The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-206-RC2, 2020

The Cryosphere Discussions
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EGU

Interactive comment on "The cooling signature of basal crevasses in a hard-bedded region of the Greenland Ice Sheet" *by* Ian E. McDowell et al.

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Received and published: 7 September 2020

McDowell et al. present results from borehole temperature strings that show a pattern of cooling in the lowermost third of the ice column. The measurements are 2-3 years long, which is longer than any other similar dataset from Greenland that I am aware of. To avoid the thermal disturbance caused by drilling with pressurised hot water, they focus their analysis on their dataset after a year has passed since installation. The authors argue that the cooling they observe can be best explained by the latent heat released by meltwater refreezing in basal crevasses. Basal cooling and the thermal effect of basal crevasses have had little consideration in the literature to date and this paper will make a strong contribution to the field. The paper is well written and the figures are well presented.

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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.

(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. 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.

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.

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 approrpiately in a supplement?

(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

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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.

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. 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.

(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.

Minor comments & Technical corrections

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.

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.

L17 - 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 . . .”

L21 - suggest ‘thermal regime’ rather than ‘thermal state’.

L23 - something that is “current” cannot be “predicted”

L25 and maybe L26 - consider omitting “englacial” as otherwise you are unnecessarily excluding basal refreezing/heat sources.

L57 - 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.

L60 - state what Clausius Clapeyron constant was used.

L66 - your measurements suggest that the temperature measurements are not static, so you cannot measure a static vertical profile. Consider omitting “static”.

L74 - 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?

L78 - add figure reference for “stepped behaviour”

L84 - to remind the reader state here that the analysis began a year after installation

L94 - frame this as an opinion “. . ., we argue that the trends are a real signal . . .”.

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L95 - 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 digitised.

L96 - 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.

L97 - "packages" is the wrong word here. Please check definition. Also should be singular "cool".

L115 - what is the basis for smoothing with a 5-degree polynomial? Would another type of filter be more appropriate.

L152 - 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.

L168 - delete extra "to"

L193 - 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".

L227 - add reference for observations of basal crevasses here.

L230 - add reference for Stefan boundary condition.

L264 - insert "at distances" before "over 100 m . . ."

L265 - 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).

L293 - 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?

L302 - specify "... water flow paths".

L304 - 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.

L415 - state period of observation for Fig. 3.

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

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.

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

References:

Doyle, S.H. et al. 2018. Physical Conditions of Fast Glacier Flow: 1. Measurements From Boreholes Drilled to the Bed of Store Glacier, West Greenland, Journal of Geophysical Research: Earth Surface, 123, 324-348

van de Wal, R.S.W. et al. 2015. Self-regulation of ice flow varies across the ablation area in south-west Greenland, The Cryosphere, 9, 603-611

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