Review of Kindstedt et al.: ***Ongoing firn warming at Eclipse Icefield, Yukon, indicates potential widespread meltwater percolation and retention in firn pack across the St. Elias Range***

This paper utilizes firn core and radar observations to characterize the current state of the firn column of Eclipse Icefield, Canada, and the authors run a suite of simulations to assess how the thermal state and hydrologic regime of the firn column will change in the coming decade (2033). While the authors find that the firn column is dry in the early spring, they do note the presence of melt features in their detailed stratigraphy measurements, while also documenting a warming of the firn below 10 m depth by 2 °C between 2016 and 2023, which suggests that meltwater percolation occurs and has increased. Using the community firn model, the authors show that the firn > 15 m depth has a > 50% chance of becoming temperate by 2023 with at least 0.5 °C of warming. The authors argue that the development of temperate firn is a result of more positive degree days and melt event intensity rather than number or duration. Temperate firn or a firn aquifer will change the icefield's hydrologic regime and may make it unsuitable for future ice coring projects.

Overall, I think this paper is interesting and will make a nice contribution to the firn hydrology literature. My major concern is that I am not convinced by the finding that it is melt event intensity rather than melt season duration driving the firn warming. It appears in Table B2 that both of these are statistically significant drivers. Either the values in the table are incorrect or I am not sure that the authors can confidently come to this conclusion. I've made some comments about this below.

Even if the conclusions change, I think that they will be interesting and ultimately useful to the firn hydrology community regardless, and the findings will be important for understanding firn evolution in response to future warming in both alpine and ice sheet settings.

I have a number of comments and questions that I hope will lead to improvements to the paper. I believe that major revisions are required before its eventual publication, but I am happy to re-review it after changes are made. My comments are categorized into two sections: general comments likely requiring some revisions throughout the manuscript, and specific comments focused on particular lines of text.

General Comments

(1) In the paper you state *"Model results for Eclipse show the development of year-round temperate firn at 15 m depth associated with an increase in total PDDs throughout the melt season and more extreme individual melt events, rather than a greater number of*

melt events or prolonged melt season" (Lines 453 – 455). This statement seems to support findings of others such as Nghiem et al. (2012) and Horlings et al. (2022) that show summers of extreme melting in Greenland are mostly dominated by intense but short-lived events. I was wondering if you could dive into this some more? You show the number of PDDs for temperate vs polythermal firn across all model runs. Are there other ways to look at this that might be as/more useful to justify this conclusion? What about calculating the average PDD (or meltwater input) per melt event for temperate vs. polythermal firn? In my mind that should show the intensification of melt events that produce temperate firn.

However, I'm confused with this conclusion on lines 453 – 455 because in Table B2 you report p values less than 0.05 indicating significant differences in the melt season start, end, and length between model runs that produce temperate firn and those that do not with 0.2 and 0.5 °C of warming, so how do you rule out that prolonged melt seasons are not driving the creation of a temperate firn layer?

Additionally, in combination with intense melt events causing firn aquifer expansion, Horlings et al. (2022) found that winter temperatures substantially increased and the firn's cold content significantly decreased. Do you see similar trends? For example, do you find that when the firn becomes temperate there is less of a regeneration of cold content in the winter? Or is it primarily a summer-driven process?

I think fleshing out some of the drivers of firn warming/possible aquifer formation would be really interesting and useful for the firn community.

(2) This paper has a lot of components to it, all of which are interesting on their own, but can sometimes make the paper feel a bit disjointed. To me, it seems as though you are trying to make two main points with the paper: (1) that the deep firn column is warming from increased latent heat inputs, and there is a high likelihood that Eclipse will become temperate by 2033; and (2) although there are melt inputs into the firn, the firn column here is relatively "dry" and can still (for now) be used as an ice coring site. This may be my personal research bias, but it seems like point number 1 is the "highlight" takeaway, and it is the point which it appears you spend the most time analyzing with the modeling study. For me, I find there is a lot to keep track of and I would urge the authors to consider ways to make the paper more concise and punchier. Could you structure the discussion to just more concisely say (1) say the firn column is still relatively dry (from GPR and cores), so it could still be a valuable ice core site. (2) but there is evidence of increasing meltwater inputs (firn density, temperature increases), (3) firn modeling shows that there is a likelihood of the firn becoming temperate. I also think you could move the hypsometry to the Appendix and just reference it when mentioning that 90% of the

Eclipse icefield might be at risk for firn aquifer development? I am just trying to make sure the key points aren't lost, but feel free to disagree with these suggestions.

Specific Comments

Line 26: I think Harper et al. (2012) may be a more appropriate reference here.

Line 29: *"as irreducible saturation or a firn aquifer"* also as slush fields (e.g., Clerx et al. (2022)) although this isn't really multi-annual storage of meltwater.

Line 61: I feel like a bit more description is needed here or needed in this section. Could you define fine grained, coarse grained, textured ice? Perhaps moving sentences on Lines 212 – 215 might be appropriate.

Line 80: A few things about this line: the parenthetical was a bit confusing to me. Could you just say (i.e., either if the density was greater than 917 kg m^{-3} or was 300 kg m^{-3} less than the density of the summer surface). Also, can you add a space between your units here and elsewhere (change kgm^{-3} to kg m^{-3}). Lastly, why do you plot densities > 917 kg m^{-3} in Figure 4 if you have removed them as outliers?

Line 81-82: Is it common to report depths of the top of the segment? Why not report it for the midpoint depth?

Line 103-104: The temperature profiles look reasonable, so not a major concern, but even after you let the borehole equilibrate what about any advective heat flow through the top part of the borehole from advective heat transport from wind, etc. while collecting the measurements?

Line 137: What are typical precipitation patterns like here? Is it reasonable to distribute snow accumulation evenly throughout the year?

Line 150-151: Does the selected pairing of density and DDF also capture the ~1.7 degrees of warming that occurs between 2016 and 2023?

Line 158: Where does 0.024 °C yr^{-1} come from?

Figure 2: Nice figure. Could you indicate the size of ice lenses by the fractional width that they occupy in the core (similar to Figure 2 in McDowell et. al (2023)? Not a high-priority change.

Line 221: Maybe I missed this, but when you say "visibly metamorphized and/or melt altered" are you including ice layers, coarse-grained, and melt-affected sections that you identify in Figure 2?

Lines 289 – 293: This paragraph is confusing. You state that you *"find a significant (p < 0.05) difference"* on Line 289. But then on Line 291 you state *"we do not find any significant (p < 0.05) difference…"*. If it is not significant, the p value should be > 0.05 using your confidence level. Also, please check Table B2. It appears that your p values in the table suggest that there are significant differences between median melt season start, end, and length (they are < 0.05)? If that is the case, then these sentences are incorrect, and doesn't that also change your interpretations/conclusions? How do we know if it is melt intensity vs melt duration that is producing temperate firn?

Line 317: Should Fig. 11 be referencing Fig. 1 instead?

Figure 11: I would consider moving this to the Appendix to make the general text shorter.

Line 373: Also probably more appropriate to cite MacFerrin et al. (2019) here.

Lines 384-385: Was the 2017 radar profile by McConnell (2019) collected at the same time of year as your radar data? It could have more liquid water just because it was later in the summer?

Lines 453 – 455: Again, please be sure this is correct. It appears that the melt season lengths are significantly different as well.

Table B2: See previous comments about the p values. Also, why are the scenarios of warming an order of magnitude lower than what you state in the text on Lines 164 – 165?

References cited:

Clerx, N., Machguth, H., Tedstone, A., Jullien, N., Wever, N., Weingartner, R., & Roessler, O. (2022). In situ measurements of meltwater flow through snow and firn in the accumulation zone of the SW Greenland Ice Sheet. *The Cryosphere*, *16*(10), 4379-4401.

Harper, J., Humphrey, N., Pfeffer, W. T., Brown, J., & Fettweis, X. (2012). Greenland ice-sheet contribution to sea-level rise buffered by meltwater storage in firn. *Nature*, *491*(7423), 240-243.

Horlings, A. N., Christianson, K., & Miège, C. (2022). Expansion of firn aquifers in southeast Greenland. *Journal of Geophysical Research: Earth Surface*, *127*(10), e2022JF006753.

MacFerrin, M., Machguth, H., As, D. V., Charalampidis, C., Stevens, C. M., Heilig, A., ... & Abdalati, W. (2019). Rapid expansion of Greenland's low-permeability ice slabs. *Nature*, *573*(7774), 403-407.

McDowell, I. E., Keegan, K. M., Wever, N., Osterberg, E. C., Hawley, R. L., & Marshall, H. P. (2023). Firn Core Evidence of Two‐Way Feedback Mechanisms Between Meltwater Infiltration and Firn Microstructure From the Western Percolation Zone of the Greenland Ice Sheet. *Journal of Geophysical Research: Earth Surface*, *128*(2), e2022JF006752.

Nghiem, S. V., Hall, D. K., Mote, T. L., Tedesco, M., Albert, M. R., Keegan, K., ... & Neumann, G. (2012). The extreme melt across the Greenland ice sheet in 2012. *Geophysical Research Letters*, *39*(20).