

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.

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)
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.
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/m<sup>3</sup> in Figure 5)
4. In Figure 5, the 8-m--depth temperature seems to always be 0 °C. 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.
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
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.
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.

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...”

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

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

102: suggest using “pore space” instead of porosity

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”

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

Figure 1: add reference to the satellite imagery used

121-122: remove extra references to NWB1

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

170: possible typo --  $df=0.1$  for  $f=0.8$  or  $f \geq 0.8$ ?

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

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

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.”

253: 2004 was also common to all three sites

284: not sure which two cores are being referenced

304: unit missing in “up to 4.5-5.5 thick”

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

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

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”

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.

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

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

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

483: Specify whether the runoff is reported in  $\text{m}^3$  of ice or water equivalent

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

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