December 21, 2020
The Cryosphere

To the editor,

We would like to thank Drs. Martin Lüthi and Samuel Doyle for their comments on our manuscript. We have made significant edits in response to the suggestions, and we feel that the manuscript is greatly improved.

Reviewer comments are in bold text, and our responses are un-bolded in the document below. Any changes that we have made to the text of the manuscript in response to the comments have been italicized. Additionally, we have uploaded the revised manuscript with our revisions highlighted using the “Track Changes” function so that they are readily apparent to you and the reviewers.

We look forward to receiving your decision.

Sincerely,

Ian McDowell, Neil Humphrey, Joel Harper, and Toby Meierbachtol

Response to Referee Comments by Martin Lüthi

Thank you very much for your suggestions and comments. Your review has greatly improved the quality of the manuscript. All comments by the reviewer are in bold with our responses below. Changes we have made to the text of the manuscript are italicized.

General Comments:

While the argumentation of the paper is logical and easy to follow, I have some reservations about the claims of measurement accuracy, especially given the very crude calibration in the field after deployment.

We appreciate you highlighting this concern and agree that more information should be added so readers can evaluate our results. We have added more information about the sensors used and our calibration methods below when addressing your specific comments. However, we do want to stress that the major results of this study are the observed temperature changes – which are based on sensor resolution. We are not as interested in the absolute temperature of the ice column, and potential calibration errors do not affect the strong cooling signal we observe throughout the lowest portion of the ice column.

The description of the "modeling" is quite unclear. First it sounds like no numerical model is used, but then suddenly there are model results. Please provide details on the model used (FE, FD, ...), spatial and temporal resolution, solution accuracy, implementation details (if not from a standard package). How does this model treat the phase transition (Stefan Problem)? Is there an enthalpy scheme involved (probably not, why not)? Is dissipation due to ice deformation considered as changing source? How is vertical stretching dealt with, which alters temperature gradients, and therefore heat fluxes, over time? Initial and boundary conditions for the modeling exercise seem rather ad-hoc. They should be justified, and it should be shown that the conclusions don't depend on the specifics of these values.

We understand the confusion over our observed temperature profile comparison to modeled temperature profile in using a 1-D finite difference diffusion model. Our intention with this was to simply highlight the fact that other heat sources must be altering the vertical temperature field (which is a rather obvious conclusion). However, we found it interesting that the areas within the observed profiles that were warmer than the idealized diffusion profile are often where we observe cooling. This result suggests to us that previous heating occurred to warm ice, and ice is now cooling back to a more diffusive state.

However, given your concerns about the modeling methodology, we have re-written this section. Rather than introduce a section describing modeling methodology, we have decided to remove these comparisons from our analysis. Instead, we compare each collected temperature profile to the site-average temperature profile. The average temperature profile should be shaped by all processes that transport heat within the ice column as it moves from the ice divide to our field site in the ablation zone (dissipation, geothermal heat fluxes, vertical stretching, latent heat sources, etc.). Deviations from this mean profile will highlight regions that reflect recent changes to heat sources or sinks. Therefore, we have rewritten the two paragraphs beginning on line 164 of the manuscript with tracked changes as follows:

"In addition, we also highlight regions of the temperature profiles that are not only warming or cooling anomalously, but that may be warmer or cooler than would be expected given the average vertical temperature field at our site. The shape of the site-averaged temperature profile results from ice integrating changes in boundary conditions and all upstream thermomechanical processes. Any deviation from this averaged profile within each collected temperature profile will highlight thermal regions that reflect changes either to the local boundary conditions or heat sources or sinks. These changes must have occurred recently for sensors within the closely spaced boreholes to capture temperature fields that do not match the average vertical temperature field at the site.

Discrepancies between each collected profile and the site-average temperature profile are shown in Fig. 5. Generally, there are positive deviations within the lowest portion of the ice column where there is a clear signal of cooling in boreholes 14SA, 14SB, 14N, 15CA, and 15CB, showing temperatures in the lowest portion of these boreholes are higher than those in the site-average profile and cool over time. Encouragingly for this analysis, the temperatures in the upper half of the 14N and 15CA profiles are warmer than the site-average profile and cooling occurs over time. While the pattern of temporal temperature change in boreholes 15S and 15E are not as clear, 15E is colder throughout the full-ice depth than the mean thermal profile, but this borehole also is where sensors record the most warming throughout the profile. Spatially, the southeast portion of our field site does not have a cooling signature in relatively warm ice near the bed, which is evident throughout the other boreholes that have time-series of temperature measurements."

The conclusion that refreezing of water-filled crevasses is intriguing, but it is unique? Could vertical stretching/compression, and thus altered heat fluxes, account for the observed cooling?

We have suggested within the manuscript that likely the solution to this problem is not unique, because we do not know the configuration of the basal stress state at our site that could introduce/remove sources of strain and dissipated heat. However, given the surface strain observed we do not believe that vertical compression would alter heat fluxes sufficiently to cause the cooling we have observed. Given that we are looking for a likely discrete heat source that manifests in cooling, we believe that basal crevasses are the most likely mechanism to do this.

<u>Specific comments:</u>

Line 36: Most other studies also have time series often at higher time resolution (minutes).

We have changed the sentence to read as follows (lines 40 – 41):

"Our data provide sub-daily continuous records of ice temperature over a 2-3 year period, longer than many similar temperature time-series datasets."

Line 57: Please provide information on the manufacturer/series, and on the type of AD converter (with the manufactured probe, or externally on an NTC or PT100)

Thank you for pointing this out. The sentence now reads as follows (lines 61 – 66):

"Cat-5 cables instrumented with TMP102 temperature sensors with 12 bit analog-to-digital converters were installed in the boreholes after drilling and logged with a custom data logger at the surface. Each temperature sensor was controlled by a separate downhole microprocessor that ensured the sensor was powered only as minimally required to obtain a reading. The paired microprocessor handled all digital communications, power control, and data processing to minimize errors caused by self-heating."

Line 61: Yes, it will stabilize at the phase change temperature, but what is this freezing point? It depends on all kinds of things, dissolved air, soluble and solid impurities, crystal curvature, etc. If so, you can only give temperature with respect to local phase change temperature (which itself is not static, but depends on the ambient pressure, which as you claim in your paper changes due to connection to basal water (among other things)). On Gornergletscher, the temperature variations due to variations in the subglacial system were 0.5 K. So, you don't really know what the exact temperature is.

We will incorporate which CC gradient was used for the freezing point calibration. And your point is well-taken; both due to the uncertainty in the freezing point itself and changes in ambient pressure, we will reduce our confidence level to 0.5 °C. This will affect our confidence in the static profiles, but will not influence the calculated temporal rates of temperature change which relies on the sensor resolution. This section now reads (lines 68 – 74):

"We performed a freezing-point calibration prior to complete borehole freeze-up using a Clausius – Clapeyron gradient of -7 x 10^{-8} °C Pa^{-1} (Cuffey and Paterson, 2010). However, due to uncertainty in the Clausius – Clapeyron slope given dissolved air and impurities within the ice and the ambient pressure at the time of calibration, we are confident in the sensors' accuracy to approximately 0.5 °C."

Line 62: You mean "absolute accuracy"? I don't think so, this depends a lot on the CC-Gradient which you don't know.

See correction above.

Line 59: No lab calibration?

Line 65: How much electrical power was dissipated in the sensors? Was the measuring interval ever changed? These are important sources of heat (we spent days puzzling over sudden cooling in boreholes, until we figured out that dissipated electrical power was to blame (Ryser, 2013).

In response to these two comments above, the measurement collection times never changed, thus the measuring interval was constant. Great care was taken to ensure that measurements were taken under exactly the same electrical state over the 2-3 year time period for the reasons that you discuss above. Although we did not do laboratory calibrations on the individual sensors used, extensive laboratory testing was done on representative sensor strings in a cold room to ensure that the sensors had a stable resolution and that problems associated with self-heating could be avoided.

Line 70: It seems that analyzing exactly this transient temperature field would provide convincing evidence that another heat source is needed.

Thank you for this suggestion. We believe that we have shown the need for a heat source to manifests in cooling by allowing the sensors to equilibrate with the ambient ice temperature and discussing a number of thought experiments to eliminate potential processes that might result in cooling.

Line 71: What does "electrically isolated" mean?

Changed to read: *"in the environment isolated from electrical noise"*

Line 74: Please specify whether these jumps happen in near-temperate conditions (i.e. closer than 0.5 K from the pressure melting point). If so, these jumps (at only 4-hour intervals) could be due to subglacial pressure changes. It would be useful to also check these and show time series of temperature and pressure (at least in Supp. Material). If the temperatures are much below the melting point, then this is very likely sensor/electronics noise.

The jumps are due to the nature of the low power, long cable, and data transfer protocol that was developed in house for these instrument strings. The protocol is not as fully robust as a full i2c protocol and has the potential for very occasional line conflicts lasting less than one bit count, The protocol has many advantages in terms of power saving and computational expense, reducing down-hole self-heating. The occasional glitch was a known and accepted problem. If the reviewer is interested; the protocol is a modified and simplified i2c protocol, using modified RS-485 electrical cable protocol. It is the simplified buss assertion part of the protocol that leads to the occasional one bit glitch.

Line 81: A short description of the heat flow model is missing, and here would be a good place to do that. Just method, resolution, BCs etc.

Because we have eliminated our comparison to best-fit modeled diffusion profiles, we will not discuss any modeling here. Our modeled crevasse field in the discussion is just simply to illustrate how distributed cooling could occur, we will leave our discussion of this modeling in the supplement.

Line 84: Give temperature values.

We have added (lines 113 – 115): *"Temperatures are lowest in approximately the middle of the ice column, ranging from ~ -12 °C to -10 °C and increase to ~ -11 °C to -4.5 °C at the surface. Temperatures also increase towards the bed, reaching temperatures that range from approximately -0.9 °C to -0.5 °C."*

Line 86: This is not visible in Fig. 2. Show a close-up of the area close to the bottom, the lowest 10 or 50 m or so maybe as an inset. Also indicate the CC gradient there. Since this is the main topic of the paper, basal temperatures should be shown in detail.

Thank you for this suggestion. We have updated Fig. 2 to include an inset of the lowest 20 m of the ice column. We have also plotted the CC-gradient using the two end member values given in Cuffey and Paterson (2010).

Line 94: It would be interesting to show something like a longitudinal or cross profile of near-basal temperatures. Does this produce anything systematic?

We believe that this can be seen in Figure 5, with 15S and 15E showing no clear cooling signature. We have added this sentence at the end of the paragraph that we added above (lines 192 – 194):

"Spatially, the southeast portion of our field site does not have a cooling signature in relatively warm ice near the bed, which is evident throughout the other boreholes that have time-series of temperature measurements."

Line 110: Conventionally, *k* is used for conductivity and kappa for diffusivity. It would help to use these standard symbols here (first I thought the equation was wrong).

We have made the changes throughout the document to use kappa (κ) when referring to thermal diffusivity of ice.

Line 111: Please give the values used for these calculations, including references. Especially kappa is important here.

We have added a table in the supplement with the values used in this study.

Table S1. *Constants used in this study (Cuffey and Paterson, 2010)*

Property	*Variable*	*Value*	*Units*
Density of ice	ρ_i	*917*	*kg* m^{-3}
Density of water	ρ_w	*1000*	*Kg* m^{-3}
Heat capacity of ice	C	*2097*	$J\ kg^{-1}\ K^{-1}$
Latent heat of fusion	L_f	3.335×10^5	$J\ kg^{-1}$
Thermal conductivity of ice	K	*2.1*	$W\ m^{-1}\ K^{-1}$
Thermal diffusivity of ice	κ	1.09×10^{-6}	$m^2\ s^{-1}$
Clausius-Clapeyron slope	γ	-7.4×10^{-8}	°C Pa^{-1}

Line 118: This is just an analytical equation, so I would not call this a "model".

This sentence has been changed to (lines 155 – 157):

"Comparison of our data and expected temperature changes from the analytical equation (Eq. 1), indicates that the curvatures in our temperature profiles cannot produce the observed temporal changes of temperature over time through diffusive vertical heat flow alone."

Line 120: Calculated. You just take measured d^2T/dz^2 values.

Changed modeled to calculated.

Line 148: The question is rather: are heat sinks necessary? Maybe this simple model is insufficient, and warming is simple. If heat sinks are necessary (as you argue from your 1-D analysis of heat fluxes), what temperature gradients would be needed horizontally to account for the mismatch?

From our 1-D analysis, we could convert these missing heat fluxes into necessary temperature gradients. Because all negative heat fluxes are required in all boreholes across the site, this indicates that heat must be flowing out of the instrumented block of ice, which we later discuss in line 275.

Section 4.2 – 4.2.1 (lines 200 – 204) has been changed to:

"Pervasive cooling in the lowest portion of boreholes where ice is warmer than the site-average profile in some, but not all, of our borehole data is enigmatic, since multiple heat sources exist near the bed, but identifying heat sinks is problematic. In addition, the non-uniform nature of cooling near the bed requires explanation. We discuss several possible heat sinks below."

Additionally we have updated the sentence on lines 350 – 353 to read:

"Given negative heat fluxes are required in the lowest portions of all boreholes (Fig. 4) to create observed cooling, thermal energy likely diffuses away from our site in 3-dimensions, which would result in cooling not captured in our vertical 1-D diffusion calculations."

Line 153: How? How would heat be transported to the melting front?

The sentence now reads (lines 206 – 208):

"Some melting can occur in this temperate ice as the pressure field varies due to ice flow, and transient increases in pressure decrease the melting point."

Line 161: Yes, and you could actually quantify this (as was done i.e. in Lüthi et al., 2002). For this, the data close to the CTS should be enlarged.

Because this has the opposite effect of the observed cooling, we make no attempt to quantify this; however, we have noted that Lüthi et al. (2002) has performed this quantification. Lines 214 – 217 now read:

"Any interstitial water present in the ice would then be colder than the new melting temperature and would then refreeze. Lüthi et al. (2002) estimated the water content of the temperate layer of ice near the bed of Jakobshavn Isbrae has a moisture content of ~ 1%. However, refreezing releases latent heat into the surrounding ice, resulting in warming rather than cooling."

Line 191: Change "heat" to "dissipated heat".

We have made this change (line 247). It now reads *"… accumulates dissipated heat near the bed where strain rates are highest."*

Line 193: I would qualify this better. This internal strain heat source is removed while frictional heating prevails at the base.

The sentence now reads (line 248 – 249):

"At our site ice moves primarily by basal sliding (Maier et al., 2019), and this internal strain heat source is removed although frictional heating still exists at the base."

Lines 194-195: Yes, but does it matter why the profile looks as it does? If you measure cooling, but the profile does not provide enough thermal gradient, then you either need more dimensions for diffusion or an active cooler.

Yes, and we have shown that the shape of our temperature profiles that would result from accumulating strain heat will not produce cooling from vertical conduction alone. We have added a sentence to explain this at the end of the paragraph (lines 254 – 256).

"Additionally, we have shown that that the shape of the temperature profiles that would result from previously accumulated strain heat near the base will not produce the magnitude of observed cooling from vertical thermal conduction alone. An additional heat sink must be required."

Line 203: Cooling caused by variations in the basal boundary stress state is not obvious, please explain.

The sentence now reads (lines 261 – 263):

"However, it does point out that variations in the basal boundary stress state can cause minor heating or cooling if strain and thus dissipated heat is removed or reduced…"

Line 221: By definition of "crevasse", probably yes. But I doubt that basal cracks are as nicely aligned as surface cracks. Deep in crevasses the patterns become chaotic, and this is probably even more so if they form under water. So maybe this is just some void space that was water filled for a while and refreezes. The exact shape probably does not matter for the discussion at hand, but this could be quite spongy.

We agree that these are likely not simple planar features. Additionally, even if they form perpendicularly to the bed, deformational gradients will deform these features, and they potentially can intersect more recently formed crevasses. However, to simply model the heat source, we treat basal crevasses as planar features. We have changed this section starting on line 282 to read:

"Basal crevasses can have a thermal influence on discrete but large regions of ice. Because these fractures are distinct features, the fact that we do not see a clear cooling signature in relatively warm ice throughout the base of the ice column at our site is not surprising. Slight warming in cold ice in borehole 15E and no clear signal in borehole 15S provides evidence that the ice is being affected by a thermal process that is not distributed in nature.

Multiple fractures can open similar to crevasses at the surface, and their growth is likely vertically restricted above the bed due to the cold, stiff central core of the ice column which reduces fracture propagation. Therefore, these features only affect the lower portion of the ice column. Additionally, basal crevasses likely are not simple planar features; strong deformational gradients near the bed can alter their shape and cause them to intersect more recently formed crevasses. Although basal crevasses may be more complex than an en échelon arrangement of vertical fractures, for simple modeling we focus on the thermal disturbance by basal crevasses that allow the influx of a plane of water from the bed up into the cold ice."

Line 222: Why would basal crevasses be vertically restricted when propagating from the bed?

See rewritten section above.

Line 240: Again, better use kappa.

We now use κ throughout the manuscript for thermal diffusivity.

Line 251: This is somewhat ambiguous. Better: "for the time since water-filled basal crevasses were formed".

The sentence now reads (line 322): *"It is tempting to estimate the time since water-filled basal crevasses were formed."*

Line 263: They also could form under dry conditions given enough longitudinal or shearing stresses. This is just a fracture process. But they might not open up, unless water under high pressure forces them open and precludes rapid healing.

We are trying to highlight the subglacial conditions at our site are conducive to basal crevasse formation because of high basal water pressures observed in Greenland's ablation zone. The beginning of this paragraph has been modified as follows (lines 333 – 334):

"Basal water pressures at or above ice overburden pressure promote the growth of basal crevasses 10s of meters above the bed."

Line 268: there are many observations of basal seismic sources compatible with fracturing.

Thank you for pointing this out. We have added (lines 341 – 343):

"Additionally, observed stick-slip behavior and hydraulic jacking from rapid delivery of surface meltwater through passive seismic surveys could provide another mechanism for basal crevasse formation (Moore et al.,2013)."

Reference added:

"Moore, P. L., Winberry, J. P., Iverson, N. R., Christianson, K. A., Anandakrishnan, S., Jackson, M., Mathison, M. E. and Cohen, D.: Glacier slip and seismicity induced by surface melt, Geology, 41(12), 1247–1250, doi:10.1130/G34760.1, 2013."

Line 273: This is incorrect. There is no "abrupt warming" due to refreezing, but the boundary condition at the crevasse wall is melting temperature until all water is frozen. So the width of the crevasse simply determines how long it exists until completely refrozen.

We have changed the sentence to read (lines 348 – 349):

"The 1-D temperature field evolution illustrates the warming that occurs due to the release of latent heat immediately as the crevasse completely refreezes."

Line 275: So do a 3D modeling, this is not hard and one standard example in any Finite Element code (e.g. MOOSE, Libmesh, MARC, Ansys, Comsol).

We appreciate the suggestion. However, like other recent observational studies of Greenland ice temperature (e.g. Seguinot et al., 2020, Doyle et al., 2018, Hills et al., 2017, Harrington et al., 2015) that do not attempt highly detailed modeling, we would prefer to keep this study focused on what the observations explicitly show. Particularly now that we have removed comparison to modeled profiles and only compare our temperature profiles to site average profiles, we believe that the results will come through stronger without spending too much of the text describing a 3-D modeling exercise and associated assumptions.

Line 279: Moulins are not linear features, but usually a series of vertical shafts connected through meandering passages, much like any Karst cave system. And potentially they continue as intraglacial channels of arbitrary direction at depth (as e.g. observed by Catania).

We agree that moulins are likely not nice linear features; however, we model them as such since they appear much more pipe-like at the surface. We have added language to indicate that these may be complex features on lines 355 – 357.

"While the internal architecture of moulins is likely complex, as a simple model, we conceptualize moulins as linear pipes that release thermal energy from the surface to the bed because of how these features are manifested at the surface."

Line 282: Moulins usually form in a row with certain spacing, since surface features leading to crevasse formation are stagnant while the ice flows through them (Catania,

2010). But I agree that moulins are probably not the best explanation for the features unless they themselves are some kind of crevasse system at depth. Nobody knows.

Lines 283 – 284: As mentioned above, active moulins are not important for the argument, but active moulins times velocity which gives their spacing along the flow line. Since they get advected and re-created every 100 m (or so), quite a large portion of the ice could be affected by their thermal influence, but extremely inhomogeneously.

To address these two comments above we have added (lines 362 – 365):

"In the ablation zone surrounding Issunguata Sermia, active moulins have a mean density of 0.1 per square kilometer (Smith et al., 2015), and Catania and Neumann (2010) found relict moulins to be spaced ~ 1 km apart, which suggests that it is unlikely that multiple moulins have thermally altered the instrumented block of ice with a surface area of ~ 0.125 km^2."

Added reference:

Catania, G. A. and Neumann, T. A.: Persistent englacial drainage features in the Greenland Ice Sheet, Geophysical Research Letters, 37(2), doi:10.1029/2009GL041108, 2010.

Line 294: But: at your site there is no temperate layer to support this. And at our sites (and at Store) the temperate ice is mostly stiffer than Paterson would make us believe. This story is much more complicated.

This section reads (lines 373 – 376):

"The lack of a thick basal temperate ice layer indicates the transient thermal influence of basal crevasses. Ice viscosity is highly temperature dependent, thus basal crevasses could create regions of temporarily enhanced deformation, although previous observations in Greenland have shown that deformation rates in temperate ice are often lower than theory predicts (e.g. Ryser et al., 2014; Doyle et al., 2018; Maier et al., 2019)."

Added reference:

Ryser, C., Lüthi, M. P., Andrews, L. C., Hoffman, M. J., Catania, G. A., Hawley, R. L., Neumann, T. A. and Kristensen, S. S.: Sustained high basal motion of the Greenland ice sheet revealed by borehole deformation, Journal of Glaciology, 60(222), 647–660, doi:10.3189/2014JoG13J196, 2014.

Line 302: Exactly. So these are probably not just nice cracks as we observe at the surface, but a chasm of destroyed ice intermingled with water, much like the underside of icebergs. Or like the Jakobshavn surface filled with water. So maybe a sponge-like structure, or a chaotic network of cracks in all directions is a good mental picture. "Crevasse" somehow alludes to en echelon, nicely shaped cracks.

We agree that the basal hydrologic system is likely complex. However, we feel that these two observations point out the intricacies of the hydrologic system without further speculating on its characteristics.

Line 328: DOIs should be given for all references.

We have added DOIs to references where they are missing if a DOI is available.

<u>Technical corrections:</u>

Line 12: Change strain heat to strain heating

We will leave this as strain heat to maintain the parallelism in the sentence.

Line 17: Change temporal to transient

Changed.

Lines 28 – 32: Change references Luthi et al. to Lüthi et al.

We have changed this throughout the document and apologize for the typographic error.

Lines 28 – 33: Probably could make these citations less repetitive, now they are repeated after each sentence.

We will include them all at the end of this last sentence as a list *(e.g. list of references)*.

General technical comment - references to figures should be spelled out (e.g. Figure 2 instead of Fig. 2) and equations should be spelled out (e.g. Equation 2).

We are following the instructions listed under The Cryosphere Manuscript Composition that say:

> "The abbreviation "Fig." should be used when it appears in running text and should be followed by a number unless it comes at the beginning of a sentence, e.g.: "The results are depicted in Fig. 5. Figure 9 reveals that..."."

Additionally these instructions say:

> "In the text, equations should be referred to by the abbreviation "Eq." and the respective number in parentheses, e.g. "Eq. (14)". However, when the reference comes at the beginning of a sentence, the unabbreviated word "Equation" should be used, e.g.: "Equation (14) is very important for the results; however, Eq. (15) makes it clear that..."."

Line 109: Remove parenthesis around the Q term in Eq. (1)

We have removed these parentheses.

Line 123: Remove "temporal".

We have removed this word.

Line 149: Change dismiss to discuss?

Changed.

Line 251: A timescale?

This sentence has been changed to read:

"It is tempting to estimate the time since water-filled basal crevasses were formed."

Line 252: Change under constrained to under-constrained.

Changed.

Line 257: Change decay to dissipation.

Changed.

Line 291: Change strain heat to strain heating.

Again, we will leave this as is for sentence parallelism.

Line 315: Is "warm" a verb? If so, maybe it should be "warms".

This can serve as a verb, and because we are talking about basal crevasses, which is plural, we will use the plural form of the verb, warm.

Figure comments:

Figure 1: Black triangles are barely visible. Why not use more colors, also for the labels? Source and year for the WorldView 2 Image?

We have made the WorldView 2 image lighter, and increased the symbols used to indicate borehole and GPS station locations. Additionally, we have increased the font size to make the figure more readable:

Updated figure and caption:

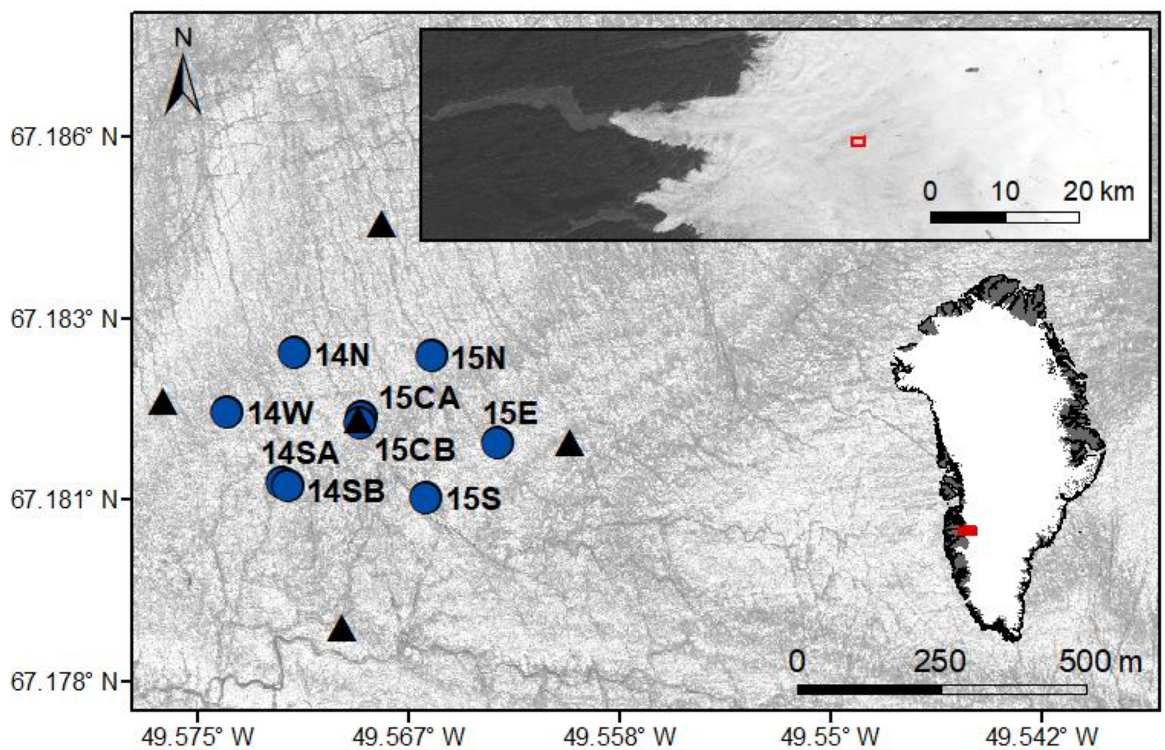


Figure 1: Map of the field location showing the layout of the 9 boreholes (blue dots) and the 5 GPS stations (black triangles). Borehole nomenclature is determined by the year that they were drilled and the direction from the center borehole and GPS station. The boreholes are located ~33 km from the margin of Issunguata Sermia outlet glacier shown in a LANDSAT 8 image taken in July 2014 in the upper-right inset. The location in southwestern Greenland is shown in the lower inset and boreholes are plotted on a WoldView-2 image from July 2012 (Copyright 2011, DigitalGlobe, Inc.).

Figure 2: Wouldn't distance below surface be more meaningful for the full plots? Then you could also plot the Clausius-Clapeyron gradient which is very important, especially for this study. Maybe just have an inset with the bottom 50 m or so and the CC-Gradient? The gray legend is not easy to read, just change it to white background.

We appreciate these suggestions and have added the CC-gradients and an inset showing the lowest-most temperatures. We will continue to plot ice temperatures with respect to height above the bed because we believe this reference frame makes the most sense as we discuss temperatures in the lowest portion of the ice column.

Updated figure and caption:

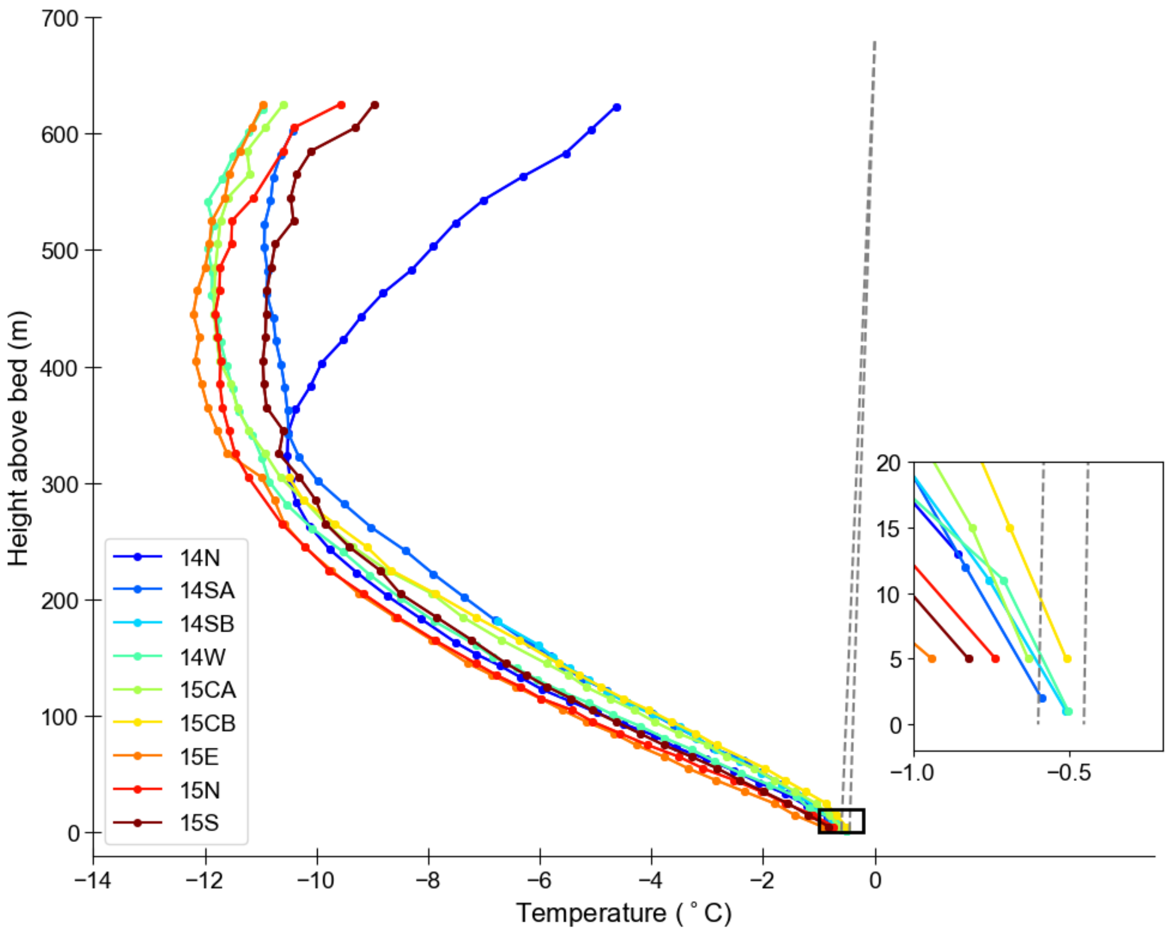


Fig 2: Temperature profiles for each borehole plotted with respect to each sensor's height above the bed. Sensor locations are marked with points and temperatures between sensors are linearly interpolated. Dashed lines indicate the range of pressure melting temperatures given the various Clausius-Clapeyron gradients presented in Cuffey and Paterson (2010). Black box indicates the extent of the inset showing temperatures in the lowest 20 m.

Figure 3: Use colored dots, and larger fonts. Also, the depths don't have to be repeated in each subplot. It might also be useful to show the temperature profile in each panel with a distinct color, but maybe this is just too confusing. Or at least indicate CTS and height above bed of the temperature minimum in each panel.

While we do understand and appreciate your suggestion to use color for the following plots, we feel that it would actually make it less accessible to more readers. Additionally, because the CTS height is essentially the same as the bottom-most sensor of each profile, we do not think it would add much detail to this plot. We have enlarged the font size in this figure and reduced the axis redundancies to increase readability.

Updated figure and caption:

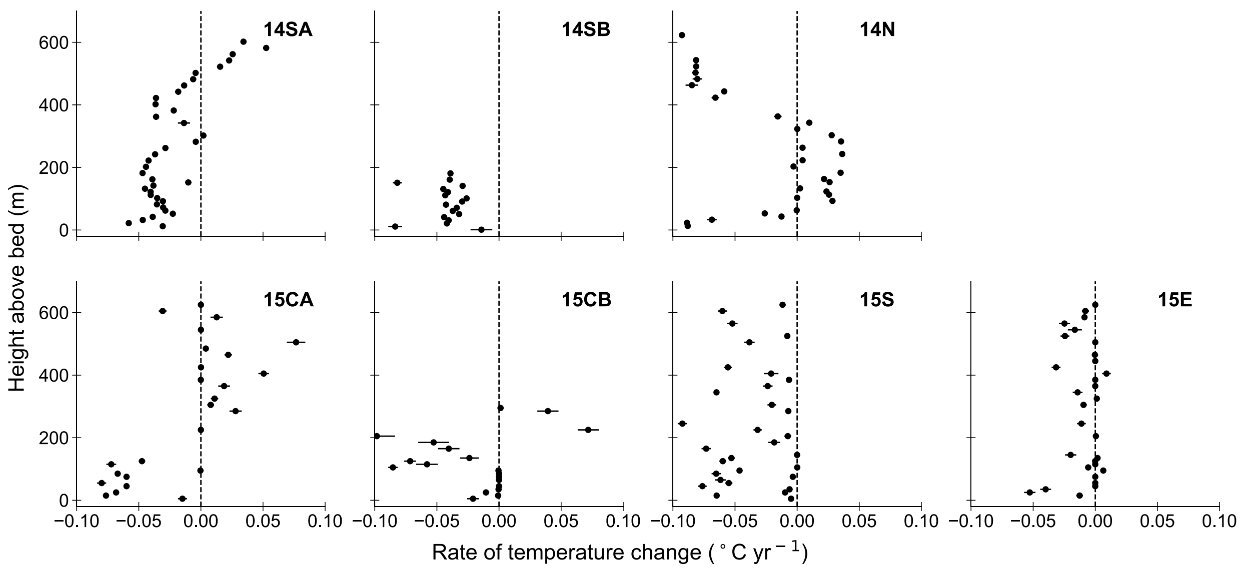


Figure 3: Rates of temperature changes for each borehole plotted against each sensor's height above the bed. Boreholes drilled in 2014 show trends between July 2015 and July 2017, while temperature trends in boreholes drilled in 2015 represent temperature changes observed between July 2016 and July 2017. Error bars indicate 2 standard errors from the mean rate, determined by the linear regression.

Figure 4: Use colors for the curves. Also change units to mW m^{-2}. Why are there less panels than in Figure 3? Could they be combined, as they show almost the same?

Thank you for suggesting changing the magnitude of the x-axis. We have made this change. Additionally, we have added the missing non-full-depth profiles that were missing. While this plot is very similar to Fig. 3, this plot is part of our analysis, and we do not want readers to get confused by displaying results and analysis in the same plot. We will leave it as a separate figure.

Updated figure and caption:

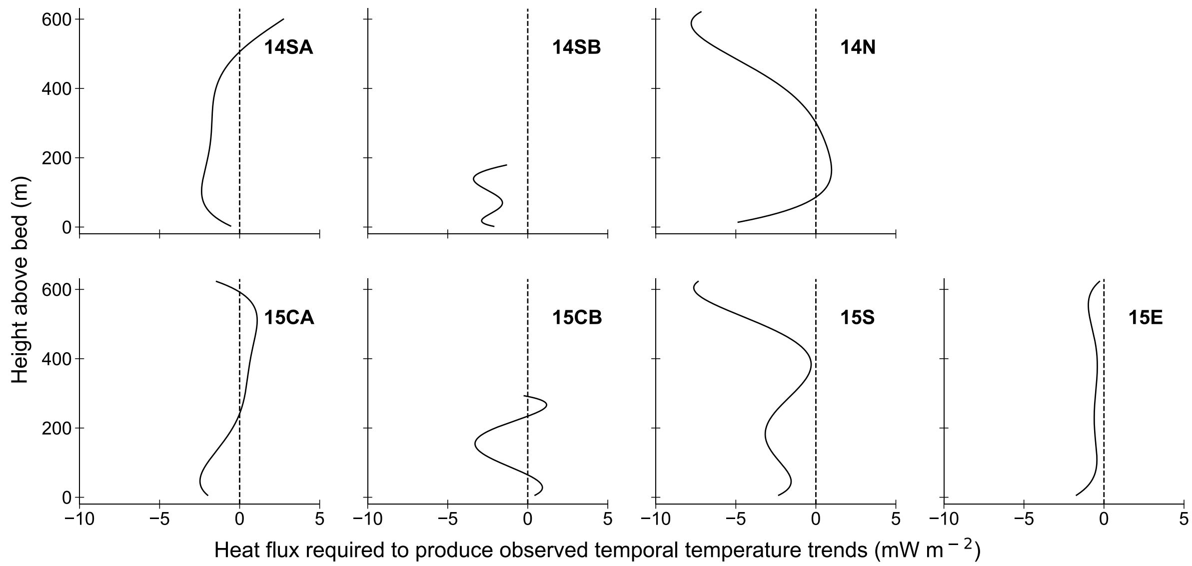


Figure 4: Additional heat fluxes required to maintain observed rates of temperature change if the collected static temperature profiles evolve through vertical conduction over time. Dashed line indicates the switch between a required heat source or sink.

Figure 5: Colors might be more useful, same as in Fig. 3. Why only show full-depth profile, other profiles would be as interesting.

We have added the other profiles to this figure, which now examines temporal temperature trends plotted concurrently with observed temperature profile deviations from the site-average temperature profile.

Updated figure and caption:

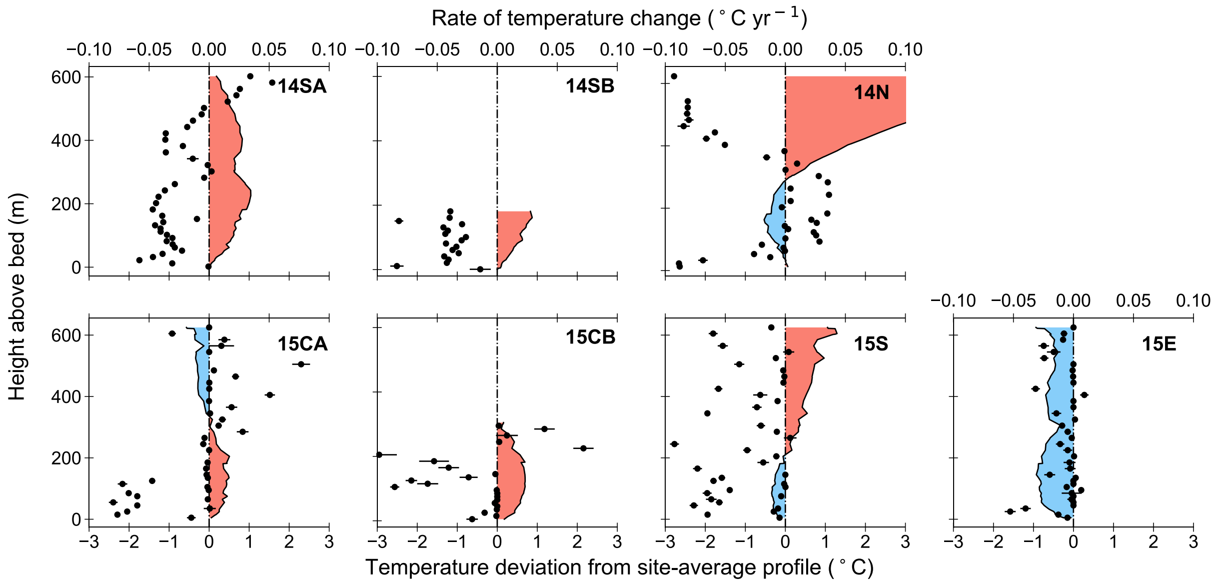


Figure 5: Rates of temperature change for each borehole (black dots) plotted with temperature deviations within each borehole from the site-average temperature profile. Red shaded regions indicate that the temperature profile of that borehole is warmer than the site-average profile, and blue shaded regions show the temperature profile is colder than the site-average profile. Error bars on the calculated rates are the same as in Fig. 3.

Figure 6: Why is this black and white, colors would be more useful. Change "thick" to "wide". This is a strange graphic, why don't you just show the temperature iso-lines? Better (shorter) vertical label needed.

To make it more accessible for all viewers, we will leave this figure in black and white. We have made the label in panel C to make it easier to read. We don't only show the temperature isotherms because we want to explicitly illustrate how a process that injects heat into the ice leads to not only warming, but produces cooling at rates that can match those observed at our field site.

Updated figure and caption:

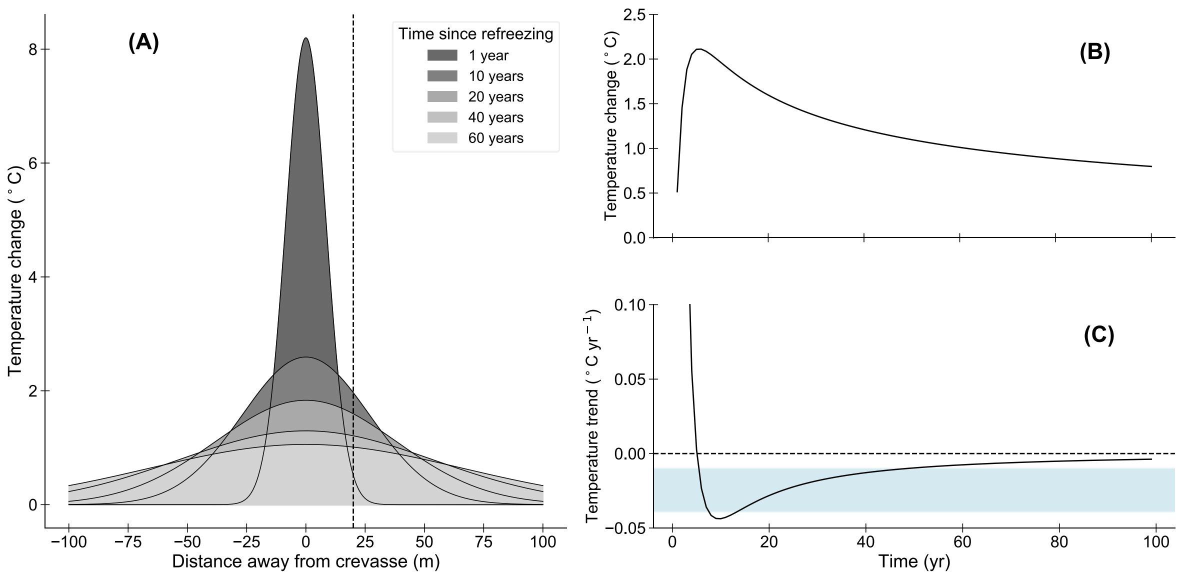


Figure 6: (A) Modeled changes in temperature after a crevasse 1 m wide refreezes. Darker colors show earlier times. Dashed line indicates a distance 20 m from the crevasse, which temperatures and temporal rates of temperature change are shown in B and C. (B) Changes in temperature over time at 20 m away from the basal crevasse. (C) Corresponding rates of temperature change for panel B. The blue shaded region shows the range of averaged rates of cooling in the lowest 100 m of the boreholes.

Figure 7: Use colors. What is the use of the main plot, the inset shows it all, just display the most important part.

We have enlarged the inset and only show that as we agree it is the most important part of the figure and should be prominently displayed. We experimented with various colormaps to cycle through and display the temperature field evolution, however, we believe that the greyscale evolution is easiest for readers to see.

Updated figure and caption:

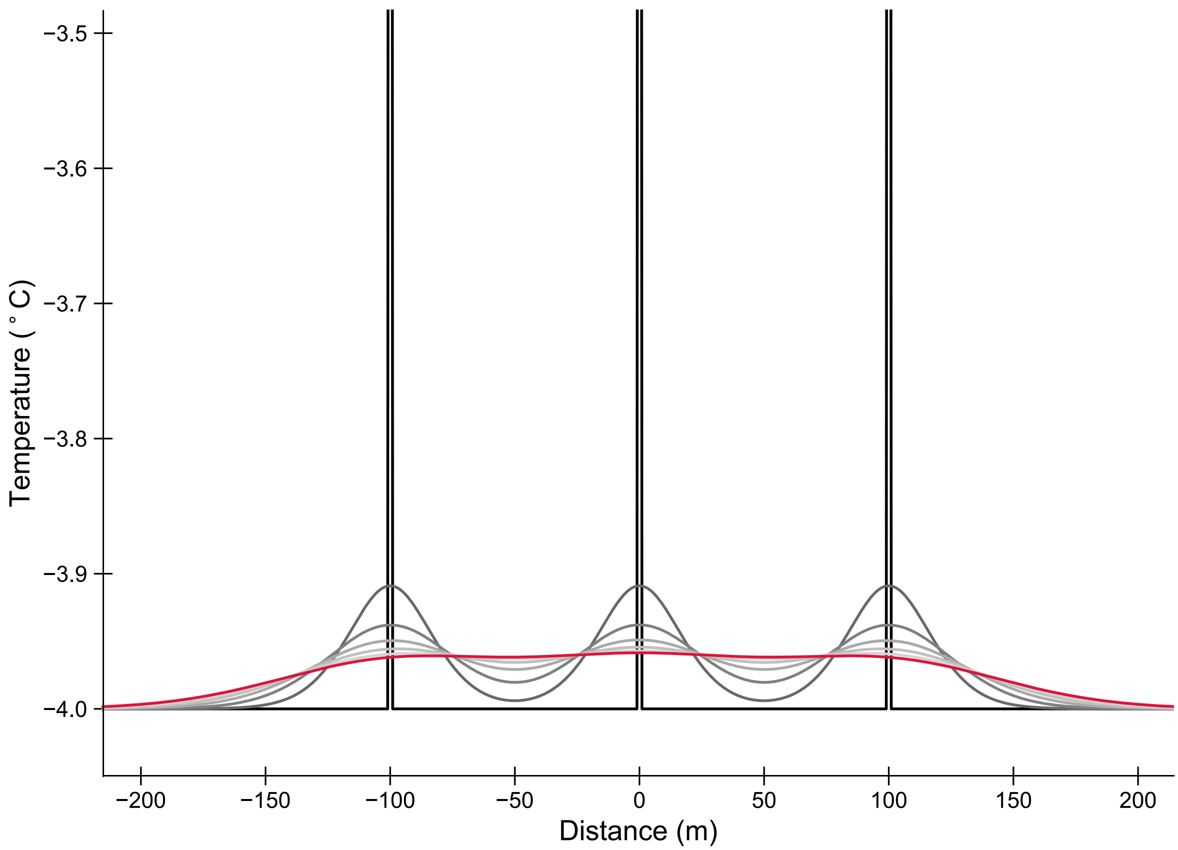


Figure 7: Evolution of a synthetic 1-D temperature field from 3 basal crevasses spaced 100 m apart. Temperatures are plotted every 5 years, and older temperature fields are shaded darker shades of grey. Red line shows the temperature field 30 years after refreezing.

Response to Referee Comments by Samuel Doyle

Thank you very much for your comments and suggestions; they have greatly improved the quality of the manuscript. All comments by the reviewer are in bold with our responses below. Changes we will make to the text of the manuscript are italicized.

General Comments

I have a number of general comments that should be addressed before publication. These relate to the (i) presentation of a number of important sections only within the supplement when it is arguably more appropriate and helpful to present them in manuscript, (ii) lack of important details on the methods, (iii) the conclusion of "consistent" basal cooling when the observations suggest heterogeneity.

We want to thank you for your thoughtful comments as they have greatly improved the quality of the manuscript. We have addressed each of your general comments and have made the corresponding technical corrections that you have suggested.

(i) Given the implications of the very interesting conclusions regarding the thermal signature of basal crevasses, it is imperative that the limitations of the techniques used be presented clearly and up front. The conclusions are based on measuring very small temperature changes (< 0.1 K/year) using (potentially) relatively inaccurate and low resolution sensors. The actual accuracy is difficult to assess as the sensor model number is never given.

We appreciate you bringing up you concern about sensor accuracy. We will add additional information about the sensors so readers can independently assess the results. We would like to stress, however, that the key result of this study is the temporal temperature trends, which is dependent upon the sensor resolution and not sensor accuracy. We are not as interested in absolute temperature, and potential calibration errors or inaccuracies will not affect the observed temporal temperature trends.

We have added more information on the temperature sensors on lines 61 - 66:

"Cat-5 cables instrumented with TMP102 temperature sensors with 12 bit analog-to-digital converters were installed in the boreholes after drilling and logged with a custom data logger at the surface. Each temperature sensor was controlled by a separate downhole microprocessor that ensured the sensor was powered only as minimally required to obtain a reading. The paired microprocessor handled all digital communications, power control, and data processing to minimize errors caused by sensor self-heating."

Important assumptions on the equilibration to the undisturbed ice temperature and a section describing dataset reconstruction are currently only presented in full in a short supplement. Some details of the modelling may also appear only in the supplement. Moving these sections to the main manuscript would be more appropriate.

Thank you for bringing up this concern. We have moved some information from the Supplement to the main manuscript. On lines 79 - 85 we have added:

"The temperature of the borehole decays towards the ambient ice temperature. After 1 year < 0.1% difference remains between the borehole temperature and ambient ice temperatures. For ambient ice temperatures below -1 °C, this is well below sensor resolution and does not affect our observational data. Additionally, regressing equation 24 in Humphrey and Echelmeyer (1990) between 1 and 3 years after borehole closure results in a rate of residual cooling on the order of -10^{-4} *°C* yr^{-1}*. To avoid any potential contamination of both the static and temporal trend of the sensor temperature data from the thermal disturbance of installation, we disregard temperatures recorded in the first year after drilling in our analysis."*

Additionally, we have added more information on lines 100 – 109 about the dataset reconstruction. Our goal was for readers to not get bogged down with this information in the manuscript, and to look at the Supplement if interested. But we agree, this is an important step that we have taken and should be addressed more in the main text. We have added:

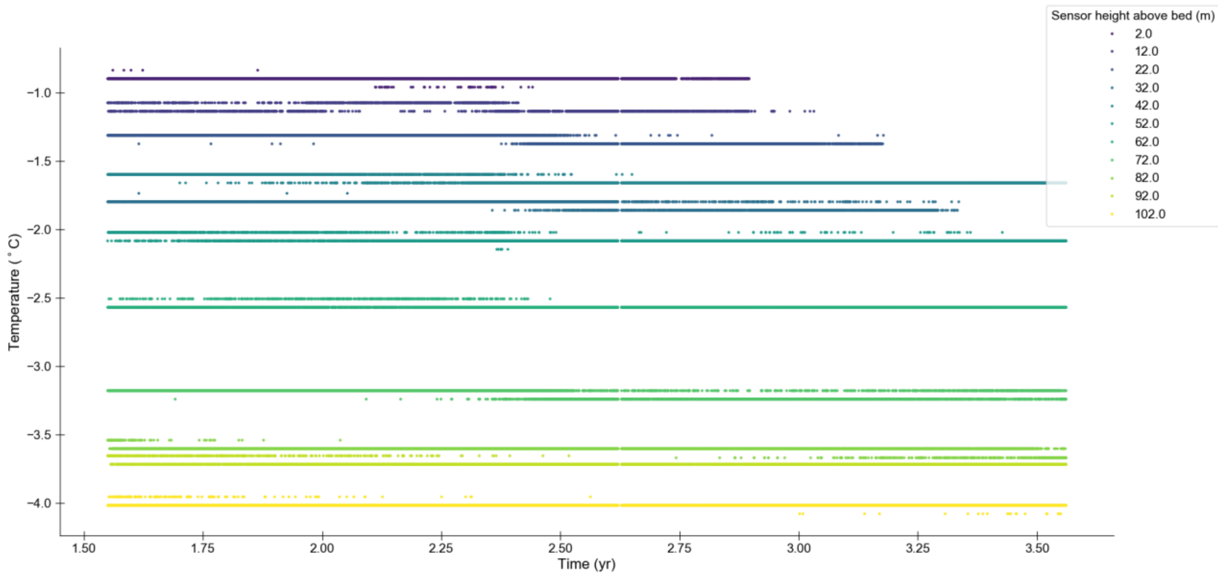
"Often the first and last digital steps of this record have fewer entries recorded due to the restrictions of the sampling window. Assuming that temperature change is close to linear with time, we expect that with a longer record, the sensor would record approximately the same number of readings at each resolution step. Short data lengths occurring at either the beginning or end of the records are likely truncated by the restricted time period of our study. To correct for our restricted sampling time, we pad the first and last steps in our data to match the length of a fully-recorded resolution step. This is performed by copying the required length of data from a full resolution step, and pre- or post-appending it to the truncated data, while equating the temperatures. This procedure is illustrated in Fig. S2, which also shows the difference in derived rates of temperature change over time."

My main concern on reading the manuscript was whether the ice had actually thermally equilibrated to the undisturbed ice temperature before the cooling was observed. The assumption that the temperature disturbance of the drill is negligible after 1 year is fundamental to the analysis and results presented - discussion of this should not be hidden in the supplement.

See our textual additions addressed in the comments above.

The underlying dataset comprising temperature time series is not shown with the exception of one example provided in the supplement. Can more time series be presented in the manuscript or perhaps more appropriately in a supplement?

Because we have time series from over 300 temperature sensors, we feel that presenting all in figures in the Supplement would be essentially unreadable. We do understand the desire to see more temperature time-series. However, because of the oscillatory nature of the data, these time series do not plot nicely in a single figure. We could plot a moving average of the raw data, however, since we did not apply this method to determine the rate of temperature change, we feel that simply displaying our scatterplot of calculated rates in the manuscript is sufficient. For your interest, below is an example of temperature time series from the lowest 100 m of borehole 14SA.



(ii) The temperature methods should be described in full. The sensor model is never stated. An accuracy of 0.1 degrees Celsius is assumed following down-borehole calibration against the pressure-dependent melting temperature, but this level of accuracy is hard to achieve with off the shelf sensors. For example, DS18B20 temperature sensors, which match the details given, have an accuracy of 0.3 degrees Celsius after ice bath calibration according to the manufacturer. Estimation of the pressure-dependent melting temperature is also prone to error as it requires knowledge of the depth, ice density and Clausius-Clapeyron gradient, none of which are precisely known. The Clausius-Clapeyron gradient used for calibration is never stated. Unless more details are given the accuracy of the measurements cannot be independently assessed.

We agree that we should add more information to address sensor accuracy. However, again we would like to note that our main observation of cooling is not affected by sensor accuracy and only by the sensor resolution. We have added the sensor information as requested in a comment above. And have added more information about our freezing point calibration on by changing the sentences on lines 68 - 74:

"We perform a freezing-point calibration, using a Clausius – Clapeyron gradient of -7 x 10^{-8} °C Pa^{-1} (Cuffey and Paterson, 2010). However, due to the uncertainty in the Clausius – Clapeyron slope given dissolved air and impurities within the ice and the ambient pressure at the time of calibration, we are confident in the sensors' accuracy to approximately 0.5 °C."

Also, regarding the temperature methods, very small rates of temperature change were estimated (e.g. < 0.1 K/year) from linear regression of coarsely sampled (0.0625 K resolution) data points. The time series had to be reconstructed in an attempt to mitigate the effect of sampling a rate of change near the sampling resolution for an insufficient period of time. While this method for linear regression appears to make sense it does, however, represent a limitation of the dataset which would be better stated up front in the methods section rather than hidden in the supplement.

Please see the textual additions regarding the linear regression methods addressed in your comment above.

It would be great if the effects of the linear regression method could be analysed by artificially truncating and then analysing a real (or possibly synthetic) temperature time series with the same characteristics. This would give an accurate picture of the limitations. It may also help provide a robust method that future studies could use to examine similar measurements.

We believe that Fig. S2 provides insight as to how our method affects the linear regression slopes on the padded and truncated data and shows that where readings at each resolution step do not span the same amount of time, the regression slope may be biased. Our method provides a more conservative estimate of the temporal rate of change of temperature. In the example given in Fig. S2, the slope to the original line of best fit was $-5.3 \pm 0.05 \times 10^{-2}$ °C yr^{-1}, while the slope of the line fit to the augmented data was $-4.4 \pm 0.02 \times 10^{-2}$ °C yr^{-1} (mean ± st. error). We will add this information to the supplement.

On line 46 in the Supplement we have added:

"Our method provides a more conservative estimate of the rate of temperature change over time than if we had we not padded the entries in the first and last resolution steps. In Fig. S2, the slope to the original line of best fit was $-5.3 \pm 0.05 \times 10^{-2}$ °C yr^{-1}, while the slope of the line fit to the augmented data was $-4.4 \pm 0.02 \times 10^{-2}$ °C yr^{-1} (mean ± st. error)."

(iii) In the abstract the authors state that "temperature sensors . . . consistently record cooling over time within the lowest third of the ice column". This suggests that every temperature sensor in the lower third of the ice column showed a decrease through time, which does not reflect the heterogeneity in temperature change presented in Figure 3. Many sensors recorded no change and some recorded an increase in temperature. The wording in the abstract (and possibly elsewhere) could be more appropriate. Can an explanation be provided for this inter- and intra-borehole variation in temperature change? It may be as important an observation as that of cooling.

This is a good point and one that we have thought about as well. We will remove the language that suggests that we consistently record cooling from the abstract and conclusion. Additionally, we have changed a portion of our analysis and have compared each observed temperature profile to the site-average temperature profile, instead of modeled best-fit diffusion profiles (see newly written section in response to Martin Lüthi). This shows that in 15E particularly that there is some warming occurring in a site that is colder than the site-average profile. We believe that this inter-borehole variability provides additional evidence for our basal crevasse hypothesis, as basal crevasses are dispersed features that are likely widely spaced. It is likely that not every borehole would be thermally affected by these features. We have tried to incorporate more of a discussion of these heterogeneities.

Our analysis now compares observed profiles to the site-average temperature profile, and the two paragraphs beginning on line 164 now read:

"In addition, we also highlight regions of the temperature profiles that are not only warming or cooling anomalously, but that may be warmer or cooler than would be expected given the average vertical temperature field at our site. The shape of the site-averaged temperature profile results from ice integrating changes in boundary conditions and all upstream thermomechanical processes. Any deviation from this averaged profile within each collected temperature profile will highlight thermal regions that reflect changes either to the local boundary conditions or heat sources or sinks. These changes must have occurred recently for sensors within the closely spaced boreholes to capture temperature fields that do not match the average vertical temperature field at the site.

Discrepancies between each collected profile and the site-average temperature profile are shown in Fig. 5. Generally, there are positive deviations within the lowest portion of the ice column where there is a clear signal of cooling in boreholes 14SA, 14SB, 14N, 15CA, and 15CB, showing temperatures in the lowest portion of these boreholes are higher than those in the site-average profile and cool over time. Encouragingly for this analysis, the temperatures in the upper half of the 14N and 15CA profiles are warmer than the site-average profile and cooling occurs over time. While the pattern of temporal temperature change in boreholes 15S and 15E are not as clear, 15E is colder throughout the full-ice depth than the mean thermal profile, but this borehole also is where sensors record the most warming throughout the profile. Spatially, the southeast portion of our field site does not have a cooling signature in relatively warm ice near the bed, which is evident throughout the other boreholes that have time-series of temperature measurements."

The third and fourth paragraphs in section 4.3 starting on line 282 now read:

"Basal crevasses can have a thermal influence on discrete but large regions of ice. Because these fractures are distinct features, the fact that we do not see a clear cooling signature in relatively warm ice throughout the base of the ice column at our site is not surprising. Slight warming in cold ice in borehole 15E and no clear signal in borehole 15S provides evidence that the ice is being affected by a thermal process that is not distributed in nature.

Multiple fractures can open similar to crevasses at the surface, and their growth is likely vertically restricted above the bed due to the cold, stiff central core of the ice column which reduces fracture propagation. Therefore, these features only affect the lower portion of the ice column. Additionally, basal crevasses likely are not simple planar features; strong deformational gradients near the bed can alter their shape and cause them to intersect more recently formed crevasses. Although basal crevasses may be more complex than an en échelon arrangement of vertical fractures, for simple modeling we focus on the thermal disturbance by basal crevasses that allow the influx of a plane of water from the bed up into the cold ice. The refreezing of this injected water releases heat into the surrounding ice. Accurately modeling the thermal disturbance created by a basal crevasse is hampered by our lack of knowledge of these features, since they have only been directly observed under mountain glaciers (Harper et al., 2010)…"

<u>Minor comments & Technical corrections</u>

Thank you for pointing out these areas where we could provide more clarity or fix typographical errors.

Is it necessary to state hard-bedded in the title? It is contradicted in the field site section which states that a thin (up to 0.1 m) layer of sediment was found.

Thank you for requesting clarification. Harper et al. (2017) presented a suite of borehole experiments that found that the ice at this field site overlies a relatively clean bed without a thick layer of deformable sediment. We want to emphasize that the hydrologic system here is indicative of an ice/bedrock system rather than an ice/till system. For clarification, lines 52 – 54 of the field site section now read:

"*Borehole experiments conducted immediately after drilling indicated that ice rests on bedrock with perhaps a thin veneer of sand and gravel approximately a decimeter thick (Harper et al., 2017). The subglacial drainage system was indicative of one overlying bedrock and governed by hard-bedded physics."*

Throughout the manuscript the present tense is used for things (e.g. data collection and processing, changes in temperature, findings of previous studies) that occurred in the past.

Thank you for highlighting this. We have made sure that our methods and findings from previous studies have been changed to past tense.

Line 17 - this statement should be framed as an argument rather than as a fact: for instance, "We argue that basal crevasses are a viable heat source . . .

This sentence now reads:

"We argue that basal crevasses are a viable englacial heat source in the basal ice of Greenland's ablation zone…"

Line 21 - suggest 'thermal regime' rather than 'thermal state'.

Changed.

Line 23 - something that is "current" cannot be "predicted".

We have changed *"predict"* to *"determine"*.

Line 25 and maybe Line 26 - consider omitting "englacial" as otherwise you are unnecessarily excluding basal refreezing/heat sources.

We have removed "englacial" from both line 25 and line 26.

Line 57 - state temperature sensor model number and briefly describe how the sensors were logged. Digital sensors often cannot be logged by most off-the-shelf data loggers.

We have added more information on the sensors and the data loggers. The data transfer protocol was developed in house for the instrument strings. If the reviewer is interested; the protocol is a modified and simplified i2c protocol, using modified RS-485 electrical cable protocol. The sentence on lines 61 – 66 now reads:

"Cat-5 cables instrumented with TMP102 temperature sensors with 12 bit analog-to-digital converters were installed in the boreholes after drilling and logged with a custom data logger at the surface. Each temperature sensor was controlled by a separate downhole microprocessor that ensured that the sensor was powered only as minimally required to obtain a reading. The paired microprocessor handled all digital communications, power control and data processing to minimize errors caused by sensor self-heating."

Line 60 - state what Clausius Clapeyron constant was used.

These sentences have been changed to read (lines 68 – 74):

"We performed a freezing-point calibration, using a Clausius – Clapeyron gradient of -7 x 10^{-8} °C Pa^{-1} (Cuffey and Paterson, 2010). However, due to the uncertainty in the Clausius – Clapeyron slope given dissolved air and impurities within the ice and the ambient pressure at the time of calibration, we are confident in the sensors' accuracy to approximately 0.5 °C."

Line 66 - your measurements suggest that the temperature measurements are not static, so you cannot measure a static vertical profile. Consider omitting "static".

Thank you for raising this issue, we understand your confusion. Because our temperature measurements do indicate that the temperature field changes with time, we want to be clear that we are showing the vertical temperature field at a given moment in time. Starting on line 79, this section reads:

Our data allowed us to examine both the vertical temperature field within the ice column as well as the temporal changes in temperature. The temperature of a hot water drilled borehole decays towards the ambient ice temperature over time. After 1 year < 0.1% difference remains between the borehole temperature and ambient ice temperatures. For ambient ice temperatures below -1 °C, this is well below sensor resolution and does not affect our observational data. Additionally, regressing equation 24 in Humphrey and Echelmeyer (1990) between 1 and 3 years after borehole closure results in a rate of residual cooling on the order of 10^{-11} °C yr^{-1}. To avoid any potential contamination of both the static and temporal trend of the sensor temperature data from the thermal disturbance of installation, we disregarded temperatures recorded in the first year after drilling in our analysis. To display a snapshot of the ambient vertical temperature field with minimal effects of the thermal disturbance from hot water drilling, we report temperatures collected one year after boreholes are drilled (Humphrey and Echlemeyer, 1990; (Supplementary Materials).

Line 74 - what is meant by "digital transmission errors"? In contrast to analog voltage measurements, digital data transmission is usually quite resistant to noise and interference and when it is affected it tends to fail completely. Does the digital transmission include

error detecting code such as cyclic redundancy checks? What does the manufacturer's data sheet say about digital transmission errors?

The errors are due to the nature of the low power, long cable, and data transfer protocol that was developed in house for these instrument strings. The protocol is not as fully robust as a full i2c protocol and has the potential for very occasional line conflicts lasting less than one bit count, The protocol has many advantages in terms of power saving and computational expense, reducing down-hole self-heating. The occasional glitch was a known and accepted problem. It is the simplified buss assertion part of the protocol that leads to the occasional one bit glitch.

Line 78 - add figure reference for "stepped behaviour".

Added *"(Fig. S1)."*

Line 84 - to remind the reader state here that the analysis began a year after installation.

Thanks for the suggestion. This sentence reads (lines 111 – 113):

"The vertical temperature field collected from the nine boreholes one year after temperature sensors were installed is shown in Fig. 2, with two boreholes only being instrumented with temperature sensors in the lowest half of the ice column."

Line 94 - frame this as an opinion ". . ., we argue that the trends are a real signal . . .".

This sentence reads (lines 125 – 126):

"While the observed temporal temperature changes are small, we believe the trends are a real signal and not an artifact of sensor drift or random noise."

Line 95 - while digital systems do exhibit noise and drift it is also worth remembering that in every digital sensor there is an underlying analog sensor being digitized.

The beginning of this sentence has been changed to:

"Analog-to-digital systems can exhibit…"

Line 96 - giving the sensor model number in the methods would allow the reader to check the claim regarding sensor drift. It would be good to quantify both expected and observed "drift" if reasonably possible.

Sensor drift is of course a worry. The manufacturer claims a typical drift of 0.03 °C over the life of the sensor. To check this we have run the sensors in the cold lab for many months, but unfortunately we are unable to measure the drift with any certainty, since the temperature changes are so small, and our absolute calibration equipment is only good to about 0.05 °C. Thus, all we can say is that drift is less than about 0.05 – 0.1 °C. Most drift in these transistor junction sensors is created by electrical currents. The manufacturer determines drift by running very hot (150 °C) sensors for months. In this project, the sensors are only run for a total of a few seconds over the life of the project, since individual measurements only take milli-seconds.

Line 97 - "packages" is the wrong word here. Please check definition. Also should be singular "cool".

We have changed *"packages"* to *"sections"* and changed the verb to *cool*.

Line 115 - what is the basis for smoothing with a 5-degree polynomial? Would another type of filter be more appropriate.

While there are many ways to smooth these profiles, we choose a 5^{th} degree polynomial because it is a simple way to smooth the shape of the profile while still maintaining the observed negative concavity of the profile near the bed. We have added this sentence (lines 150 – 152):

"We choose a 5^{th} degree polynomial because it smooths over sharp kinks within the profile from calibration errors, while maintaining the original curvature, particularly the negative concavity observed near the bed."

Line 152 - the statement that basal ice at the field site is temperate needs a figure reference and possibly also a citation as it's not immediately clear from the data presented. The sentence that some melting can occur due to pressure changes should also have a citation.

We have updated Figure 2 to show an inset with basal ice temperatures and the range of CC-slopes to show that ice reaches the pressure-melting temperature near the bed. We will reference Figure 2 here.

Line 168 - delete extra "to".

Deleted. Thank you for catching this typographic error.

Line 193 - something cannot be "removed" unless it was there to start with. In any case, there will be some strain heating unless deformation is zero. Suggest "negligible" or "low".

Thanks for this point. We have changed *"removed"* to "*negligible"*.

Line 227 - add reference for observations of basal crevasses here.

We have added a reference to *(Harper et al., 2010)* at the end of this sentence.

Line 230 - add reference for Stefan boundary condition.

We have added the reference to *(Carslaw and Jaeger, 1959)* at the end of this sentence.

Line 264 - insert "at distances" before "over 100 m . . ."

We have incorporated this edit into the sentence.

Line 265 - add e.g. before citation list. There are more examples of water pressure near overburden than this (e.g. van de Wal et al., 2015; Doyle et al., 2018).

Thank you for pointing these out. We have added the two references to this list of examples and included *e.g.* at the beginning.

Added reference:

"van de Wal, R. S. W., Smeets, C. J. P. P., Boot, W., Stoffelen, M., van Kampen, R., Doyle, S. H., Wilhelms, F., van den Broeke, M. R., Reijmer, C. H., Oerlemans, J. and Hubbard, A.: Self-regulation of ice flow varies across the ablation area in south-west Greenland, The Cryosphere, 9(2), 603–611, doi:https://doi.org/10.5194/tc-9-603-2015, 2015."

Line 293 - Would it be more appropriate to say that "Ice viscosity is highly temperature dependent" when referring to observations or physical conditions, rather than referring to flow law parameters?

We agree with this recommendation and have changed the sentence to read:

"The lack of a thick basal temperate layer indicates the transient thermal influence of basal crevasses. Ice viscosity is highly temperature dependent, thus basal crevasses could create regions of enhanced deformation, although previous observations in Greenland have shown that deformation rates in temperate ice are often lower than theory predicts (e.g. Ryser et al., 2014; Doyle et al., 2018; Maier et al., 2019)."

Line 302 - specify ". . . water flow paths".

We have made this change.

Line 304 - specify that the cooling was observed after 1 year and consider making it clear that you are distinguishing this from the cooling that is always observed after installation.

Our first sentence of the conclusion now reads (lines 388 – 389):

"Temperature measurements that began one year after sensor installation in 9 boreholes and span 2-3 years show that ice cools in the lowest portion of the ice column…"

Line 415 - state period of observation for Fig. 3.

The caption now reads:

"Figure 3: Rates of temperature changes for each borehole plotted against each sensor's height above the bed. Boreholes drilled in 2014 show trends between July 2015 and July 2017, while

temperature trends in boreholes drilled in 2015 represent temperature changes observed between July 2016 and July 2017. Error bars indicate 2 standard errors from the mean rate, determined by the linear regression."

Fig. 1 - consider marking the crevasse zone inferred to be the source of the thermal anomaly on the inset or main figure.

Because we do not know the extent, arrangement, or sizes of potential crevasses, we will refrain from estimating a location of where this crevassing took place. We showed that the time elapsed after a single crevasse finishes refreezing is ~ 10 – 30 years, and that interestingly coincides with a change in slope up-glacier. However, due to the likely complexity of the basal hydrologic system, we do not feel we have enough confidence to determine the location of crevassing precisely enough to place it on our map.

Fig. 2 - plot the pressure dependent melting temperature. Consider plotting an expansion of the basal temperatures as an inset. Note the time of measurement in the caption.

We have added an inset for ice temperatures in the lowest 20 m of the ice column and have added the range of pressure melting temperatures given the different CC gradients in Cuffey and Paterson (2010).

Fig. 6 - specify in the caption that these are model results.

We have made this specification.