Response to Referee Comments by Samuel Doyle

Thank you very much for your comments and suggestions. 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 line 57:

"Cat-5 cables instrumented with TMP102 temperature sensors that have 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."

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 line 65 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^{-11} °C. 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 line 80 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:

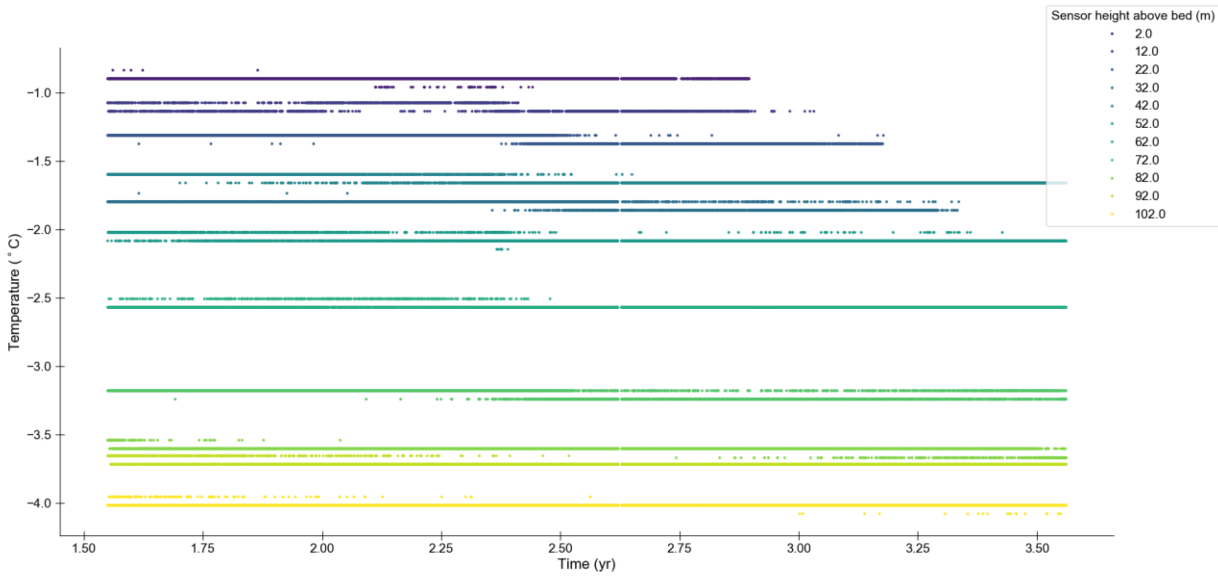
"Often the first and last digital steps of this record have fewer entries recorded due to the restrictions of the sampling window. Assuming that temperature change is close to linear with time, we expect that with a longer record, the sensor would record approximately the same number of readings at each 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 line 60 - 61:

"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' absolute 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 $\pm$ st. error). We will add this information to the supplement.

On line 41 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 $\pm$ 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 from line 126-145 are now:

"In addition, we also highlight regions of the static 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 deviations from this averaged profile within each collected temperature profile will highlight thermal regions that reflect changes to either the local boundary conditions or heat sources or sinks. These changes must have occurred recently for 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 where temperatures in these boreholes are higher than those in the site-average profile. Encouragingly for this analysis, 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 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 paragraph in section 4.3 also now reads:

"Basal crevasses can have a thermal influence on discrete, but large regions of ice. Because these fractures are distinct features, the fact we do not see cooling 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 not be more complex than an en échelon arrangement of vertical fractures, for simple modeling, we focus on the thermal disturbance created by basal crevasses that allow the influx of a plan of water from the bed up into the cold ice. Accurately modeling the thermal disturbance…"

Minor comments & Technical corrections

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, the field site section now reads:

"*Borehole experiments conducted immediately after drilling indicate 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 is 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 line 57 now reads:

"Cat-5 cables instrumented with TMP102 temperature sensors that have 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:

"We perform a freezing-point calibration, using a Clausius – Clapeyron gradient of -7×10^{-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' absolute 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. We have changed the two sentences here to read:

"Our data allows us to examine both the vertical temperature field within the ice column as well as the temporal changes in temperature. To display a snapshot of the ambient vertical temperature field…"

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:

"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:

"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:

"We chose a 5th 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:

"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)."

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:

"*Temperature measurements collected in 9 boreholes*

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 – July 2017, while temperature trends in boreholes drilled in 2015 represent temperature changes observed between July 2016 – 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.