

# Review of egosphere-2026-1842: "Summer subsurface temperature variability in the percolation zone of southwest Greenland: high resolution observations of the top meter of firn"

This study presents temperature string observations of the upper meter of firn over summer 2024 at DYE-2, located in Greenland's percolation zone. The authors extensively analyze this novel dataset and identify three characteristic thermodynamic layers indicating distinct regimes of dominant energy transport in the firn and corresponding temporal differences in the snow-atmosphere coupling.

The observational approach is very compelling. In combination, the above-surface instrument setup and high temporal and vertical resolution provide a detailed and continuous picture of energy transport in the firn, which other studies, using coarsely spaced instruments that are only installed in the subsurface and get quickly buried after deployment, do not capture.

The manuscript is generally well-written and clear, but a bit long. To improve the reading flow and make better use of the space throughout the manuscript, I recommend to avoid repetitions of lengthy formulations in consecutive sentences that are clear from the context (e.g., L275: "we believe this meltwater was caused by the instrument support structure" or L301: "surface is always at 0 cm").

In summary, I really like this work and think it is a great fit for The Cryosphere after my overall minor comments have been addressed.

## **General remarks:**

Implications for surface melt: The introduction outlines why evaluating the energetic processes in near-surface firn is important for investigating surface melt. However, it never addresses these implications directly and only refers to another submitted study, which couples the presented data with atmospheric measurement to investigate surface melt. Although the presented data are sufficiently interesting on their own, at least a more explicit outline of how the data can support surface melt studies would be helpful. The authors could for example address questions like: Which key variables were missing in past surface energy balance studies, that this new data can provide? How can this data help to validate SEB models? And how does the identification of the thermodynamic layers help to improve SEB models?

Discussion of the method: As the observational method is novel, a more detailed discussion of its limitations and potential for further improvement would be helpful. I have for example these questions: Is it possible to deploy the instrument year-long, or which challenges/limitations have prevented this? What insights could be gained during winter from these high-resolution observations? How would the observations be affected when melt events were stronger and meltwater would remain in liquid state for an extended period? And in this case, what additional insights could be gained from tracking the refreezing period?

Specificity of summer 2024: In this regard, I think it is also worth to highlight in the discussion that 2024 was an exceptionally low melt year. The authors acknowledge this in the field location section, but this fact deserves more prominent attention so that it is clear that the observations are not generally representative for DYE-2.

The mathematical notation is inconsistent throughout the manuscript and between figures and text and does not follow best practices. Variables should be italic, labels that do not describe variables should be upright and spaces in single variables are confusing and should be replaced by subscripts: e.g.,  $SW_N$ , instead of SW N. Also temperatures should be written as 0°C, instead of 0 °C.

#### **In-line remarks:**

L2: "spatiotemporal" implies lateral variability. As the sentences continues with "in the upper meter of firn", I suggest to just write "temporal variability".

L4: "temperature string": Is there a reason to not use the more common term "thermistor string"?

L21-2: The common definition of the percolation zone involves areas where runoff occurs. It is mostly characterized as the lower part of the accumulation zone, where on-average significant surface melting occurs during summer, and the meltwater can percolate into the firn where it is either retained or runs off. I suggest to rephrase this in terms of an inland expansion of the percolation zone or an upward shift of the runoff limit that lies within in the percolation zone (Tedstone and Machguth, 2022).

L32-5: I don't think most studies actively exclude the uppermost layer, but they simply cannot resolve or track it, after sensors get buried.

L64: "abandoned" only refers to the military use. However, from a science perspective, DYE-2 is an active GC-NET automatic weather station site (Fausto et al., 2026).

L65-7: This might be misleading as DYE-2 should be located upstream of the area with strong surface lowering. Yet, it is great that the authors acknowledge that 2024 was an exceptional year in Greenland (See comment above).

L129-132: This paragraph mostly repeats parts of the previous one. Given the length of the manuscript, I suggest merging them.

L166-179: Please review this paragraph which is currently hard to follow, for example formulations like "we want the maximum temperature correction" and "we do not use the maximum temperature" are confusing.

L174: Is the "shape" not set to be an inverse hyperbolic sine?

L197-8: Comparing a "curve" to a "value" does not make sense.

L198-202: It's not clear what this means or what it refers to, especially in a paragraph on the improvements of the shading correction.

EQ2: The 2 in the denominator might be erroneous, as  $t_{n+1} - t_{n-1}$  already covers two time steps.

L223-4: This is a net conduction into\out of the layer, but not a "divergence" in its mathematical definition. Correspondingly, I suggest to label it  $\Delta FC$ .

L225-8: Fix variable name "SW A".

L251-9: The clarity of this part could be improved. (1) Why are the authors referring to the 10-20 cm layer and not the direct surface layer as in the following sentence? Also the formulation "order of magnitude larger" is confusing, larger relative to what? In case it refers to the 40-60 cm layer, which is "near zero", it should not only be an "order of magnitude" difference. (2) Providing the temperature gradient values after writing "SWA values" is not ideal. (3) The formulations about the warming of the deeper layers are contradictory.

L279-80: These sentences pretty much repeat each other.

L290: Please briefly introduce the "solid state greenhouse effect".

L311-2: Following Figs. 2 and 7f, should this not also include SWA?

L317: "in time" and space?

L340-2: The formulation of the first sentence is overcomplicated, resulting in a not correct implication, because there are peak solar insolation phases during which downward conductive fluxes are dominant.

L347: Do you mean Fig. 9?

L351: This observation is interesting. But I think it is worth clarifying that this comes down to the fact that the temperature gradient is not indicative of energy transport in general, but only one form of it (heat conduction), whereas during melt events another form (meltwater infiltration and latent heat release) becomes dominant.

L375: Mentioning the initial temperature seems redundant when repeating it directly afterwards.

L395: Why is the reference height not 0 cm? The +2 cm shift unnecessarily complicates the text and figures and reduces clarity.

L403: As long as the units are consistent in your calculations, there is no need to mention the unit used during a multiplication in the text.

L411: These 20 cm layers should not be called "slab" as "ice slab" has a very specific meaning in the percolation zone context.

L426: At this point it is unclear what is meant with "horizontal advection". I am also not fully convinced, that Fig. 8e indicates horizontal meltwater advection. Could the meltwater not just infiltrate the firn very rapidly without significantly heating up the thermistor string along the way?

L460: Can you clarify the direction of the relation, i.e., a negative bias in cumulative energy indicates that densities were under-/overestimated?

L571: Typo in Fig. C21. There also seems to be an opposing temperature bias above and below the snow surface (Fig. C2e). Which might reduce the mean difference. To get a clearer picture of the bias, I think it would be better to take the mean absolute difference or the mean difference below the snow surface.

Fig. 10: The y-labels should say "cumulative" instead of "accumulated".

Fig. D3: It is almost impossible to distinguish the two observations methods, when the lines have the same color. I suggest to use different tones of blue and grey/black.

Fig. D4: In the caption,  $\Delta FC$  is inconsistent with the definition elsewhere in the text (Although I prefer this labeling, see comment on L223-4). The "2 cm" in the caption seems to be an artifact.

Table A1: Typo in "SAMS Enterprise SIMBA". As it is a bit hard to keep track of the variable labels while reading the manuscript, I suggest including them here in the table, so it could also serve as a glossary.

## References:

Fausto, R. S., How, P., Vandecrux, B., Lund, M. C., Box, J. E., Mankoff, K. D., Andersen, S. B., van As, D., Bahbah, R., Citterio, M., Colgan, W., Jakobsgaard, H. T., Karlsson, N. B., Kjeldsen, K. K., Larsen, S. H., Olsen, C., Oraschewski, F. M., Rutishauser, A., Shields, C. L., Solgaard, A. M., Stevens, I. T., Svendsen, S. H., Langley, K., Messerli, A., Bjørk, A. A., Andersen, J. K., Abermann, J., Steiner, J., Prinz, R., Hynek, B., Lea, J. M., Brough, S., and Ahlstrøm, A. P.:

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Tedstone, A.J., Machguth, H. Increasing surface runoff from Greenland's firn areas. *Nat. Clim. Chang.* 12, 672–676 (2022). <https://doi.org/10.1038/s41558-022-01371-z>