks, fixed.

Line 385 - Was the water table visible in the TKG4 cores visible in the GPR? This is something that could be reported in the results, or else could be useful to discuss the use of (or inability to use) GPR for determining spatial variability.

We decided to omit the GPR section given previous concerns from the reviewer and reviewer #1, and therefore decide to omit discussion of the GPR results in regard to this question.

Line 389 - Potentially a typo, not sure what

Omitted "great" and rewrote as: "The differences in stratigraphy of each of the repeat firn cores at a single site highlights that considerable spatial variability exists ..."

Line 406 – rather

Thanks, fixed.

Line 411 - This read a bit awkwardly to me, since cold content is a value/measure, not a feature. Maybe something like "Firn cold content accumulated seasonally, and the firn was fully temperate (T=0C) by early summer each year."

We took your suggestion and rephrased as: “From 1980–2019, firn cold content accumulated seasonally in the upper 20 m, and the firn was fully temperate (T= 0^{o}C) by early summer each year.”

Line 415 - Firn buffers sea level rise (or overall mass loss from the icefield). This sentence could be rephrased to something like"... the lack of cold content limits the ability of warm, temperate firn to buffer against meltwater loss/sea level rise through refreezing"

Fixed to: “The lack of cold content underscores the limited capacity of warm, temperate firn on the Juneau Icefield, and similar glaciers in Alaska, to buffer against meltwater loss to sea-level rise through refreezing.”

Line 417 – omit “consequently”

Thanks, fixed.

Line 418 – “to a runoff”

Thanks, fixed.

Line 421 – Omit “however”; omit “then”

Thanks, fixed.

Line 422 - all liquid water becomes runoff by

We decided that this alternate wording would cause confusion as to whether runoff is a verb or noun. We kept our original wording.

Line 424 – runs off

Thanks, fixed.

Line 424 - decreases (just a grammar thing - the form "is decreasing" or "is doing" indicates that the process is ongoing, but it is limited by the length of the model run (through 2019). This is something to look for throughout the paper.)

Thanks, fixed.

Line 425 - clarify what you mean by don't change reversibly - perhaps symmetrically?

Yes, we meant symmetrically. Changed to “symmetrically.”

Line 434 – “agree”

Thanks, fixed.

Line 435 - “from the firn at our sites”

Added “at our sites” to the end of the paragraph.

Line 434 – “which instead of that”; omit “in particular”

Thanks, fixed.

Line 446 - If there is water year round, perhaps not transient, just fluctuating in liquid water content?

Yes, good point. We do not know whether the water is year around, though LWC values are high even before the main melt season starts (some > the irreducible water saturation (2-3 % vol)), suggesting that some liquid water may remain through the year.

To account for this possibility, we omit “transient” and explain that it is possible that liquid water remains in the snow and firn through the year, though we cannot confirm because we have no measurements through the winter.

Line 457 – omit “we suggest that”; insert “results could apply”

We took your suggestion: “Because a significant area (approximately 83%) of the Taku Glacier of the Juneau Icefield lies on the low-slope plateau in the accumulation zone and firn zone, our results could apply to the majority of the Taku Glacier.”

Line 457 - This is quite general. Other locations within the same elevation band? The entire accumulation zone of the icefield?

We clarify that this refers to the accumulation zone only: “Thus, significant decrease in refreeze capacity and increase in runoff in recent years may have occurred and may continue across the accumulation zone of the Taku Glacier and possibly the greater accumulation zone of the Juneau Icefield.”

Line 458 – “Thus, significant”; insert “likely has occurred”

Thanks, fixed.

Line 459 – omit to occur

Thanks, fixed.

Line 464 – omit the change in;

Thanks, fixed.

Line 464 - by 2.1-4.4 10^8 m3 depending on location, or from 2.1 to 4.4 x 10^8 m3?

We meant ‘by.’ Fixed, to insert ‘by.’

Line 464 – indicates that downstream hydrology may shift significantly as firn on the Juneau Icefield and other temperate alpine glaciers changes

Changed to your suggestion.

Line 469 - Is this a term you are proposing, or does it come from hydrology? Could be potentially useful to draw from research where seasonal snowpacks are changing hydrologic regimes, which would result in something similar or look at rain-on-snow events. Also, if there is any data or studies on downstream hydrology of the Taku or the Juneau Icefield, it would be useful to reference them here.

We have addressed the reviewer’s previous comment regarding this concern. Please see our previous response.

Line 472 - becomes runoff or runs off

Thanks, fixed.

Line 475 – omit last sentence

Thanks for the suggestion. Fixed.

Line 479 – “will accelerate”

Thanks, fixed.

Line 480 – omit “continued and accentuated changes” to “feedbacks”

Thanks, fixed.

Line 480 - exacerbate? or just continue?

Fixed to: “Future icefield-wide increases in atmospheric temperature will cause increasing surface melt and rising ELAs, which will accelerate mass loss due to hypsometrically controlled feedbacks because of the plateau-like nature of the Juneau Icefield (Ing et al., 2025; Davies et al., 2024)."

Line 484 - Useful to say explicitly that warmer temperates increase firn densification rates, decreasing pore space (and therefore thinning the firn and reducing FAC). Denser firn is harder to cool because more latent heat is required.

Thanks. We include a more explicit statement to clarify to readers:

"Firn warming is less reversible than firn cooling, due to the nonlinear effects of temperature on firn-densification processes that increase densification, decrease pore space and decrease firn thickness. This effect is especially accentuated by latent-heat release due to meltwater refreezing (Thompson-Munson et al., 2024). Therefore, the likelihood of regenerating cold content in systems such as the Juneau Icefield, where long-term firn warming has occurred, is low."

Line 485 - "the likelihood of regenerating"; change "from" to "in"; "systems?" instead of "locations

Thanks, fixed.

Line 486 – "has occurred" instead of "is occurring"; replace "limited" with "low"

Thanks, fixed.

Line 508 – spanning

Thanks, fixed.

Line 510 - Are there particular kinds of measurements or locations/regimes of particular uncertainty?

We clarify:

"We also suggest that continued study of the evolution of firn on temperate alpine glaciers in Alaska is needed, including understanding the heterogeneity of meltwater processes, especially in high-melt climate regimes like the Juneau Icefield. Due to a lack of field measurements at large, and/or the limitations of point measurements, firn models are commonly used to determine firn evolution over wide spatiotemporal scales."

Line 510 - I would move this sentence to before "Therefore" on line 507

The first paragraph is designed to address future work specifically related to the Juneau Icefield. The second paragraph is designed to address future work at large. Instead, to address the reviewer's concerns, we attempted to clarify this distinction in the text:

“While the Juneau Icefield is currently one of the most studied glacier systems in the world, there has been a lack of measurements spanning the entire firn column and its hydrology. This study is the first to characterize the evolution of the icefield’s firn hydrology. However, our study has spatial and temporal limitations, as it focuses primarily on the firn evolution at four sites, with field data collected in June and July of 2024 and modeling which spans 1980-2019. Therefore, further investigation of the spatial and temporal variability of the firn on the Juneau Icefield is needed, in particular: (1) in-situ measurements during a full summer season; and (2) more measurements across the icefield to constrain spatial variability.

We also suggest that continued study of the evolution of firn on temperate alpine glaciers in Alaska is needed, including understanding the heterogeneity of meltwater processes, especially in high-melt climate regimes like the Juneau Icefield. Due to a lack of field measurements at large, and/or the limitations of point measurements, firn models are commonly used to determine firn evolution over wide spatiotemporal scales.”

Line 517 - cite the GlaMBIE study https://www.nature.com/articles/s41586-024-08545-z

Fixed, and changed percentage from 25 to 22 to reflect the results of the GLAMBIE study.

Line 521 – “during” instead of “through”

Thanks, fixed.

Line 524 - could rephrase: "decreasing firn thickness and increasing firn densities led to a decrease in firn air conten of between 22% and 35% at our sites."

Thanks. We decided to keep our sentence structure but remove the duplicate “due to”:

Line 525 - I am not completely convinced still that the decrease in the number of days with high cold content is solely responsible for the increase in runoff and earlier regime transition --- decrease in FAC and thinning firn packs (resulting in less pore space for water) likley also played a role. If this is your argument, I think it needs to be strengthened by showing correlations between cold content and runoff in the results or discussion.

We have addressed the reviewer’s similar previous concern. Please see our previous response.

Line 530 – “storage and refreezing capacity”

Thanks, fixed.

Line 530 – omit continue to

Thanks, fixed.

Figure A2. Hard to tell from this plot, but does the data from 2019 indicate that all sites were in the ablation zone then?

The year 2019 was a particularly high-melt and low-accumulation across all sites. We suspect that NWB1 site has possibly entered the ablation zone in 2019 and was near the ELA, but we cannot state whether NWB1 has consistently entered the ablation zone post 2019.

Editor's Suggestions

We thank the editor for their suggestions, which has further improved the quality of the manuscript.

The presentation quality is generally high, with a concise and well written text that is easy to follow. In a few instances I felt that the presentation had the potential of being further improved, though. As examples: (1) Table 1, for example, could have provided some additional information on, e.g., how the 13 firn cores were distributed over the 4 sites, at which dates they were taken, and which depths they reached.

The core sampling dates, alongside mean density and LWC, are detailed in Table 4, while Table 1 is simply a brief overview of the sites. We do this because we want to show these details in the results section. We make this clearer by adding a statement in Table 1: "Core sampling dates, along with mean density and LWC, are shown in more detail in in Table 4"

(2) The fact that the cold content represents the energy necessary to heat the firn to the melting temperature is repeated multiple times (e.g. L. 51, 103, 216).

Fixed.

(3) Equation 5 has some undefined variables and minor formatting issues (here, I actually wondered whether some text may have been accidentally truncated after the equation?).

Fixed.

(4) The Results (L. 222) start presenting findings for "the three study sites", while L.187 told there would be four sites.

Clarified that there are three sites in the accumulation zone for which we run the model for (MLD, MG3, NWB1), and four sites total that we took measurements at (MLD, MG3, NWB1, and TKG4). This is described in Figure 1 caption as well.

(5) Out of the 12 Figures in the Appendix, only two are referenced from the main text, with one reference (L. 229) even being unresolved. This makes the supplementary figures somewhat “lost”.

Fixed. Referenced appendix figures more.

(6) Figure 3 misses a definition of the depth axis (I guess it is “meters”?)

Fixed.

(7) Section 3.2.4 “Ground-penetrating radar” (within the Results chapter) does not present any figure, which is surprising when considering that the results are described in words.

We deleted the GPR section in response to concerns from the two reviewers.