Comment on tc-2021-179
Samuel Doyle (Referee)

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-RC2, 2021

Review of Harper et al. "Variability of basal meltwater generation during winter, Western Greenland Ice Sheet.

This manuscript by Harper et al. brings together previously-published datasets with new data and analysis to investigate whether subglacial cavity expansion caused by basal melt can explain the often-observed – but never satisfactorily explained – winter-time acceleration of surface ice velocity in Greenland. It makes an original and important contribution to solving this problem. The hypothesis that subglacial cavities become hydraulically isolated during winter is tested and the authors suggest that some interconnections must remain as melt volumes exceed plausible cavity storage volumes. Important points are made regarding basal melt variability, particularly that the majority of total basal melt is generated in areas with low and invariable basal melt rates, and that this will result in relatively constant winter-time discharge fluxes at the terminus. It follows that substantial perturbations in basal melt must represent the sudden release of subglacially stored water (e.g. drainage of subglacial lakes). The manuscript is concise, well written, well-presented and the arguments are framed well within the inherent limitations of the dataset. I have a number of comments detailed below.

General Comments

▪ Further description of the GPS filtering and basal shear stress calculation are required to ensure reproducibility.
We have added details concerning the binning and smoothing of GPS data used to produce our velocity curves. And, we have added an equation and defined all of its terms to better describe the calculation of basal shear stress.

▪ Expansion on the application of the Kamb (1987) analysis for determining cavity size could be helpful, for example by giving some of the original equations to explain the basis of the analysis. At present the description is limited to just two equations (Eq. 2 and 3).
We obviously provided inadequate information to the reader regarding this aspect of the paper (we apologize to reviewers for this tactical mistake). 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.
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.

▪ Previous studies which reported and attempted to explain the winter-time acceleration in ice flow should be introduced in the Introduction – this would highlight to the reader the research gap that this investigation fills.
We have substantially expanded the opening paragraph of the introduction to address this comment. We provide references for the observation of winter speed up on both glaciers and Greenland ice sheets. Some of this text was moved from the discussion since it is more appropriate here.

▪ The Introduction (and discussion) could be expanded slightly to introduce current theory and modelling relevant to the behaviour of subglacial cavities during winter to set the scene for this study, and to show how this study contributes to our understanding.
We understand the rationale behind this comment and we deliberated on this issue for some time. In the end, we are hesitant to send the reader in too many directions in the introduction: topics that include basal ice melt, winter ice flow acceleration, and also subglacial hydraulics. Our paper does not present new advancements on understanding of subglacial hydraulics and the cavity analysis we perform is nothing new; it was developed by Kamb (1987) and subsequently applied by other workers (e.g., Zoet and Iverson, 2015). Thus, the discussion presents some implications with references of our findings, but does not delve deep into a full overview of subglacial hydraulics. The third paragraph of the introduction is devoted to our paper's contribution in this area -- an assessment of the potential sources and sinks for basal water during winter.

Specific Comments

Please find minor typographical corrections and suggestions in the marked up PDF attached.
→ We have addressed all of the copy editing remarks included in the supplemental PDF. We are very appreciative of these comments and the time invested in careful reading and commenting on the manuscript.

L1 – In the title "western" should be lowercase as there is no "Western Greenland Ice Sheet". Also, this paper does more than just measure basal melt variability and its main take home point relates to cavity dynamics and subglacial hydrology – the title could be revised to reflect this better.

We have made sure that "western" is lower case in the manuscript body (but the first letter of each word in the title is capitalized). We have edited the title to reflect the paper's broader scope and implications.

L15 - Can you mention in the abstract that you calculate cavity dimensions/dynamics and compare them to melt volumes to determine whether or not all basal meltwater can be accommodated by cavity growth. At present you just refer to "insights into subglacial hydrology": I think you can be more specific. You might also mention in the abstract or introduction that you specifically test the hypothesis that cavities remain hydraulically isolated in the winter.
We do already mention that we perform 'analysis of basal cavity dynamics'. Our issue with adding more is that the manuscript guidelines say that abstracts should be 150-250 words, and we are already at 249 words.

L36 – you should introduce here previous observations of, and explanations for, winter- time acceleration in ice velocity. You currently do this on L352-358 but it should come earlier as it is key to the data and analysis presented. The winter-time acceleration is evident in most measurements of ice velocity (e.g. Sole et al., 2013). Note that there are also detailed winter velocities from near your study site but 140 km inland that show winter acceleration presented in Doyle et al. (2014). Winter acceleration in ice velocity is also presented in Phillips et al. (2013; Fig. 4), which I believe is mis-interpreted therein as being caused by increased deformation due to heating of the ice due to warming ice temperatures. The first paragraph of the introduction could introduce the literature on this topic slightly better, which would set the reader up for the analysis to come.
We have made this change to the manuscript: we moved the text introducing the winter acceleration (L353-358) from the discussion to the introduction where it is more appropriate, and we expanded the wording.

With regards to Phillips et al. (2013), Fig. 4 supports our findings but we believe it presents a completely aliased representation of winter ice flow, since it has just four data points spanning three different years. Sole et al. (2013) focus on mean winter velocity, but do not discuss winter accelerations. Looking at their Fig. 2, their site 4 does indeed seem to demonstrate a steady increase in speed all winter like we observe, whereas all other sites show none. Thus, our Fig. 4 is used to examine the variability around the basin. The winter accelerations in van de Wal et al., (2015) are circled in a figure showing speeds, but never discussed in a caption or the text. These examples demonstrate the challenge of trying to summarize all that's been measured and presented about winter acceleration, which we must do briefly since our paper in mainly about the fate of basal melt. Thus, we think it is best to simply provide some general summary statements and example references as we have done. The community needs a full review paper about ice flow acceleration in winter because there are so many different scraps of observations scattered about.

L69 – delete "full". All years have winter gaps due to power outage.
one

L73 – what is meant by “bed framework”?
We have changed this to ‘composition of the bed-surface’

Figure 1 caption and elsewhere – consistency with “basal water pressure” and “borehole water pressure” would help the reader who doesn’t know these are assumed to be the same thing.
We have eliminated use of ‘borehole water pressure’ in favor of ‘basal water pressure’, with section 2.3 describing how basal water pressures were measured in boreholes.

L93 – The methods used to filter and differentiate GPS position data need to be detailed to allow the study to be reproduced.
We have added this and additional information about the GPS processing.

L124 – why not rearrange to get M on its own on the LHS?
The equation is expressed as the energy balance at the bed, and we have added wording to make this clear. We note that this a common way to express the relationship (e.g. Binschadler et al., 2011, J. Glac.; Paterson, Physics of Glaciers).

L126 – More detail on how the basal shear stress was calculated (e.g. the equation used) needs to be given.
We have added an equation and defined all of its terms to better describe this calculation.

L128 – there is avoidable repetition here with L142-144 and neither spell out why basal temperate ice presents a barrier to upwards heat conduction, which is due to the Clausius-Clapeyron gradient causing a reverse (and small) temperature gradient.
We have edited this text to avoid repetition.

L185 – add an example reference to support the statement that cavities are often assumed to become isolated during winter. In general, slightly expanding the discussion of the theoretical understanding and modelling treatment of cavities and basal melt in winter would boost the significance of this papers’ findings.
This section has been redone to provide more detail on the cavity analysis methods, with more explanation and additional equations. We provide the rational for the cavity analysis as a test of the end-member closed water budget. Also, see response to bullet four above.

Table 1 – “Site” needs to be taken out of brackets and put into a new column.
Done.

L199 – I’m not sure you mean “daily acceleration”, will “acceleration” suffice?
Yes, we have changed the text as suggested.

L209 – The seasons have strict definitions in meteorology, and it would be helpful to note early on that the terms “spring” and “winter” are being used more loosely than their normal strict definitions, that is to reflect the period at melt-onset and the period when the subglacial hydrology is in its “winter-mode”.

This information has been added to the second paragraph of the methods.

L213 – Move descriptions of water pressure results to the section on water pressure (or remove subsections altogether). It would be good to expand on the description of winter- time pressure variability due to its relevance for cavity opening.
We have moved this text as suggested, and edited the section on basal water pressure for clarity.

L241, L255, L263 – "sliding friction", "basal friction", and "bed friction" should be "frictional heat from sliding".
These have all been changed as suggested.

L269 – observations of inter-annual variability in winter-time acceleration in ice flow at 140 km inland are presented in Doyle et al. (2014, Fig. 2). Can these measurements help quantify what is meant by "little to no winter acceleration"?
These data would be a nice example to present to the reader concerning our point. However, the S10 site located about the ELA and therefore beyond our study domain.

L323 – With the uninformed reader in mind, what are sliding speeds high relative to? Perhaps cite your previous work on this.
We have added details to make this clearer.

L328 – Note that cavities accessed (or even created) via drilling may not be representative of the majority of natural cavities. Analysis of the glacial foreground or subglacial topography in West Greenland would provide a better picture on the basal roughness; such analysis has been done (e.g. Lindback, 2015).
Agreed that drilling can create an artificial condition. The paper we reference discusses this and argues that while no subglacial measurement can be considered undisturbed, at least we know that the borehole was advected 10s of meters from the original drill site. We offer this fact as essentially an anecdote – the one attempt we know of to measure the size of a subglacial cavity in Greenland. We do not hang our results or interpretation on this single, and perhaps flawed, observation. The Lindbäck paper discusses roughness at larger scale than individual cavities. However, in response to comments by reviewer 1, we have added text two paragraphs later to address the issue of basal roughness.

L367 – you could spell out for the reader even more clearly that previous explanations are not sufficient to explain all the observations.
This is a good point and we have added explanatory text suggested.

L374/375 – mention that the studies reporting sediment are also from discrete sites (two of the references are even for the same site). The nature of the bed over large areas remains unknown. Therefore the capacity of the bed to store water in troughs and sediment also remains uncertain.

We edited this section to clarify discrete measurements at specific sites, and that the nature of the bed remains unknown. We have added discussion of water storage in troughs and sediments in the section above on the advice of reviewer 1.