Reviewer #1

The authors did a good job of responding to my initial comments and overall, I think the manuscript is now suitable for publication. I would encourage the authors to do a very careful proofread of the text and particularly to double check all of the references to figures, some of which I think have gotten a bit mixed up in the revisions. It would also be good to double check that figures and tables are all in the order in which they are referenced in the text. Below are a few errors of these types that I caught, but I'm sure I missed some as well:

We thank the reviewer for taking the time to review our paper again. Their comments here have helped to improve the paper through technical changes and clarification.

Equation 5: I think it would be most correct to integrate from -20 m to 0 m (rather than 20 m to 0 m) so you don't get a negative cold content. Also I'm not sure the j subscript makes sense for a continuous integration rather than a discrete summation (though I guess in practice you are doing a discrete summation over the core segments?).

Fixed to -20 m. We leave the j subscript due to the reason suggested by the reviewer.

Line 324: I think this should be a reference to Fig. 5d-f instead of Fig. 4d-f

Thanks, fixed.

Line 352: I think this should point to Fig. A10 rather than Fig. A2

Thanks, fixed.

Line 380: The decrease in accumulation rate seems off by an order of magnitude compared to what is shown in Fig. 3 and probably it should be Fig. 3 rather than Fig. A3 that is referenced here.

Fixed.

Reviewer #2

Thanks very much to the authors for all of their work thoroughly responding to the reviews. I have some minor comments. I otherwise recommend this article for publication, which makes a good contribution to the literature on wet firn measurements and modeling in a particularly wet and warm environment (the Juneau Icefield).

Thanks very much to the authors for all of their work thoroughly responding to the reviews. I have some minor comments. I otherwise recommend this article for publication.

We thank the reviewer for taking the time to review our paper again. Their comments here have helped to improve the paper through technical changes and clarification.

Minor comments:

1. Somewhere in the introduction of the model and/or the discussion, it would be useful to explicitly acknowledge that the modeled timeseries and the observations do not overlap and perhaps give a justification for this (e.g., section 2.2)

To address the reviewer's concerns, we clarify the modeled dates at the end of the introduction.

We also clarify the temporal limitation of the modeled input and output in Methods section ("2.4 The Community Firn Model"):

"We force the CFM with the CFSR SMB product from Ing et al. (2025) over 1980 to 2019 (Figure 2). Consequently, the model outputs are confined to this temporal range and do not temporally overlap with our field measurements which were taken in June and July 2024."

We already provide a thorough justification of using this climate product (in section "2.2 Climate") and therefore do not provide additional justification.

2. Line 211 states that any meltwater (except for irreducible LWC) that doesn't refreeze becomes runoff, which subsequently implies that no water can be retained beyond the irreducible water content and therefore no firn aquifers can form. This is true for some firn models (e.g., the IMAU-FDM) but seems at odds with findings later in the paper that a persistent firn aquifer forms. It would be useful to clarify whether this model is capable of producing firn aquifers, and ensure this aligns with any conclusions drawn from Figure A6. All the values in A6 are within the Colóeu and Lesaffre values for irreducible, density-dependent LWC (their Figure 4). This suggests, however, that this figure shouldn't be used as evidence of firn aquifers (as in line 464), though it could be used as evidence of persistent liquid water (temperate firn near the firn/ice transition) year-round.

Thanks. We clarify the reasons why the model results supports the potential for firnaquifer formation. We add some clarification in Section 4.4 Aquifers:

"… and (4) modeled results showing interannual liquid-water retention at all sites, indicating the potential for long-term liquid-water storage (Figure A6 and A7). In the model runs, these high LWCs result from the irreducible saturation predicted by Colóeu and Lesaffre parametrization, around 20% for densities near the prescribed impermeable density of 830 kg m^-3. The model results demonstrate that there is inadequate cold content to refreeze water and that the firn is capable of retaining meltwater at depth. This, along with other lines of evidence (1-3), supports our hypothesis that aquifers may be present on the Juneau Icefield."

To visualize our modeled LWC results more clearly, we edit the colorbar limits of Figure A6 so that the colormap does not completely saturate. We alsp change the colormap so that readers may be able to discern high LWC layers more easily:

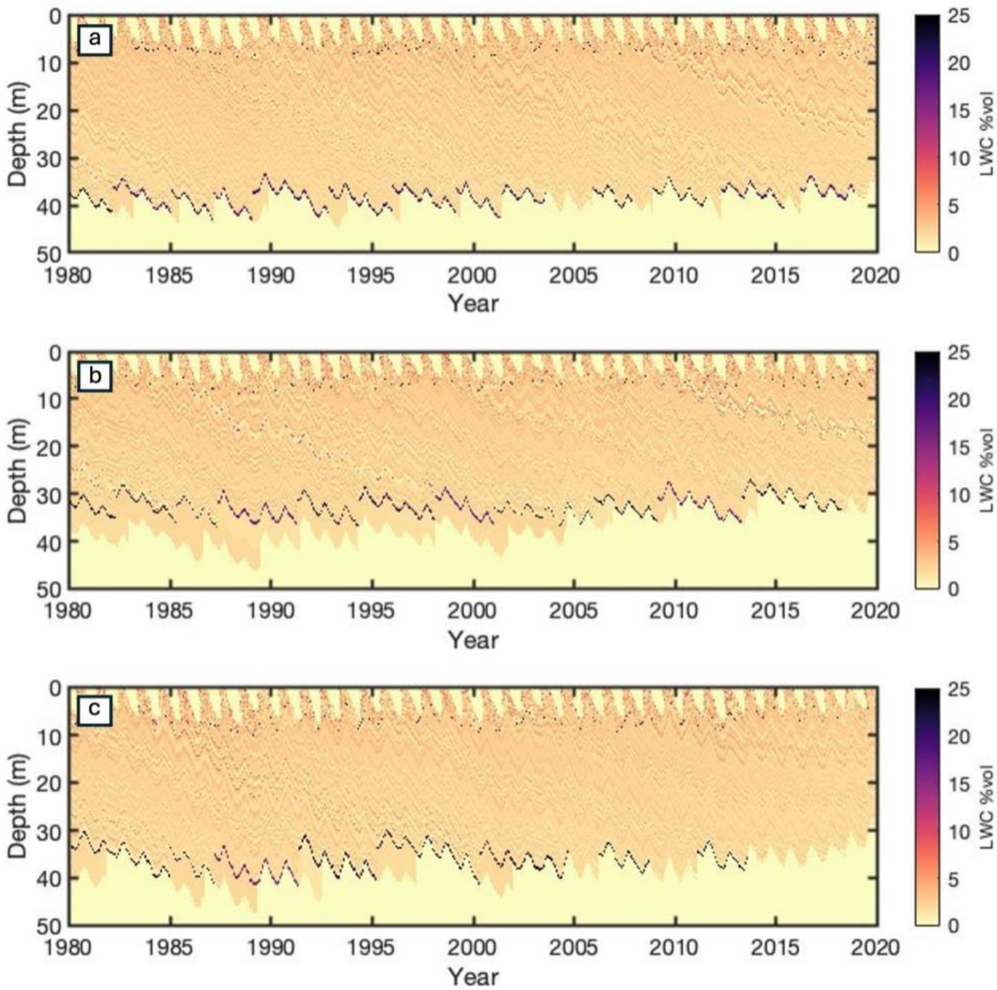

Figure A6. Modeled liquid-water content (LWC) evolution for the three sites: a) MLD, b) MG3, and c) NWB1.

We also plot mean annual LWC profiles and add this figure to the appendix:

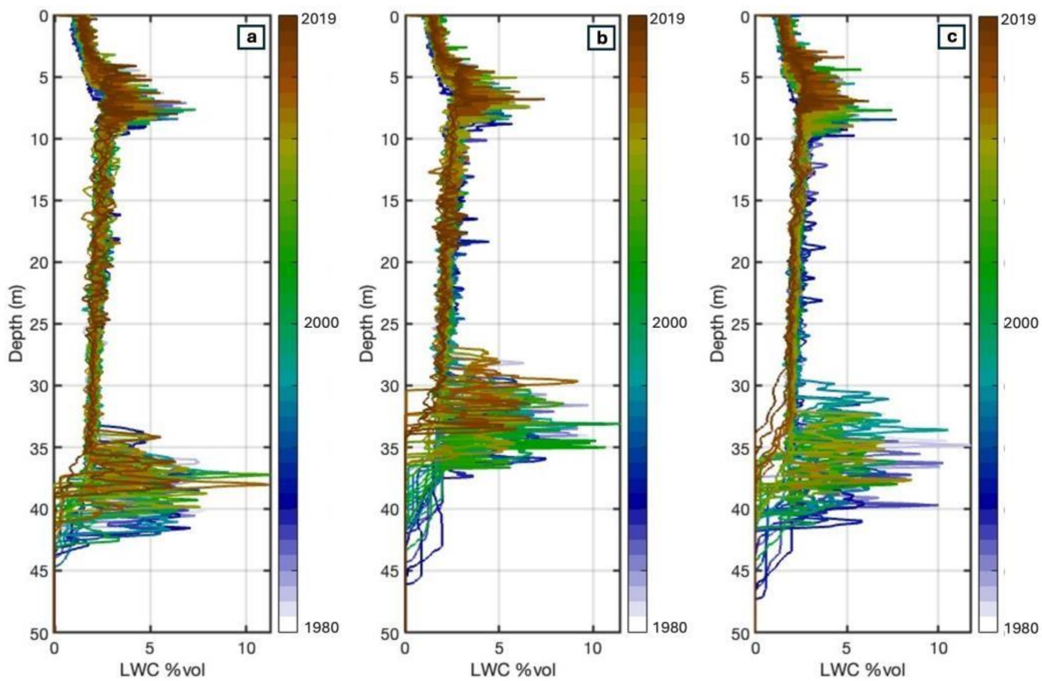

Figure A7. Modeled mean-annual liquid-water content (LWC) profiles for the three sites: a) MLD, b) MG3, and c) NWB1.

In addition, we clarify the modeled LWC in the text:

"Modeled annual depth-averaged liquid-water content (LWC) (Figure 7a-c) decreased at a rate of 2.4 (MLD), 6.2 (MG3), and 7.2 (NWB1) cm i.e. per decade. Model results also suggest high-LWC layers are retained inter-annually (Figure A6 and A7) at or near bubble close-off and in the shallow firn for all sites. Deep layers of high LWC at NWB1 disappear from 2014-2019."

3. (Lines 329-339) – I am unclear why a percent change (essentially, the difference between the first and last time steps) is a better indicator than a linear regression here. I would suggest either using something like time series decomposition to separate seasonal signals and an overall trend, and/or using change point detection to determine when the timeseries experiences a behavior change (e.g., around 2002 for the MG3 and NWB1 depths to 830 kg/m3 in Figure 5).

Thanks. We calculate the linear regression for FAC as well as firn thickness, and revise Figure 6 to reflect this.

We decided to omit the mean density for this figure, because the linear regression showed low R^2 values and was not integral to the narrative or conclusions of the paper. We consequently omitted discussion of the mean density changes in the discussion.

We also delete the two paragraphs that included the original description of Figure 6 in the results, and add a paragraph to include the linear regression results in the paper:

"Modeled mean annual firn-column thickness (defined here as from the surface to bubble close-off depth) decreased at a rate of 1.2 m (MLD), 3.3 m (MG3), and 2.6 m (NWB1) per decade from 1980 to 2019 (Figure 6a; Table 4). Additionally, modeled mean annual firn-air content (FAC) decreased at a rate of 0.5 m (MLD), 0.8 m (MG3), and 0.8 m (NWB1) (Figure 6b; Table 4) per decade from 1980 to 2019."

We also edit these values to reflect the new calculations in Table 4 and Table A2.

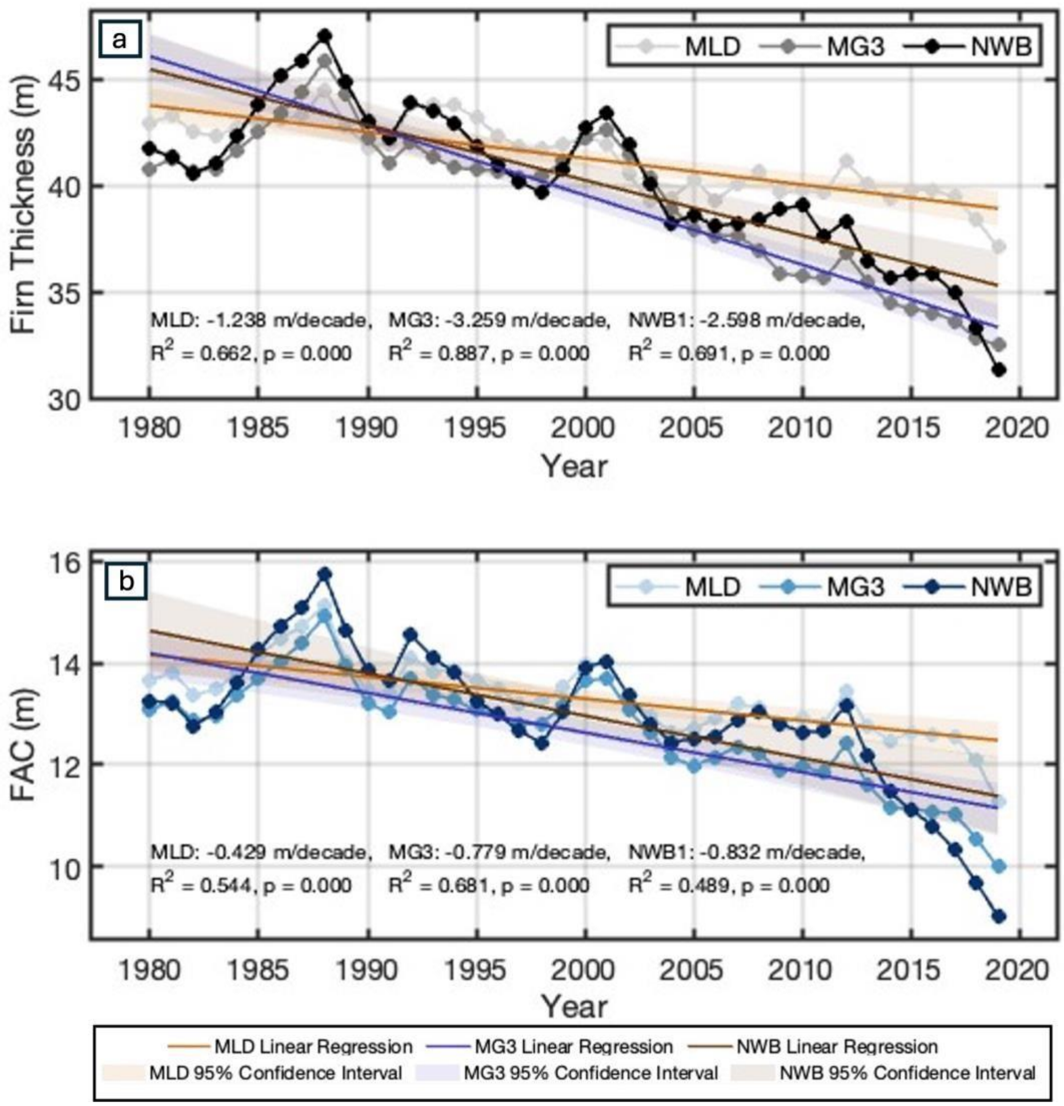

Figure 6. Model results from the Kuipers Munneke (2015) firn-densification model: a) mean annual firn thickness; and b) mean annual firn-air content (FAC).

4. In Figure 5, the 8-m--depth temperature seems to always be 0 ℃. Since it is quite difficult to tell the difference between the red and dark orange, I would suggest the authors remove 8-m depth and just state that it is almost always at 0.

Thanks. Note that in the Figure 5 caption, we already state the 8-m depth temperature: "The temperature at 8-m depth was always at or very near 0 °C over this interval, see Figure A5."

We clarify that it is indeed at 0 °C: "The temperature at 8-m depth was always at 0 °C over this interval, see Figure A5."

5. I suggest that any value reported as meters ice equivalent be converted to meters water equivalent (e.g., cumulative runoff reported in lines 339-340), or at least that the density used for ice equivalency be explicitly stated.

We clarify the meaning of m. i.e. in the Methods section:

"Where applicable, we report model input and output in units of meters of ice equivalent (m i.e.; multiply by the ratio of the density of glacial ice to density of water, 0.917, to convert to m w. e.)."

6. In Section 4.5, I would suggest the authors include a reference of some estimate of how much water runoff from the Taku contributes to downstream hydrology (e.g, % volume in summer months, if this exists). This would strengthen the argument that changes in melt refreeze/retention affects downstream hydrology.

Note that we already calculated the percent of the Taku's total ablation that the firn runoff constitutes, see current line 489:

"This estimate is approximately 32-72% of the total ablation over a single year for the Taku Glacier, assuming the area-averaged ablation rate of 0.83 m w.e. yr−1; McNeil et al. (2020)) over the area of the Taku Glacier (725 km2; McNeil et al. (2020))."

7. In Figure A7, why does the April dry density start at 4 m? I would expect the July depths to start lower relative to the April surface due to ablation. In Figure A8, I suggest the authors use the colormap used in A9 so the different years can be more easily identified.

In Figure A8 (previously Figure A7), the dry density and the (dry + liquid) density for April are the same until 4 m in depth. We clarify this in the figure caption:

"Modeled depth-density profiles for April and July of 2019 at Site MLD, showing the ablation of the snow layer, and the dry and wet densities. Modeled dry and wet densities for April are the same until about 4-m depth."

We also change the colormap in Figure A9 (previously A8) to the same colormap we use in A9, to highlight the different years more clearly.

Line edits:

20: Is the sentence starting with “By 2100” trying to convey that the loss of 30% of Alaskan ice as measured in 2015 will contribute 17 mm of sea level rise? Perhaps could be rephrased “By 2100, Alaskan glaciers are expected to lose 30% of their 2015 ice mass/volume, raising sea levels by 17 mm…”

Changed to: “Between 2015 and 2100, approximately 30% of glacier ice in Alaska is expected to contribute to 17 ± 4 mm of global sea-level rise under emission scenario RCP 4.5 (Hock et al., 2019).”

61: suggest removing “via latent heat release” as it is unclear in the sentence as it stands what this phrase refers to

We leave this phrase in the text and change “via” to “through” in case that makes more sense to readers. The “and/or” comes in between each process after a comma, which should section each idea separately: “These thermal and hydrological changes alter the firn’s densification and response to surface melt by limiting deep 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)).”

78: suggest stating here that you use the Coléou and Lesaffre formulation (where the %LWC can range from ~5-15%)

We prefer to introduce the Coléou and Lesaffre formulation in our methods section, as first-time readers would not yet know of the study details (including the methods) of this paper.

102: suggest using “pore space” instead of porosity

Fixed.

103: as you mention in your response to the reviewers, useful to add a clause here “and available pore space exceeds the total melt volume by a factor of 5”

We change to: “While firn volumes and therefore available pore space are decreasing on the icefield, we expect that cold content is the primary limiting factor for refreezing meltwater because the snow and firn are nearly isothermal, and because the available pore space is much greater than the total melt volume.” We do not specify the factor, as this is part of the study results and first-time readers would not yet be aware of these results.

117: confirming that TKG4 had *firn* in 2024 even though it is/was in the ablation zone?

Yes, that is correct. The firn is ablating at this location but has not been entirely removed yet.

Figure 1: add reference to the satellite imagery used

These products primarily used LANDSAT-5, LANDSAT-7, SPOT-5, and Terra satellite data. This information is stated within the reference we provide, as it is a Randolph Glacier Inventory (RGI). Details about the data, including the data description, satellite data used, sensor details, and coverage are included in the reference's site. As such, we decide to leave the reference as is. 121-122: remove extra references to NWB1

Fixed.

134: not sure what is meant by "daily climate changes"… maybe interannual/decadal climate trends and seasonal firn evolution?

Changed to: "We use the CFSR climate output from Ing et al. (2025) to determine decadal to interannual climate changes, as well as input to the Community Firn Model."

204: the phrase starting with "as well as" is ambiguous—do you mean dfferent components of the firn model? If referring to other firn models not used in this paper, I think you can remove this phrase

Rephrased to: "Input to the model includes variables such as snow-accumulation rate, skin temperature, melt rate and surface-snow density."

170: possible typo -- df=0.1 for f=0.8 or f≥0.8?

Thanks for noticing the typo. Corrected to: "df = 0.1 for f ≥ 0.8 and df = 0.2 for f < 0.8"

206: remove the sentence starting with "Bucket schemes" as it rephrases the previous sentence

Removed.

207: Here is a suggestion for a way to rephrase this sentence, as not all bucket schemes include ice-lens impermeability – some, like MAR, say 30% of meltwater at an ice lens of a minimum thickness becomes runoff; others, like the IMAU-FDM, ignore ice lens impermeability and allow water to go through/ignore ice lenses. "In bucket schemes, during each time step, water moves downward instantaneously when the volume of water exceeds the capillary capacity of the firn parcel, and may be refrozen upon reaching a firn parcel with enough cold content and pore space for refreezing, and/or partially or entirely "become" runoff when an impermeable layer like an ice lens is reached."

Thanks for the suggestion. Because we wish to describe how the scheme works in the CFM, we refer to its specific meltwater scheme: "We use the CFM's bucket scheme, one of many treatments of meltwater retention and flow within water-saturated firn layers. In this bucket scheme, 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 in which case the meltwater runs off."

253: 2004 was also common to all three sites

Thanks, fixed.

284: not sure which two cores are being referenced

We clarified: "Grain size was only intermittently and qualitatively noted on all cores when significantly large or fine, and therefore we do not provide a detailed description of grain transitions."

304: unit missing in "up to 4.5-5.5 thick"

Fixed to: "4.5-5.5 cm thick."

328: clarify what it means to use the mean annual FAC to "smooth" the high seasonality variability

This paragraph was omitted in response to the reviewer's concerns regarding the FAC linear regression.

341: add some context for why an LWC of > 10% vol is meaningful/significant

We meant to emphasize that the LWC values we see in the model are within the firnaquifer range of LWC. Therefore, we change to:

"Model results also suggest that layers of LWC of 5-25 \% vol are retained interannually."

377-380: some issues with verb order in the list which makes the sentence difficult to parse e.g., instead of "increased melt" should be "melt increased"

Fixed to: "Based on our results, these changes have likely driven several primary impacts on the Juneau Icefield's firn: (1) thinning and widespread disappearance of the firn layer, especially since 2012; (2) increased density of the firn column and reduction of total firn-air content; and (3) a decreased capacity for meltwater refreezing and increased meltwater runoff."

393: Fig. A9 does not show an increase of depth-averaged density and liquid water content between June and July 2024. Fig A7 shows an increase between April and July 2019 but this is a different set of months during a different year.

Thanks. We clarify by omitting our reference to modeled results on line 393, to eliminate the mismatch in months that are referenced.

395: The mention of irreducible water saturation as 2-3% vol. is at odds with the physics used in the firn model (Coléou and Lesaffre); these numbers are definitely up for debate in the community, but potentially worth noting

We tried to clarify: “Most of our LWC measurements are higher (and at times, substantially higher) than the traditionally accepted definition of the irreducible water saturation (2-3 %), which suggests that the water flux out of the firn is not as rapid as its generation, either because the snow and firn do not have high enough permeability, and/or because the hydraulic head is not high enough to accommodate the meltwater flux into the system.”

423: Firn thinning doesn’t necessarily increase mean firn-column density (e.g., [12345] and [135] have the same mean), would remove the implied causality

Thanks, fixed to: “Further, mean firn-column density increased from 1980-2019 (3-5%) and firn-air content decreased (22-35%).”

476: Is there an updated value for the Taku accumulation zone % or is 83% likely still a good estimate despite the rise in ELA?

This is the last published accumulation-zone estimate. Given that this is an idealized calculation, and there are no other published estimates, we decide that this is a sufficient estimate to use in our idealized calculation.

483: Specify whether the runoff is reported in m3 of ice or water equivalent.

Specified as ice equivalent.

499: Rephrase opening to make sentence clearer: “Future increases in icefield-wide atmospheric temperatures”

Fixed.

530: the sentence starting with “In addition…” seems out of place here, referring to firn models in general (?). This could be contextualized by saying that in-situ measurements across different seasons (e.g. in the winter) would help constrain firn models, which mostly rely on summertime measurements; otherwise I’d recommend removing this sentence

We moved the sentence to the next paragraph, which speaks more to the constraints of using firn models and future improvements related to firn modeling.

Editor

We thank the editor for taking the time to provide input. Their comments here have helped to improve the paper through technical changes and clarification.

Abstract, Lines 5-6: The sentence mentions an “increase of up to 5% and 71% during the 2024 summer season”, but does not specify the baseline against which this increase is assessed: is it an increase w.r.t. the beginning of the summer, or an increase since the 1980s, which seems to be the baseline for all comparisons that follow? Please clarify.

The increases are in reference to our first measurement of the 2024 summer season. We changed to clarify: “First, we found that measured mean density and liquid-water content generally increased up to 5% and 71%, respectively, during the 2024 summer season relative to our initial summer measurements. This indicates meltwater retention in the snow and firn and reveals the potential for local or regional aquifers.”

L. 39: Please use consistent capitalization for the section headings. Here, all words of “Firn Hydrology” are capitalized, while at e.g. L. 141 (“2.3 Firn cores”) there is no such capitalization. The Cryosphere uses sentence-style capitalization (i.e. capitalization of first word and proper nouns only), cf. https://www.the-cryosphere.net/submission.html
Thanks, fixed all section headers.

L. 169-170: In these two lines, “g” seems to be meant to be the unit “grams” rather than gravitational acceleration; it should thus not be italics.

Fixed.

L. 183: This empirical equation seems to assume that the various quantitates (i.e. \rho_s, \theta_w, etc.) are given in a specific set of units -- otherwise the unit of “k” does not work. Please specify what those units are, e.g. by writing “where ρ_s is density in kg\,m^3, k_w is …” (or whatever applies) at L. 184.

We use this equation from Webb et al. (2021), and realize that their equation is not explicitly clear regarding units. Webb et al. (2021) provide all details regarding units and derivation, and we try to clarify as follows.

In our text, we add the units for the snow/firn density ρ_s which are kg m^{-3}. Both relative permittivities (k and k_w) are unitless. The volumetric LWC (θ_w) is also unitless.

Webb et al. (2021) performed a regression analysis of k as a function of observations of ρ_s and estimates of θ_w to derive the equation (our Equation 4; their Equation 5). The units of coefficients 0.0014 and 2 x 10^{-7} have implicit units of m^3 kg^{-1} and m^6 kg^{-2}, respectively, derived from the regression analysis that resolves the equation to a dimensionless quantity.

We clarify in our text:

“We used Equation 4 to convert permittivity *(k*; unitless) to volumetric liquid water content (θ_w; unitless) (Webb et al., 2021):

$$k = [1.0 + 0.0014(\rho_s - 1000\theta_w) + 2 \times 10-7(\rho_s - 1000\theta_w)^2] + (0.01\theta_w + 0.4\theta_w^2)k_w \quad (4)$$

where ρ_s is density (in kg m^{-3}); kw is the relative permittivity of liquid water at 0 degree Celsius (unitless); coefficients of 0.0014 and 2 x 10^{-7} have implicit units of m^3 kg^{-1} and m^6 kg^{-2}, respectively; and the bracketed part of the equation accounts for the impact of dry permittivity."

L. 241: This is the first time the abbreviation "i.e." is used as a unit. Is it meant to say "ice equivalent"? If so, either (i) introduce the abbreviation properly (but note that throughout the remining text, "i.e." is used to abbreviate "id est"), or (ii) consider using the more common unit of "water equivalent (w.e.)".

We clarify this in the methods section ("2.4 The Community Firn Model"):

"Where applicable, we report model input and output in units of meters of ice equivalent (m i.e.; multiply by the ratio of the density of glacial ice to density of water, 0.917, to convert to m w.e.). This is to clarify the unit from the abbreviation "i.e." (id est) used elsewhere."

For Table 2, please (i) use the caption to define the units of the values provided by the table and (ii) possibly reconsider the number of significant digits shown by the individual values.

The Normalized Root Mean Square Deviation (NRMSD) is a dimensionless value. Therefore, the values in Table 2 do not have units.

We change the values to have two significant figures.

L.547-549: Please reconsider the placement of the references to "The GLAMBIE Team (2025)" and "Hugonnet et al. (2021)", as neither of the studies focuses on future contributions of glacier melt to sea level rise (they rather estimate past contributions).

Thanks, fixed, and added our reference Fox-Kemper et al. (2021) to support the claim regarding future contributions.

Fox-Kemper, B., Hewitt, H., Xiao, C., Aðalgeirsdóttir, G., Drijfhout, S., Edwards, T., Golledge, N., Hemer, M., Kopp, R., Krinner, G., et al.: Ocean, cryosphere and sea level change. Climate change 2021: the physical science basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change, Climate Change, 2021.

L. 545: the meaning of the symbol ">" is not entirely clear here. If it is meant to say "larger than the ..", please spell that out in words.

Fixed to "greater than."