Referee 2:
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; the impact of density assumptions on flux calculations; and edits throughout the text and figures for improved clarity. Edits to the text include improving the discussion of diurnal cycles and removing unnecessary or redundant language. Justification as to why some reviewer suggestions were not implemented, e.g., additional consideration of the full surface energy balance, are provided in response to the comments below. Our responses to all referee comments are given in blue.

The manuscript "Summer subsurface temperature variability in the percolation zone of southwest Greenland: high resolution observations of the top meter of firn" by Sledd et al. describes the installation of a thermistor string in the percolation zone of Southwest Greenland. Advanced data processing is described, to correct for the impact of solar radiation on the thermistor string, in order to be able to reliably analyze the near-surface firn (uppermost 1m). The processing of the data is described in good detail, and seems robust, even though I have little experience with thermistor string measurements to fully judge this. The manuscript presents interesting and novel insights and analysis in the thermal behavior of the near-surface firn layer. Having said that, the manuscript is somewhat long, with some repetitive, or needlessly extensive explanations, and if the authors would simplify and streamline the manuscript a bit more, this would help with accessibility for readers. At a scientific level, I also think that the manuscript would mostly benefit from some improvements in the Results section, as I'll point out below.

Particularly when reading the Results section, it sometimes felt that the only emphasis was on internal conductive fluxes and shortwave absorption, without considering the impact of the full surface energy balance, that includes the turbulent fluxes, and longwave absorption.

Two examples:

L340: "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 an upward conductive flux at the surface even during peak solar insolation" I don't follow the logic. If the average is negative, this does not necessarily exclude the possibility that during part of the averaging period, the conductive flux was 0, or even downward? Are there not any phases, for example during cloudy weather (thus when SWA is reduced), and a positive surface energy balance gives a downward conductive flux, even through a brief period of time?

L344: "This average negative vertical gradient at the surface is perhaps unexpected given that there should be net energy into the surface, i.e., a positive gradient, to warm it and eventually cause melt during summer" This sentence seemingly ignores the full surface energy balance. The surface melt is not governed solely by the heat flux from below.

Also, I think that the authors could try to better discuss the impact of water flow on the thermal structure of the firn. For example, L350: "Below this depth ΔT/Δz can increase to move the energy further down into the firn." Here, it should not be overlooked that when part of the snow reaches melting point and there is sufficient meltwater production, the downward percolation of meltwater is a much more efficient process to heat deeper firn, than the conductive fluxes governed by the temperature gradient. And in L351: "When the surface is no longer melting, the surface will cool first and cause upward conduction" When the surface is no longer melting, first the meltwater in this layer must freeze, before temperature gradients will become non-zero. And in fact, the statement is not generally true, as there are also phases, particularly early in the melt season when the firn is still cold, where refreezing of the surface meltwater is also driven by heat conduction downward, heating up deeper firn. So it feels like in this discussion, some nuanced description regarding the role of the full energy balance, and the role of water flow, is missing.

For this manuscript we have intentionally chosen to focus on the subsurface fluxes and exclude consideration of the full surface energy budget (SEB) and atmospheric fluxes, which is the focus of ongoing work. We will however rewrite portions of the manuscript to address the reviewer's comments, such as those noted below.

In regards to the first example, there are times when there is a positive vertical temperature gradient and downward conductive flux at the surface, which we mention on the original lines 345-6 and is discussed with Fig. 7. Our intention was to convey that the *average* conductive flux is upwards, which does include instances of downward conduction. Melt is the result of the full surface energy budget. The point we were trying to convey was that averaged over the season we expected a net warming of the firn and energy to be conducted away from the surface deeper into the firn. The temperature gradients at the surface are complicated by zero vertical gradients at the surface during melt and refreezing, as you have pointed out. This was not well communicated, so we will rewrite the paragraph as follows:

> 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. This average negative vertical gradient at the surface is perhaps unexpected given an assumption of** net energy into the surface, i.e., a positive gradient, to warm it and eventually cause melt during summer. Part of this result is due to the specific set of observed events, such as snowfall occurring after surface melt that insulated relatively warm temperatures, which are averaged together with occurrences of positive vertical temperature gradients (e.g., May dates in Fig. 7d). The average values reported in Fig. 9 also include melt at the surface, which has a distinct impact on vertical temperature gradients: because melting snow has an upper bound of 0 ^{o}C, the vertical temperature gradient goes to 0 ^{o}C cm^{-1} over the depth where snow is melting and the magnitude and frequency of downward ΔT/Δz is effectively truncated. Below this depth ΔT/Δz 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.

L351 did miss the nuance of meltwater needing to refreeze. It will also be reworded in the revised paragraph above. We don't see a strong signal from meltwater below the surface in Fig. 9 since it shows averages over time when melt was intermittent and relatively limited. However, we will include the following text to discuss this:

> Below the diurnally-responsive layer, the average ΔT/Δz is weakly positive, meaning energy is conducted down into the firn over summer. **This average also includes melt and refreeze events that cause relatively large vertical temperature gradients both upwards and downwards from a given level. However in these observations such events are not frequent enough to have a significant impact on the average ΔT/Δz at lower depths.**

L273: The authors write: "This horizontal advection, as opposed to channeling down the temperature string itself, is consistent with ice layers seen when removing the instrument at the end of summer (Fig. D2). Because we believe this meltwater was caused by the instrument support structure, we do not think it is representative of the magnitude of melt in the surrounding area."

L367: The authors write: "... when there was instrument-induced meltwater and horizontal advection and channeling to lower depths. ... Although this event had real impacts on the subsurface temperature, its magnitude and timing likely do not represent typical conditions or relationships between the subsurface and atmosphere."

While it's good to be critical on the impact of the thermistor string on melt and meltwater percolation processes, preferential flow is known to occur, and it typically has such effects on the temperature structure of the firn. See for example Fig. 8 in Humphrey, N. F., J. T. Harper, and W. T. Pfeffer (2012), Thermal tracking of meltwater retention in Greenland's accumulation area, J. Geophys. Res., 117, F01010, doi:10.1029/2011JF002083, or Fig. 6c in Wever, N., Würzer, S., Fierz, C., and Lehning, M.: Simulating ice layer formation under the presence of preferential flow in layered snowpacks, The Cryosphere, 10, 2731–2744, https://doi.org/10.5194/tc-10-2731-2016, 2016. Ice layer formation is common in Greenland, as discussed in The Firn Symposium team. Firn on ice sheets. Nat Rev Earth Environ 5, 79–99 (2024). https://doi.org/10.1038/s43017-023-00507-9 and references therein. In my opinion, the impact of the various water transport mechanisms needs to be discussed in more detail in relation to the thermistor string measurements. While undoubtedly there was some influence from the thermistor string, I would not be surprised if the observed process of the ice layer formation did in fact occur. Or was the ice layer really only constrained to the measurement site?

It is difficult to explain with 100% confidence what exactly happened with the limited observations available during the June 11-12 melt event. Based on the local temperature maximum around -70cm we are fairly certain meltwater moved latterly to the temperature string. When we removed the temperature string and its support structure we found more ice around them than the surrounding area. This additional ice included some frozen onto the support structure, some frozen onto the bottom of the temperature string, and some ice layers observed between the support structure and the temperature string that did not continue beyond the temperature string, documented in the supplemental video. The additional ice at the bottom of the temperature string corresponds with the height of the increased temperatures around -60—80cm on June 11-12. The CAMEO installation several meters away also had significantly more ice around it than the surrounding area at the end of the season, supporting the idea that the instruments/installation increased the amount of meltwater locally. Our interpretation is somewhat speculative, though, given that preferential flow is a documented process and we have limited observations of the subsurface beyond the temperature string at the time. We are not suggesting all melt observed at the temperature string was caused by the support structure, though, and we agree that meltwater presence/movement is important for subsurface temperature variability, which is why we still discuss the impact of the meltwater and its refreezing on the subsurface temperatures. We will reword this section as follows:

In addition to melt at and immediately below the surface, there is evidence of lateral movement of meltwater that creates a local temperature maximum, shown by the 0 oC temperature below -60 cm (Fig. 7c). While horizontal flow has been documented before (e.g., Humphrey et al., 2012), we suspect the support structure of the temperature string caused local downward channeling of meltwater that moved horizontally to the temperature string based on ice layers seen when removing the instrument at the end of summer (Fig. S2). Regardless of the exact origins of this meltwater, it alters the subsurface through the release of latent heat as it refreezes, such that the temperatures at those depths are never as cold as prior to this event and the entire observed firn column warms (Fig. 7c).

Finally, for Section 3.3, the authors assume a linear trend in density to model the subsurface conductive fluxes, in order to interpolate from the initial and final density profile taken in the field. Given that the changes in density are likely not linear, because of viscosity changes due to temperature, enhanced compaction upon first wetting (as for example: https://scispace.com/pdf/the-first-wetting-of-snow-micro-structural-hardness-2islaa1vn8.pdf), and refreezing of percolating meltwater, what impact could this have on the calculated time series shown in Fig. 10?

We completely agree that in reality density changes are likely nonlinear. For a comparison, we have recreated Fig. 10 assuming any increases in density for a given layer occur in steps after melt events. The density step change is the total density change from when the instruments were installed to when they were decommissioned divided by the number of melt events. Melt events are defined as when any temperature observation within the layer is greater than -0.5 ^{o}C. The new density time series is used to calculate storage and thermal conductivity for conduction.

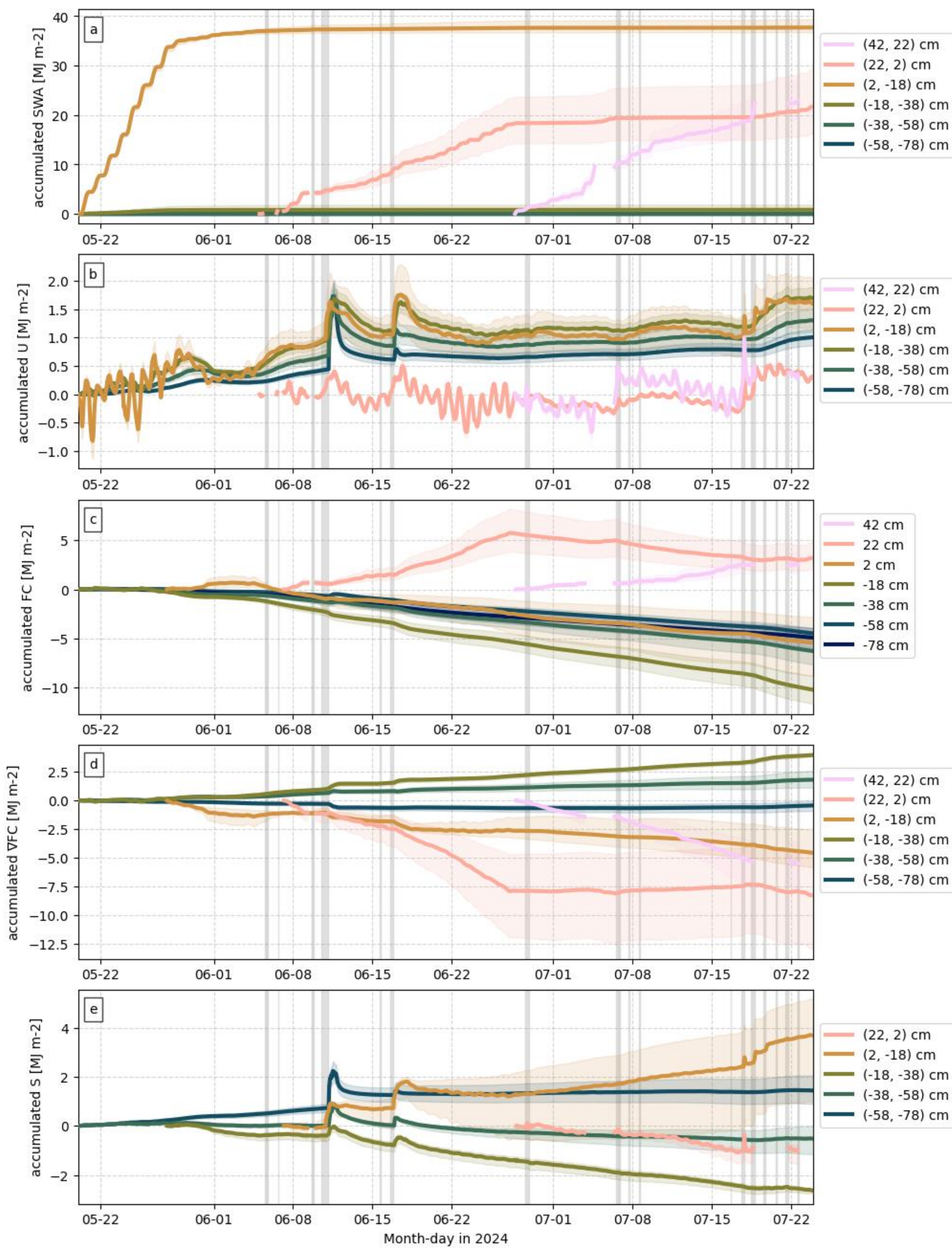


Figure S3: Original plots of cumulative subsurface fluxes assuming linear changes in density.

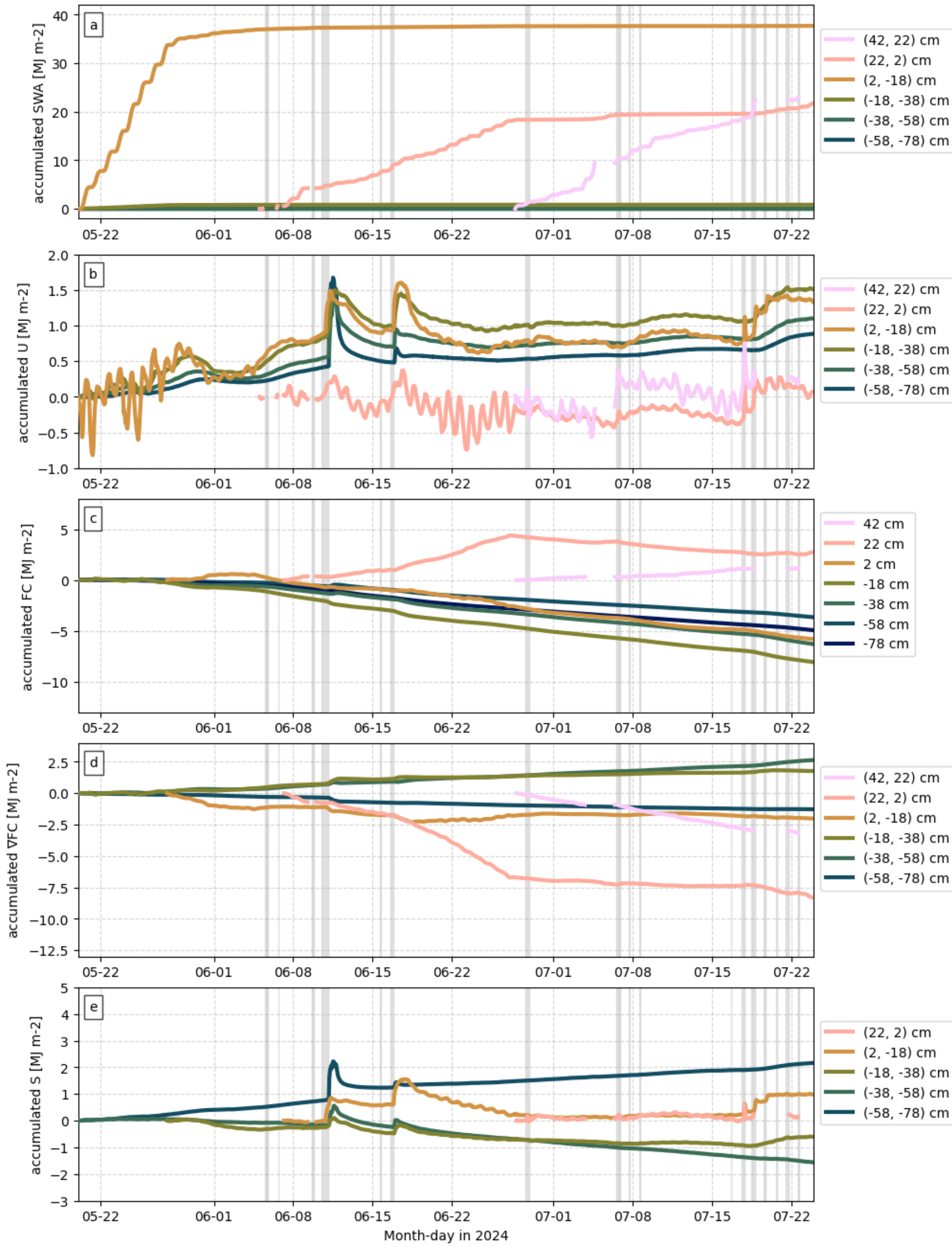


Figure S4: Alternative plots of cumulative subsurface fluxes assuming step changes in density after melt.

Panels b-d (storage and conductive fluxes) have different magnitudes but generally the same basic behavior; changes in temperature seem to drive the temporal variability. Panel e (the residual) still shows a lack of closure, although three of the layers show less drift away from zero, while two of the layers show more drift. This behavior supports our conclusion that better (continuous) density observations are needed for improved flux estimates.

The assumption of density change occurring only after melt events is also not accurate since clearly density can change between the melt events. The final density is also from two weeks after the end of the observations, and during that time there were likely more melt events. Since the main relationships do not change, we will continue to assume linear density changes while including the following statement in the methods:

> Estimating step changes in density instead of linear trends slightly impacts the magnitude of fluxes over time but not the temporal variability or general conclusions of this work.

Minor comments:
L93: "To evaluate the near-surface firn, the temperature string data must be processed and corrected" Somewhat odd phrasing. To evaluate what of the near-surface firn?
This will be updated to be:

> To evaluate **energetic processes of** the near-surface firn…

L129 and L275: "we believe": In my opinion, "believing" is not part of science. Please provide the actual arguments why you think something is the case.
We will reword "believe" in L275 as "we suspect…" to still reflect the uncertainty in discussing how meltwater moved below the surface on June 11, discussed in the major comments above. The original sentence from L129 has been removed to shorten the manuscript.

Fig. 2: The sentence "Shading the temperature string gives information ... and the solar bias correction." should not be part of the figure caption, but of the main text.
This following sentence will be added to the end of the solar correction subsection:

> However, the overall agreement provides some confidence in the solar heating correction and assumptions used for it.

Fig. 3: "is normalized vertically" I would add: "per time step".
We will include this in the figure caption.

L240: "The first month of observations began with spring conditions that transitioned to warming and eventually melt events". I think spring conditions is not well defined (as spring conditions should include warming and eventually melt). I would not use the term spring, but more use terms that describe the actual state of the firn, by using common terminology such as: cold, warming, ripening, percolation phase, etc.
We will rewrite this sentence as follows:

> The first month of observations began with **cold, dry firn** that transitioned to warming…

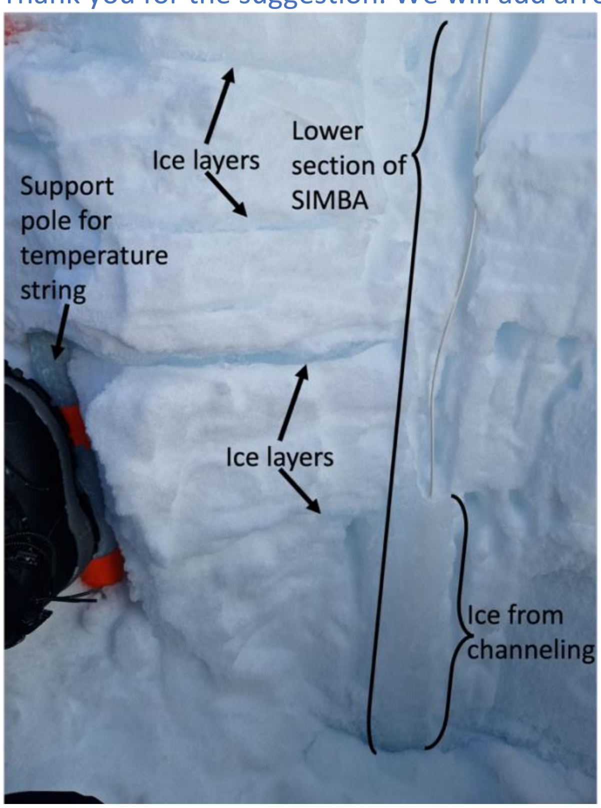
L255: "the largest SWA values, up to 1 C/cm": this implies that the color bar in Fig. 7 is narrower than the actual range of values. I would mention this in the figure caption to avoid confusion.
To avoid confusion, we will remove the reference to magnitudes in this sentence.

L257: "Although the uppermost firn warms and cools with Tskin,rad," I'm not sure I understand what is meant here. That Tskin,rad is driving the warming and cooling of the uppermost firn? This is obviously not the case, because shortwave absorption is a strong contributing factor.
We will shorten this sentence for improved clarity as follows:

> The magnitude of warming in the uppermost firn is approximately twice as large as the cooling and extends further down…

L279: "The latent heat from refreezing of this meltwater impacts the observed temperature structure over the following week" I would say that either: this refers to the big disturbance around June 10/11, which was not lasting for a week, or for the generally increased firn temperature in the period that followed, which would be until June 21st (and likely the rest of the summer season), and thus much longer than a week. I cannot identify a feature that lasted during a week.
We were referring specifically to the five days following the melt event, although the temperatures are elevated for the remainder of the summer, as noted. We will replace "week" with "five days."

Fig. D2: I think the figure should show to the reader the ice layers, for example using arrows, for those who are unfamiliar with typical visual clues one gets from snowpits.
Thank you for the suggestion. We will add arrows to indicate ice layers Fig. D2.



L290: Please explain “solid state greenhouse effect”.

We will cut this part of the sentence to shorten the manuscript.

Throughout: mostly the term "temperature string" is used, but on a few occasions "thermistor string". Apart from the fact that I think thermistor string is a clearer term here, at least consistent terminology should be used.

References to our instrument are now all “temperature string”. We choose to use “temperature” instead of “thermistor” because the instrument uses digital temperature sensors instead of thermistors.

L454: "According to Pfeffer and Humphrey (1996), apparent heat sinks at depth can occur when the actual thermal conductivity is lower than what is assumed, i.e., the calculated conductive flux leaving a layer is larger than the actual flux, making it appear that there is net energy lost": it is not clear why this is a one-way process. I.e., is it not also possible that apparent heat sources at depth can occur when the thermal conductivity is overestimated?

Yes they can, so we will add in the following sentence:

> Apparent heat sources can also appear when the actual thermal conductivity is higher than what is assumed.

L512: "While it is clear observations with high vertical and temporal resolution are necessary for ..., it remains to be seen if such processes are necessary for modeling the seasonal evolution of ice sheets." Why would it not be important to simulate the seasonal evolution of ice sheets? The sentence feels a bit out of place, since no arguments are provided why this would be the case. That makes the sentence a bit irrelevant, I think.

We meant that the *high resolution* of the near surface firn and processes associated may not be important, as in, do you need to model centimeter resolution near the surface to accurately simulate the evolution of kilometers of ice? We will update the sentence to reflect this as follows:

> While it is clear observations with high vertical and temporal resolution are necessary for observing and understanding the near-surface firn temperature variability, it remains to be seen if resolving such processes with high-resolution is necessary for modeling the overall seasonal evolution of ice sheets.