Authors point-to-point response on Referee Comment #3 to tc-2021-37

1. *General Comments*

#1

The change in reconstructed basal melting from one year to the next seems to result mostly from differences in ΔH (measured) and ΔH_f (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 ΔH_f, 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.

Many thanks for raising this point. The difference in basal melt rate from one year to the other is mainly caused by differences in the measured change of ice thickness ΔH. This change in ice thickness (the movement of the surface relative to the ice base) is shown by the red dot at z = 0 m in Fig. 2 and Appendix Fig. 2. As correctly mentioned by the reviewer, ΔH_f leads to slightly different numbers of ΔH. We are convinced that the estimation of ΔH is less robust than the estimation of ΔH_f since it is based on a displacement derived from only one segment.

However, we agree that stating the averaged basal melt rate with the uncertainties based on the differences between both years gives a more realistic representation of the uncertainty of the method itself. We will follow your suggestion and that of Reviewer 1 and update the stated melt rate to 0.19 ± 0.04 m/a instead of two separated melt rates.

#2

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 observations of combinations of Nye channels (incised into the sediment) and Röthlisberger channels (incised into the ice) existing, which demonstrates that a saturated till layer does not necessarily favor distributed flow or prevent channels of forming.

Indeed, the interaction between the porous till layer and the water layer is extremely interesting. There are case studies in which the flow of water in the porous medium and the water layer is simulated using the Navier-Stokes equation. In these types of studies that are coming with enormous computational costs, the porous medium is either approximated by a matrix of simple geometries (cubes, cylinders) or a CT-derived geometry is used. Figure 11 in Kutscher et al. (2019) is showing a situation which is likely very similar to the subglacial hydrological system with a wet till. Also Fig. 14 of the same publication shows nicely how strong the interaction between the flow in the channel and porous medium is in terms of velocity and pressure.

With respect to our choices of the values for the velocities in the water layer, we have no such direct simulation as Kutscher et al. (2019) for our system and no observation of the speed. Therefore, we have tried to take two end members, the speed in the ocean and of an open channel. If this is indeed capturing the maximum velocity well is unclear to date. It would be great if this could be measured when the EastGRIP is giving access to the bed.

#3

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.

We would be more than happy to obtain a better constraint or more knowledge on the subglacial hydrological system with the ApRES, but at the end, an ApRES does only survey the ice body and the response of the ice body to forcing at the ice base, may it arise from friction of a saturated till layer, from a thick water sheet or a channel. Only the 'ice side' is accessible with the ApRES.

#4

In particular, I would also like to see further discussion of the velocity of the subglacial water system.

We fully understand the intention of the reviewer and are ourselves interested in the subglacial water velocity. Currently, the subglacial hydrological models applied to NEGIS/EastGRIP are using an effective porous medium (EPM) layer approach and although this computes the flux from which the velocity can be constrained, the velocity in the porous medium may differ from the real world situation to some extent. To solve this, simulations resolving the water layer are required, so no porous medium approach anymore, but Navier-Stokes type of simulation for this water system. If that velocity matches the EPM derived velocity well, then we (the community) would be able to get more into the velocity of the water layer and this allows then to constrain by far better than we do here the frictional heat. But this does not only go beyond the scope of this paper, it is also not easy to achieve. We are however, still somewhat optimistic that a direct measurement of the water velocity may be possible when the EastGRIP consortium drills into that system.

Although we have now a relatively dataset over almost two years, this is still located at one spot. Our plans for the next field season are to deploy as many ApRES as possible with some distance to the EastGRIP camp and also one outside the main ice stream, to estimate the spatial variability of basal melt. Indeed, it would be best suited to match these locations with the seismic lines of Christianson et al. 2014.

#5

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.

In our manuscript we present measurements of the basal melt rate and discuss which heat budget is needed to produce such melt rates.

It is correct that without the volume of the conduit, it is not possible to constrain the FLUX in the conduit and with that all contributions in the energy balance that contain the velocity of the water. In addition, the temperature field of the water is unknown and with that the heat flux arising from the water going into the ice q^sw in our notation.

Indeed, a year-round, but not necessarily steady, source of subglacial water is likely to exist, although we only present a point-measurement at EastGRIP. Simulated basal melt rates are showing the area that experiences melt and airborne radar observations are used to infer a wet base, indicating melt, too. Some of the water in the water catchment will feed into the system at EastGRIP, as simulations of Smith-Johnsen et al., 2020a and Beyer et al., 2018 showed. Simulated basal melt rates might be off in magnitude to some extent, still the area experiencing melt may be relatively well constrained. In particular the study of Smith-Johnsen et al. 2020a showed where and which amount of basal melt rates is needed to produce a basal water pressure that leads to the ice stream in its present form. Given that recent studies of basal sliding laws are showing the appropriateness of the applied sliding laws (Maier et al., 2021), the implications of Smith-Johnsen et al.'s study must not be underestimated.

To conclude: there is a wide water catchment that supplies the subglacial hydrological system year-round.

#6

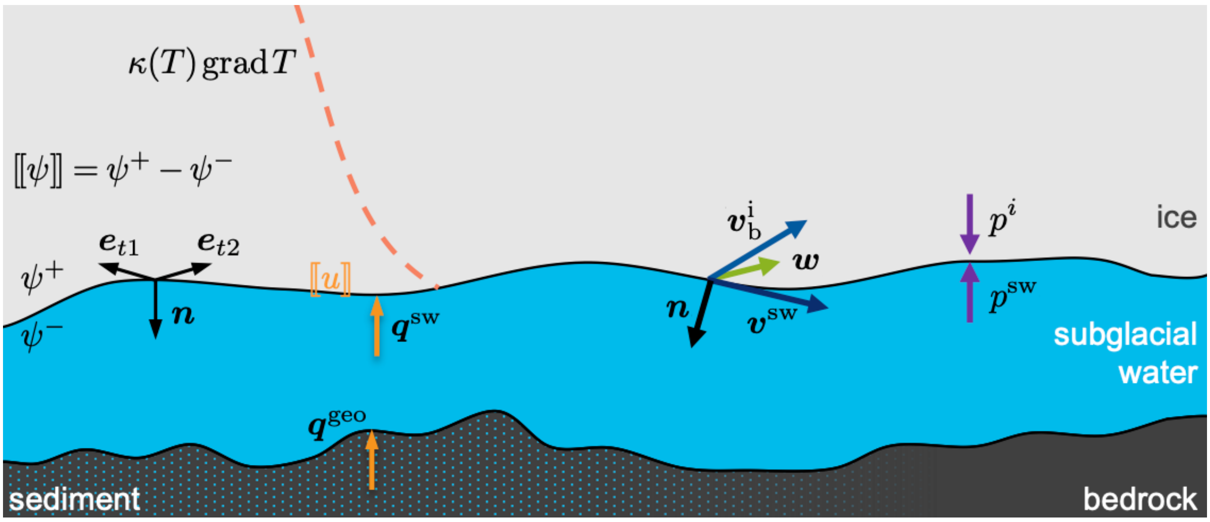
The work of Karlsson and Dahl-Jensen (2015) may be interesting to engage with here as well, as their findings are highly relevant to this discussion.

> Karlsson and Dahl-Jensen (2015) is only considering a routing scheme, which does not represent the hydrological system adequately. There are more approaches then EPM-type models (de Fleurian et al., 2014, Sommers et al., 2018, Beyer et al., 2018) that may also be well suited for the area around EastGRIP (e.g., GlaDS Werder et al. 2013, Hewitt 2011 type models), but an EPM model does represent both, efficient and inefficient drainage, hence sheet flow/distributed flow and channelised flow. Both types of models are, however, better suited for NEGIS from our perspective. Nevertheless, this goes beyond the scope of our manuscript, which only intends to present the measurement of basal melt rates.

#7

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).

> Many thanks for raising this point! We fully agree and are happy to support our energy balance consideration with a schematic figure, that appears now as Fig. 3.



2. Specific comments

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

- *Line 1 “associate” change to associated*

  Agreed

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

  Thanks for the hint. The manuscript by Franke et al. has now been published, which is why we are now able to complete the citation information.

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

  Many thanks for pointing us to this reference! We went through the manuscript and did not find the 0.2 m/a in the publication, but this statement for the central trunk. '*The average inferred basal-melt rate outside the ice stream is 0.05 m/a, which is significantly lower than the average basal-melt rate inside the ice stream (0.11 m/a) and in the line crossing the southeastern margin (0.09 m/a), but all are quite elevated*. Therefore, we will add the reference and keep the text with “*0.1 m/a and more*”.

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

  We will change the sentence as suggested.

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

  We will change the sentence as suggested.

- *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?*

  Yes, indeed we could have written this in more detail. We will enlarge this and give details of which and why segments are disregarded.

  Version 1, Line 85**:**

  > *“Segments whose time series contain outliers or whose shift deviates significantly from their neighboring segments were discarded.”*

Revised:

"To avoid influences of firn densification on the determination of ε^{obs}_{zz}, we excluded all segments above a depth of 250 m (~9 % of all segments). In addition, segments below the noise-level depth (depth at which the noise-level of the ApRES measurement prevents an unambiguous estimation) limit of h ≈ 1450 m were excluded (~45 % of all segments). Furthermore, outliers were filtered out (~7 %)."

- *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.*

Many thanks for raising this point. Indeed, there is no physical reason for assuming the kink to match the location of the depth limit. The only reason we have chosen this is that it would represent the upper limit. Our entire intention to discuss a DJ-type of profile was to help readers that are coming from the community applying DJ-models at other locations, like at drill locations on ice divides, into what would it mean to have made the DJ assumption in this particular case here.

As the way we used the DJ-model was obviously more confusing than helpful, we will follow the suggestion from Reviewer 2 and remove this part from the methods and the results. We will keep a few sentences in the discussion explaining that a DJ-type of strain would lead to larger values for a_b, although the assumption the Dansgaard-Johnsen distribution is based on is rather unrealistic for an ice stream.

Revised:

"A frequently used strain distribution (e.g., Fahnestock et al., 2001a; Keisling et al., 2014; MacGregor et al., 2016) that takes into account deviating strain within a shear zone is the Dansgaard–Johnsen distribution model (Dansgaard and Johnsen, 1969). As this model assumes a linearly decreasing strain in the shear zone that reaches zero at the ice base, the resulting basal melt rate at EastGRIP would be even larger. However, the Dansgaard–Johnsen model represents a no-slip boundary condition at the ice base. As this is an unrealistic assumption in an ice stream, we did not consider the Dansgaard–Johnsen model further."

(Please note, this point was also raised by Reviewer 2 and is therefore also in that point2point answer)

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

  We will change the sentence as suggested.

- *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.*

  We will include references, both simulate the NEGIS without any seasonal water input, thus they have similar characteristics than the system we discuss here.

- *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?*

  There is no way to infer the form of the channelised system, so width and thickness of the ‘void’ space and in fact, these are the critical quantities in doing such an estimation. A Master thesis (in German) was simulating closure rates for subglacial channels (T. Schultz, ‘Viskoelastische Modellierung der Dynamik eines Gletschers als Antwort auf basales Schmelzen und die Oberflächenmassenbilanz’, 2017 University of Bremen) taking a viscoelastic material model into account and conducting parameter sensitivity tests on width, thickness and water pressure. With a water pressure of 6 MPa the closure for a half-sphere-shaped channel to 5% of its original size takes in the order of 60 days. The water pressure has recently been simulated to be in the order of 20 MPa (Beyer et al., 2018, Smith-Johnsen et al., 2020a), which is by far larger.

- *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)?*

  It is yet to be determined by in-situ observation which system is underlying the ice stream. An actively deforming porous till layer may very well be part of this system, no doubts, but it won’t be sufficient to transport that large amount of water, as a porous till layer is rather inefficient in terms of water transport. The amount of deformation in the till will hopefully be measured in 2022/2023, when the EastGRIP drill progresses to the base and hopefully a Ploughmeter (and/or other instruments) will shade more light into this.

- *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?*

  Channels that are incised into the ice, such as Röthlisberger channels, would be advected with the ice, but undergo transformation by changing water input (basal melt), melt-opening, creep-opening/closure over time. The radar instruments only measure the change in ice thickness over time, but - unfortunately - no thickness of the water layer.

- *Line 216 "high-precise" change to high-precision*

  Agreed

- *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.*

  We agree to this point and will change the legend of Fig. 1 accordingly.

- *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..."*

  Many thanks for pointing out that the red dot is not well described. The big dot at z = 0 m is the derived change in ice thickness ΔH.

- *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?*

  Yes, indeed the panels are better referred to with a, b, c - we will change this. The three dots represent the ice overburden pressure p^i and as the ice thickness is well known this is only one value, therefore a dot. The lines are representing the water pressure assumptions. These information are added to the figure caption.

- *Code availability: sentence should end "on request."*

  Agreed

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

  We changed EGRIP to EastGRIP as suggested. Many thanks!

References used in this review

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