Comment on tc-2021-179
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Referee comment on "Variability of Basal Meltwater Generation During Winter, Western Greenland Ice Sheet" by Joel Harper et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-179-RC1, 2021

General Comments

This is an interesting attempt to investigate, from in-situ measurements, the basal meltwater generation under the Greenland ice sheet during winter. While there is room for improvement, I think the manuscript can be published after taking into account the comments outlined below.

In Equation (1) the melt rate seems to be volumetric. This should be explicitly stated (units for all quantities). Also state that this is the conservation of energy in a small volume around the glacier bed.

The development in Equations (2) and (3) needs to be improved. Units for the quantities should be given (is M_tot a volume, ice equivalent or water equivalent, or mass?). As it is, it seems that volume (or melted mass) and area are confounded in (3), and there are tacid assumption on ice thickness. Also, it is not clear why all the subscripts are needed. A small sketch of the geometry would help clarify the situation. Figure 8a shows the envisioned geometry, but does not help in that respect since the quantities are labeled with words, without relation to the symbols used in the equations.

Also, the formulas (2) and (3) seem to be unrelated to the results. It seems that some kind of force balance is needed to properly calculate melt rates and cavity size, as larger cavities lead to larger stresses outside the cavities, and incresed melt rates there.

The whole process is also likely spatially heterogeneous due to longitudinal and lateral stress transfer (e.g. Ryser et al, 2014 a,b). This might somehow be implied in the equations, but should be made explicit.

We obviously provided inadequate information to the reader regarding this aspect of the paper (we apologize to reviewers for this tactical error). While we follow the published cavity analysis of Kamb (1987), it was not reasonable on our part to assume the reader would be familiar with details of that analysis and how we implement it. We have taken three steps to address this shortcoming of our manuscript:

1. We added text to make clear the dimensions of various terms (which do balance).
2. We added more text and equations describing Kamb's cavity model and our implementation is better described with equations and not just words.

3. We replaced figure 8a with a new figure to demonstrate cavity analysis approach, define of the various terms, and show an actual modeled cavity.

One further point to take into account: The melt will be generated not just at the bed (as assumed in Eq. 1), but also within the ice column, especially where vertical shear rates are high. I know that at the drill site this component is very small, but this might not be the case on upstream or downstream obstacles. As a reference quantity, to which these results could be compared to, it would be interesting to caluculate total dissipation of potential energy. This is proportional to u_avg * H * slope. This quantity would also give a good estimate on the percentage of heat generated at the bed with respect to total heat dissipated.

Indeed, strain heating in the ice column can be a large heat source – certainly in locations with high vertical shear and also in places with strong horizontal or lateral shear. However, the majority of the ice column is quite cold (i.e., -18C) and so the heat generated in such locations is used to warm the ice. What matters here is the strain heating in the fraction of the temperate layer with hydraulic connectively with the bed. The connectivity/storage can exist as grain-scale voids (i.e., connected three grain intersections) and/or macro-scale voids (i.e., basal crevasses or perhaps 'vugs' which are sometimes observed in emergent ice in ablation zones. Little is known about either of these englacial storage mechanisms. As we state in the paper, however, even if we consider then entire temperate layer, we have the problem that the layer is only a few meters thick and new storage space must for some reason open over time as meltwater is generated, and then seasonally drain to avoid build up.

We understand the question here because englacial water storage/connectivity remains a poorly understood aspect of glaciology. And, Figure 7a would seem to be repetitive with the suggested calculation. Thus we feel that if we were to attempt to expand the discussion of these issues beyond the paragraph we already have, we would divert the reader's attention sideways without making more progress.

Specific Comments

40 This was estimated to be an order of magnitude higher in Lüthi et al (2003).
We added this fact and reference.

46 What are storage sinks within the ice? Basal crevasses?
We added a specific example, "e.g., basal crevasses", but left the sentence structured as generic regarding storage. This is because many ice sheet models have a term for generic water storage in ice, often conceptualized as grain-scale storage, and is sometimes set at up to 10%!

Figure 1, Caption: are all used data sets indicated here? Ordering them according to data set would make this clearer (e.g. water pressure at 27N, 27S and 33; GPS at 33 and 46).
Caption reworded for clarity.

Figure 1: dark blue on dark blue (station 46) is hardly visible.

We changed the darkness of the station box to provide more contrast with the background.

113: The term "fitted" is better expressed as "instrumented with" for an international readership.
Appreciate this perspective – changed.

116: Why not just use pressure? Then it is absolutely clear what you are comparing.
Prior publications have presented borehole water pressure measurements in units of absolute pressure, effective pressure, meters of water equivalent (above bed or below surface), and percentage of overburden. Each unit has its pros and cons. Indeed absolute pressure does provide a direct comparison between sites at a uniform scale, but assessment of the numbers is challenged by the fact that the pressure is closely related to ice thickness which is highly variable between locations. Thus, we believe that here the most appropriate presentation of pressures concerning the focus of the paper is to scale the pressures overburden. Had we been testing sliding laws, effective pressure would be better). Further, this unit is consistent with our prior publications for this region such that the new pressures presented here may be more easily compared.

125: What are the units of \dot{M}?
We have added this specific information, and have expanded our overall presentation of this section.

142: You could add some qualifying statements that with longitudinal and lateral stress transfer could change local shear stress by a large percentage below/above average values. But on the km-scale this is likely to be averaged out. But then it is not clear, what the borehole deformation measurements mean.
We have added this wording to the next paragraph (starting at line 147 rather than 142).

150: also Ryser (2014a)
Reference added.

151: In light of the above comment, this need not be true, and internal deformation is likely exceptionally low at the drill site (as compared to e.g. Ryser, 2013). It would be interesting doing the same calculations with their results for comparison.
We have added the reference (Ryser, 2014 because we could not locate a Ryser, 2013), and words to qualify our statements.

156: there is a stray "we"
Fixed.

184: This seems unintuitive. Melt only happens over \lambda - L_s,
\lambda being the wave length, and there is no melt over a cavity as there is no friction. Then also Equation (3) is unclear. This is only true if there if the water in the cavity has unit thickness. M_tot is a volume, and C_a is an area, so units do not match.

This entire section has been redone to address our lack of clarity.

Table 1: It would be useful (and more robust) to show total displacement during the winter period. This displacement times frictional stress gives the total energy released at the base, which is readily transformed to total melt during that period.
We are reluctant to divert the reader's focus from the characteristics of the acceleration during winter, which is also an important aspect of the paper. However, we understand your point and so we have added text to results section concerning displacements.

240 more precisely: "the heat generated by sliding friction"
Changed as suggested.

Figure 4b: squares are barey visible, consider using a better color
We increased the line weight three fold. The squares are now much more visible.

267 (also 397): typeset the number properly, not in computer coding
Fixed.

292: how are these values calculated. Give complete formulas that allow the reader to repeat the calculations. Why is no reference made to Eqs (2) and (3), where this whole development should be carefully made.
We agree that our presentation was insufficient. To remedy this, we have expanded our description of the methodology as described above. This paragraph now includes call out to the appropriate equation, and edited wording which hopefully improves the clarity.

297: step length should be named "wave length" as some kind of periodic bed is assumed.
We agree with the logic behind this suggestion. However, since we apply the analysis of Kamb (1987), we believe it is best to follow the terminology used in that paper, which is step/rise, length/height, etc. To adopt different terminology could confuse a reader who wishes to cross reference our work with the Kamb paper. Further, we note that Zoet and Iverson (2016), who also apply the Kamb (1987) model to their own work, also follow the Kamb terminology so it seems that a precedence has been set.

Figure 7, caption: This is not "heat", but "heating rate", "heat production rate", or similar.
Fixed.

310: it seems that 2 cm of water are not hard to store locally in sediment-filled basins or subglacial lakes in depressions. As water pressure is very high everywhere, this is a possibility that should be considered.
We have expanded the last paragraph to accommodate this observation (and the related comment regarding line 325).

317: Here seems to be a confusion between water stored within the ice matrix (Brown) and water stored in discrete cracks, e.g. basal crevasses of some sort or other, sponge-like and large, void space within the compact ice.
Brown et al.'s (2017) radar methodology for determining liquid water content in the ice is incapable of distinguishing the scale of the water, except for very large and separated basal crevasses yielding isolated reflectors. Thus, our aim here is to focus the reader on the amount of water while remaining appropriately ambiguous about the characteristics of the storage locations.

325: There are large depressions (100s of meters across) that could easily accommodate large volumes of water.
We have expanded the last paragraph to accommodate this observation (and the related comment regarding line 310).

330: But these might be unrealistic. Looking at the proglacial terrain shows very irregular bed undulations with very large and deep valley, deep and steep valleys etc. This looks very different from Kamb, Iken etc theories, and also from all these model inputs. Such bedrock topographies can, by moderate changes of water pressure, lift the ice and easily accommodate very large volumes of water.
We think it is important to discuss our findings with regards to common assumptions of subglacial hydrology models. We do, however, offer several qualifications to our discussion that address our point above: total water storage and geometry of subglacial water cavities is a function of unknown bed roughness; rounded sinusoids would store less water than our idealized cavity network, making our results an upper limit; and, some water could be stored in larger depressions.

354: "whereas they are small or absent at other locations (Ryser, 2014a).
Evidence and reference added to text.

365: Or lower effective pressure in subglacial sediments leading to liquefaction. A constant pressure and diffuse within the sediments, and increase the vertical extent of "softer" sediments. Such a process would readily explain the observed acceleration.
Our figure 5 and the data in Ryser (2014a) do indicate a decrease in effective pressure, commensurate with the observed sliding acceleration. Agreed that sediment deformation could be a key aspect of winter flow; we state this in the next paragraph.

373: This strongly depends on the time scales of cavity formation and cavity closure. It would be instructive to calculate approximate values to understand their magnitude and their changes under changing pressures and sliding speeds over winter.
Yes, the Kamb (1987) analysis includes cavity opening from sliding and creep closure of cavities, and we have piped the accelerating sliding speed over winter into these calculations (Figure 8)

Figure 8: step length could also be named wave length of the bed
See justification for naming this 'step' above, under line '297'.