Referee 1:

Thank you for the constructive review. Your comments provided valuable insights to refine the content and analysis of this work. Here, we respond to the issues raised including additional explanations of the impact of melt and refreeze; specificity of the observational period; and edits throughout the text and figures for improved clarity. Edits to the text include improving explanation of the solar correction, discussion of diurnal cycles, and removing unnecessary or redundant language. Justification as to why some reviewer suggestions were not implemented, e.g., a longer discussion of extending the observations, are provided in response to the comments below. Our responses to all referee comments are given in blue.

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?

We believe observations of the near-surface firn are what has been missing from past energy balance studies that often rely on models to quantify subsurface fluxes, discussed in the original manuscript on lines 31-45. The thermodynamic layers could serve as a benchmark for models to evaluate how models represent energy transfer in the near surface firn since they depend on physical properties that are not continuously measured (e.g., density/thermal conductivity). However, given the limited nature of this data (2 months from one spot during a low melt year), depths of layers observed here should not be taken as broadly representative. These thoughts will be included in the discussion:

> Another limitation of this study is the fact that the observations only cover two months of data during a year with limited melt. In this dataset melt is episodic. We would expect more or continuous melt to reduce near surface variability in the rapid-response and diurnally-responsive layers as temperature would be limited to 0 ^{o}C. Energy transport into the seasonally-responsive layer could increase either through stronger temperature gradients between the isothermal firn above or through percolation of meltwater.
>
> …Models could also benefit from using the thermodynamic layers to evaluate energy transfer in the near surface firn, ideally with additional observations to ensure statistical robustness.

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?
These are great questions that we also would like to know the answers to, ideally with more observations. The temperature string has been deployed year-round in sea ice and at Summit Station, but not all the additional measurements for processing and correcting the data are available. Quantifying the energy associated with refreezing and how it is coupled to the atmosphere would be interesting, as would having these same measurements at different elevations on the ice sheet to explore the impact of different melt regimes on the thermodynamic layers and energy absorbed/released throughout the year. While we are interested in these questions, they are not included in the paper due to concerns about length. We will include additional discussion about the potential impact on additional melt written in the above comment.

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.
We will acknowledge the limited record and unrepresentative melt amount by including the following paragraph in the discussion with the same paragraph from above:

> Another limitation of this study is the fact that the observations only cover two months of data during a year with limited melt. In this dataset melt is episodic. We would expect more or continuous melt to reduce near surface variability in the rapid-response and diurnally-responsive layers as temperature would be limited to 0 ^{o}C. Energy transport into the seasonally-responsive layer could increase either through stronger temperature gradients between the isothermal firn above or through percolation of meltwater.

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℃, instead of 0 ^{o}C.
We will update the notation between the labels in Figs. 10, B1, and D4. Spaces in single variables are due to typesetting in LaTex as there are no spaces in ‘SWN’ or ‘SWA’. These details will also be checked by TC copy-editors for their criteria before publication.

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".
Thank you for the suggestion, we will include it.

L4: "temperature string": Is there a reason to not use the more common term "thermistor string"?
The strings now use digital temperature sensors instead of thermistors, which differ in their measurement quality and accuracy.

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).
We will rephrase these sentences as follows:

> Within the accumulation zone of the GrIS more area is projected to transition from the dry snow zone, where there is little to no melt, to the percolation zone, where melt occurs and may runoff or be retained in the firn (McGrath et al., 2013). Regions of the current percolation zone are also experiencing more melt, and here elevations are contributing to runoff (Van Angelen et al., 2014; 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.
We will rephrase this to “often does not resolve”.

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).
We will remove “abandoned” from the sentence.

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).
We will rephrase this sentence as follows:

> Although **southwest Greenland** has experienced elevation changes on the order of -0.50 m yr^{-1} over 2007-2017 (Mottram et al., 2019), the GrIS experienced below average temperature and above average snowfall over the summer of 2024 (Poinar et al., 2025).

L129-132: This paragraph mostly repeats parts of the previous one. Given the length of the manuscript, I suggest merging them.
We will merge this paragraph with the previous one as follows:

> We further build on this algorithm when correcting for solar heating of the temperature string. Given that the amount of solar radiation penetrating the firn to the temperature string depends on where the surface is, we use the solar correction method (discussed next) to further refine the surface location. The resulting +4 cm offset is applied to all timesteps and improves agreement with the manual measurement of the surface height from the decommissioning in August. Using a 24-hour interval for calculating the variance means that any rapid changes to the surface height may not be well

represented. However, a longer interval is needed because the diurnal cycle of $\Delta T/\Delta z$ is what drives the maximum variance.

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.

We will rewrite this paragraph as follows:

> Within each wind speed bin, a curve is fit between temperature and SWT. SWT is divided into 20 bins for every 5th percentile. In each SWT bin, the median of the largest 15% of data (black circles in Fig. 5a-c) is used to fit a correction curve (red triangles in Fig. 5a-c). We use this upper portion of the data because the temperature recorded by the temperature string above 0 ^oC is the minimum possible bias, but it could be larger if the firn is not melting. However, we use the median of the upper 15% of data instead of the maximum value because we want to avoid overcorrection. The correction curves are inverse hyperbolic sine functions:
>
> $$\textit{temperature correction} = \textit{arcsinh}^{-1}(\alpha * SWT), \quad (2)$$
>
> where α determines the steepness of the IHS curve. α is parameterized as a function of wind speed using α and the medium wind speeds from each bin (Fig. 5d). Thus, the wind speed determines the steepness of the IHS correction curve for a given timestep, and the temperature correction is then calculated using the IHS curve and SWT at each level below the surface. Wind speed was chosen as a parameter because of the expectation that given a temperature difference between the temperature string in the snow and the snow itself, higher wind speeds would more efficiently reduce the temperature difference through sensible heat transfer. However, this effect appears relatively limited with the maximum difference between high and low wind speed corrections on the order of 0.5 ^oC for large SWT (Fig. 5d).

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

The shape is the IHS, but we meant the particular steepness of each IHS curve. We will rephrase this as follows:

> "Thus, the wind speed determines the **slope** of the correction curve for a given timestep."

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

We will edit this sentence as follows:

> "The **magnitude** of the solar heating bias with depth is similar to the upper values of the shading correction."

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.

This paragraph is about the improvement to the solar heating correction, which depends on our assumptions of transmitted shortwave radiation (SWT) below the surface. While we don't have any direct observations of SWT, the shading of the temperature string, discussed in section

2.2.1, gives information about the depth and magnitude of solar radiation absorbed by the temperature string, shown in Fig. 2. To clarify this, we will update these lines as follows:

> **While there are no observations of SWT, the shading adjustment (Section 2.21) provides information on the depth and magnitude of solar absorption by the temperature string. The magnitude of the solar heating correction is similar to the shading adjustment below the surface (Fig. 2a).** The heating correction, when limited to the same time steps as the shading adjustment, only follows the largest shading correction for a given depth.

EQ2: The 2 in the denominator might be erroneous, as t_n+1 - t_n-1 already covers two time steps.
Thank you for catching this typo. The 2 will be removed in Eq. 2.

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 ΔFC.
We will change our reference to “difference in conduction across the layer” and “net conduction” and replace with Δ throughout the manuscript.

L225-8: Fix variable name "SW A".
The space is from the LaTex mathematical formatting, and any other such errors will be copy-edited before publication.

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.
To support clarity, this paragraph will be revised as follows:

> Overall, $T_{sfc,string}$ agrees with the magnitude and timing of the diurnal cycle of $T_{skin,rad}$, with a +0.6 ^{o}C average difference and a root-mean-square error of 1.5 ^{o}C. The near-surface warming is coincident with the largest values of SWA, with roughly two thirds of SWN absorbed within 2 cm of the surface (Fig. 7f). The strongest temperature gradients (up to 1 ^{o}C cm^{-1}) also coincide with the largest SWA values, suggesting that increased temperature from SWA induces a temperature gradient and thus increases the conductive flux near the surface. Although the uppermost firn both warms and cools, the magnitude of warming is approximately twice as large as the cooling and extends further down, leading to the firn 20-50 cm below the surface warming by 3-5 ^{o}C by the end of May.

L279-80: These sentences pretty much repeat each other.
To remove repetition, these sentences will be condensed as follows:

> At levels where liquid water was present, the surrounding firn warms as latent heat is released from the liquid water freezing. The increased temperature from liquid water /

> refreezing processes within cold firn at depth produces greater vertical temperature gradients away from the liquid water.

L290: Please briefly introduce the "solid state greenhouse effect".
We will reword this sentence to only mention absorbed shortwave instead of the solid state greenhouse effect.

L311-2: Following Figs. 2 and 7f, should this not also include SWA?
We will include SWA in the highlight of the diurnally-responsive layer as outlined below:

> Diurnally-responsive layer: the upper tens of centimeters that show changes in temperature and gradients over the diurnal cycle; **the most relevant processes are solar absorption and** conduction; this behavior has been noted in Greenland by Pfeffer and Humphrey (1996) and Kuipers Munneke et al. (2009).

L317: "in time" and space?
Yes, we will add “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.
We agree that this sentence, and the paragraph it’s a part of, could be improved. Based on this comment and some from the second referee, the lines will be rewritten as follows:

> The depth of the diurnally-responsive layer represents how far down the energy into the surface can be conducted until the diurnal temperature forcing near the surface shifts, which depends on how efficiently energy is conducted and the magnitude of the energy input. At the surface, averaged over the full average diurnal cycle, the vertical temperature gradient is always negative (green in Fig. 9c and black line in Fig. 9d), which implies that on average there was more energy being conducted upwards to the surface.

L347: Do you mean Fig. 9?
Yes, we will correct this to Figure 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.
We will update the sentences around this line as follows:

> Below this depth $\Delta T/\Delta z$ can increase to move the energy further down into the firn, and any percolation of meltwater will also warm the firn and release latent heat. If the surface refreezes while there is still liquid water below, energy can also be conducted upwards as the meltwater freezes at depth.

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

afterwards.
This sentence will be removed to shorten the manuscript.

L395: Why is the reference height not 0 cm? The +2 cm shift unnecessarily complicates the text and figures and reduces clarity.
Originally we were referencing the height when the instrument was installed as 0 cm, but we will instead use 0 cm for the beginning of the observations.

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.
We will remove “in seconds” from L403.

L411: These 20 cm layers should not be called "slab" as "ice slab" has a very specific meaning in the percolation zone context.
We will replace “slab” with “layer” in L411 and throughout the manuscript.

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?
The cyan profiles in Fig. 8e, reproduced below in Fig. S1, show that some firn was near/at melting close to the surface before and during when the local temperature maximum around -75 cm formed. The maximum increases for over an hour (profiles are every 15 minutes), which seems inconsistent with the idea of a quick downward pulse of meltwater that didn't impact the firn (or sensors) in between. We will adjust Fig. 8e to better highlight profiles when the local temperature maximum forms.

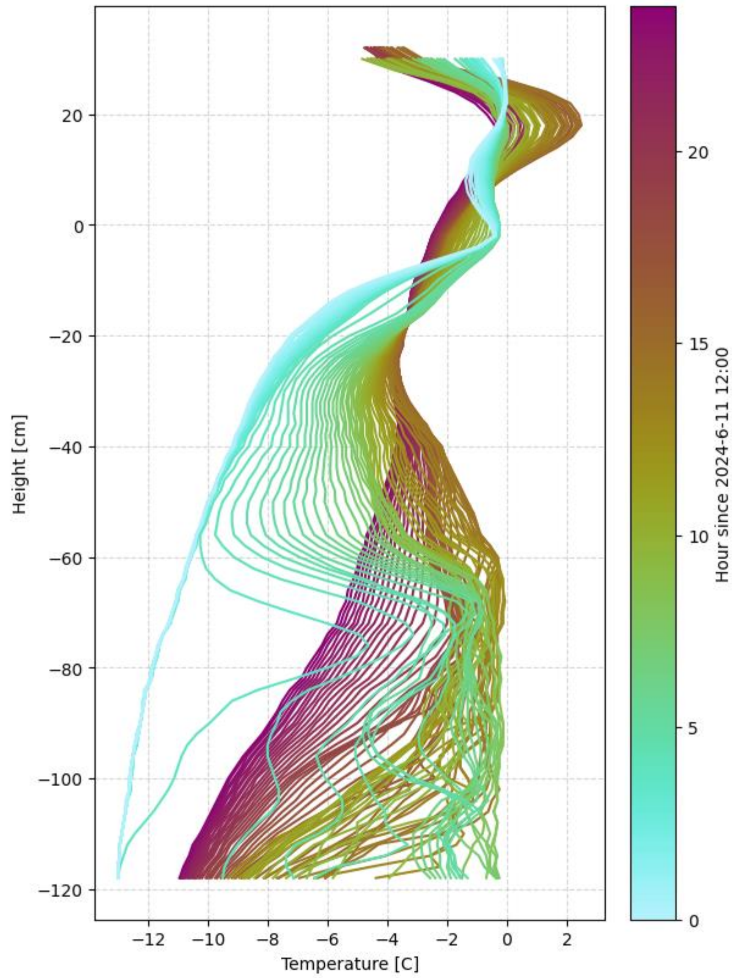

Figure S1: Temperature profiles from June 11, 2024 from Fig. 8.

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

We will update the following sentence to clarify this:

> The opposite could be true for the (2, -18) layer, which shows an increasing amount of cumulative energy that is not from conduction or SWA, so the **density**, derived thermal conductivity, and conductive flux could be underestimated.

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.

We have corrected the typo. Figure C2 shows the concatenation, which is done before identifying the surface. We will add the mean absolute difference in the sentence:

> Over all heights and times, the mean difference in temperature is 0.39 ^{o}C with a standard deviation of 1.4 ^{o}C **and a mean absolute difference of 1 ^{o}C.**

Fig. 10: The y-labels should say "cumulative" instead of "accumulated".
We will update these figure labels.

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.
We will update the colors in Fig. D3a to distinguish between the different lines and update the figure caption to match, shown below.

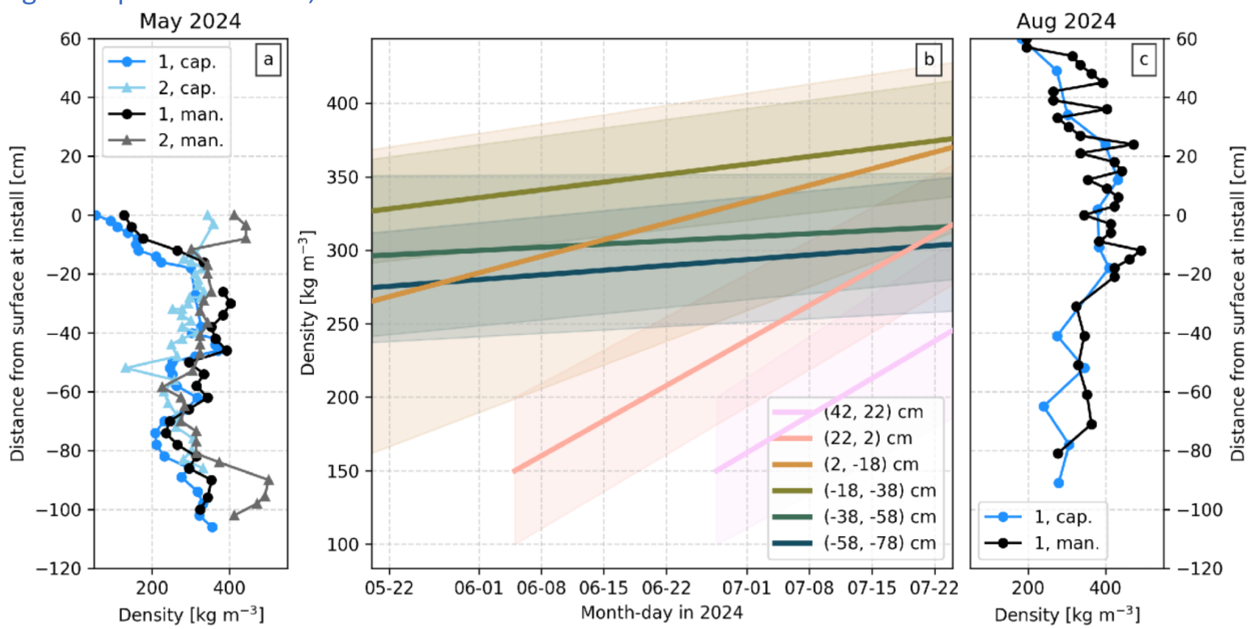

Figure S2: Updates to color scheme of density profiles shown in (a).

Fig. D4: In the caption, ΔFC 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.
We will update the notation to ΔFC throughout the manuscript, including Fig. D4.

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.
Thank you for catching this typo and the suggestion. We will add symbol labels for variables included in the text in Table A1.