The Cryosphere Discussions

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Comment on tc-2021-37

Anonymous Referee #3

Referee comment on "Indication of high basal melting at the EastGRIP drill site on the Northeast Greenland Ice Stream" by Ole Zeising and Angelika Humbert, The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-37-RC3, 2021

Zeising et al. use phase-sensitive radar observations over two seasons to investigate basal melt rates at the East GRIP (EGRIP) ice core site, located within the northeast Greenland ice stream (NEGIS). The authors directly measure strain rates in the upper ~½ of the ice column and use multiple strain-rate models to infer deformation in the lower ~½ of the ice column. They find that their observations and modelling require 16–22 centimeters per year of basal melt at their study site. This observation is supported by previous work, and the authors' analysis provides a detailed, and generally plausible, explanation for the mechanisms that could be responsible for these high basal melt rates (BMR).

In general the paper is well-written, the arguments logically supported and the conclusions well-founded in the observations. I applaud the authors for an interesting manuscript that advances our understanding of subglacial processes in a region that is important for the overall mass balance of the ice sheet. It is well-suited for publication in the Cryosphere. However, I would like to see the authors address the following major questions and concerns prior to publication:

- The change in reconstructed basal melting from one year to the next seems to result mostly from differences in ΔH (measured) and ΔHf (the offset from the linear fit to the phase-sensitive radar data). As mentioned in the text, whether ΔH is larger in '17/'18 than '18/'19 depends on the method used (compare Table 1 with lines 132–134). Thus, whether or not the basal melt rate was higher in one year or another comes down to ΔHf, which to my understanding is the distance on the x-axis between the red dot at

z=0 and the dotted line (Figure 2). If the authors want to make the claim that the basal melt rates in these years were indeed different (i.e. lines 115-116) they should provide more information about how robust their determination of ΔHf and in particular how the red dot at z=0 is defined and what the error on that measurement is, so the reader can be convinced that this difference is truly a robust indication that the system is somehow changing, principally due to firn densification, from one year to the next. Alternatively I think the results are equally robust and interesting if you consider the differences in reconstructed basal melt rate as indicative of the error in the method and provide one estimate of mean BMR based on 2 years of data.

- How do the scenarios and assumptions about subglacial water flow relate to observation of a dilatant till layer beneath this site (Christianson et al. 2014)? I would like to see a discussion of this high-porosity, water-saturated till layer added to the discussion section. Wouldn't the presence of such a till layer promote more distributed subglacial flow, as opposed to the channelized flow assumed by the authors in for example lines 199-201? There are relatively few places on earth where we have the active-source seismic measurements of Christianson et al. 2014 now coupled with these phase-sensitive radar observations and I think the authors have a very unique opportunity here to describe the processes and characteristics of this subglacial system in greater detail than they have already. In particular, I would also like to see further discussion of the velocity of the subglacial water system. Without any information about the shape of the conduit it is not possible to constrain the volume of water that would be required to maintain this heat flux into the subglacial system at NEGIS. Because there is no seasonal input of surface water (e.g. moulins) upstream of this study site, the authors' hypothesis requires a year-round steady source of subglacial water to maintain these basal melting rates in steady-state. Where do the authors think that water would originate? I would like to see further discussion on this topic. The work of Karlsson and Dahl-Jensen (2015) may be interesting to engage with here as well, as their findings are highly relevant to this discussion.

- In general, I find the discussion of the subglacial hydrological system very interesting and informative. I think this discussion would be further supported by a schematic figure which depicts the major processes and end-members that the authors consider in their arguments (i.e. lines 175–176). Otherwise I find it somewhat difficult to visualize the system that the authors are describing, which would help with evaluating the assumptions that they make in setting up their calculations and the strengths and shortcomings of those assumptions for describing the NEGIS system (see point 2).

Additionally, I suggest the following minor edits and more specific questions:

Line 1 "associate" change to associated

Line 24 “Franke et al.” is missing a year

Line 27 Keisling et al. (2014) inferred spatially variable basal melt rates of 0.05–0.2 m a-1 for the same region from ground-based radar observations.

Line 30 Suggested phrasing: In order to directly observe, among other things, flow regimes and basal conditions…

Line 79 “wide” is confusing here, I think the sentence functions equally well as “...we divided the depth profile into 6m segments with a 3 m overlap…”

Line 85-86 Why discard these segments? Is there any pattern in depth to which segments are discarded? What proportion of the data were discarded for this reason?

Line 97 - I am not convinced by the argument that *h* (i.e. kink height) in the Dansgaard-Johnsen strain rate model can simply be assumed to be the depth limit of the radar instrument. I would like to see either some citations to motivate the choice of this depth as realistic for the kink height in the DJ model or a consideration of how uncertainty in the kink height affects the final estimates of BMR.

Line 132-133 should read “time-consecutive measurements”

Line 195 please provide citations following “...consistent with subglacial hydrological modelling,” preferably those that share similar characteristics with your study site, e.g. little seasonal input of meltwater from upstream.

Lines 210-211 - Can you provide a back-of-the-envelope calculation for the creep closure rate for the kind of environment you are considering? Consider point #2 above - why would this system favor a channelized subglacial water system as opposed to distributed water flow within an actively deforming porous till layer (i.e. Christianson et al. 2014)? Does the fact that the radar instrument was advecting along with the ice give you any information about the scale and extent of the subglacial channels you are hypothesizing, or are the subglacial channels just being advected along with the ice column?

Line 216 “high-precise” change to high-precision

Figure 1. Legend - To me the legend should go the other way, with bigger numbers toward the top of the colorbar and smaller numbers at the bottom. Consider flipping the legend.

Figure 2. What is the red dot at z=0, and how is it measured? In the caption, "which" change to "whose" or "... line), the gradient of which is the vertical…"

Figure 3. May be helpful to label the three panels a,b, and c. What are the three dots in the left-most panel and why do they not connect with the thin lines?

Code availability: sentence should end "on request."

Acknowledgements: "EGRIP" is used here instead of "EastGRIP" which is used in the title, main text and Figure 1. Should be the same everywhere.

References used in this review

Christianson, K. *et al.* Dilatant till facilitates ice-stream flow in northeast Greenland. *Earth and Planetary Science Letters* **401**, 57–69 (2014).

Karlsson, N. B. & Dahl-Jensen, D. Response of the large-scale subglacial drainage system of Northeast Greenland to surface elevation changes. *The Cryosphere* **9**, 1465–1479 (2015).

Keisling, B. A. *et al.* Basal conditions and ice dynamics inferred from radar-derived internal stratigraphy of the northeast Greenland ice stream. *Annals of Glaciology* **55**, 127–137 (2014).