Authors point-to-point response on Referee Comment #1 to tc-2021-37

1. General Comments

#1

Regarding the Dansgaard-Johnsen distribution of the vertical strain I feel that the paper is not clear on how this distribution was used. At first it is presented as a comparison point (Line 91) before being quickly discarded on the ground that it does imply a no-slide basal condition. I agree with the fact that this distribution of vertical strain is unlikely to appear in this setting but then it should probably not appear in the discussion and the conclusion should state only "two different scenarios" rather than three (Line 219).

Many thanks for raising this point. We agree that the use of the Dansgaard-Johnsen model has not been consistently applied and clearly described. We intended to demonstrate to people working more frequently with the DJ-type of profiles how this model would affect the basal melt rate. We follow your advice and that of Reviewer 2 and will remove this part entirely from the methods as well as from 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."

#2

The thickness variations you present for each year have a fairly small errors but the two different years that are presented actually show quite a large spread in the value. Your further analyse on those numbers and the different values that are computed with an alternative method (Line 133) leave me with the impression that the differences that we see here are more related to imprecision in the measurement rather than the natural variability of the thinning. Regarding that point, wouldn't it be more fair to consider the difference between these measures as uncertainty of the method rather than two different thinning rates for different years.

We agree that stating the average 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 update the stated melt rate to 0.19 ± 0.04 m/a. Many Thanks!

#3

Finally, I find the last part of the paper regarding the considerations on energy balance very interesting but I am missing a final summary of this section. It would be nice to see all the heat sources summarised at the end with the range given by the different approximations. That would also shade more light on the possibility for NEGIS to have high melt rates with a reasonable geothermal heat flux.

> We agree that a kind of table would be nice, but the reason to avoid such a table is, that some terms are 'active' when others are 'inactive'. The friction of subglacial water may become the dominant term when there is a thick water layer underneath, whereas the ice-side friction term is suppressed then. In addition, the velocity in the subglacial water system is indeed poorly constrained. Our approach so far was rather to take the perspective if it is reasonable to expect a substantial contribution of a particular term.

2. Specific comments:

Below is a list of more specific comments throughout the manuscript given with line numbers:

- *Line 1: associated in place of "associate".*

  > Agreed

- *Line 3: "is largely unknown".*

  > Agreed

- *Line 4: Is "role" the proper term here, perhaps "relative importance" would fit better.*

  > We agree with this point and will change *"role"* to *"relative importance"*.

- *Line 8: I understand that the value given here represent the different years, but wouldn't it be better to have a value of 0.19 ± 0.04 m/a.*

  > We fully agree with this point and will update the melt rate to 0.19 ± 0.04 m/a.

- *Line 14: My opinion here might be biased but I think that the work of Smith- Johnsen et al. (2020a) shows that we can model the NEGIS without relying on inversion.*

  > This is correct, we were focusing more on the benchmark experiment here, which involved spin-ups and inversion and both based on different

approximations, but no subglacial hydrological models. We will add a sentence to the manuscript about subglacial hydrological models.

Version 1, Line 14**:**

> *"The distinctive extent of Greenland's largest ice stream – the Northeast Greenland Ice Stream (NEGIS, Fig. 1) – can only be represented well if an higher-order approximation is considered for the momentum balance and initial states are based on inversion (Goelzer et al., 2018)."*

Revised:

> *"The distinctive extent of Greenland's largest ice stream – the Northeast Greenland Ice Stream (NEGIS, Fig. 1) – can only be reproduced well if a higher-order approximation is considered for the momentum balance and initial states are based on inversion (Goelzer et al., 2018)* ***or involve subglacial hydrological models (Smith-Johnsen et al., 2020)****."*

- *Line 15: Shouldn't it be "inability" here rather than "ability"?*

  Yes, "inability" is correct. Thanks!

- *Line 20: "Increase in mass loss"?*

  Agreed

- *Line 22: I would say that one should aim at understanding the general dynamics and its different components rather than only lubrication.*

  Thanks for raising this point. We agree and will change the sentence.

  Version 1, Line 22**:**

  > *"Consequently, it is expected and projected that NEGIS will contribute significantly to sea-level rise in the future (Khan et al., 2014), highlighting the importance to* ***understand its lubrication.****"*

  Revised:

  > *"Consequently, it is expected and projected that NEGIS will contribute significantly to sea-level rise in the future (Khan et al., 2014), highlighting the importance to* ***understand the general ice flow dynamics and its driving mechanisms.****"*

- *Line 23: “enhances” rather than “enable”?*

  We decided to use both: enables and enhances.

  Version 1, Line 23:

  > *“One hypothesis for the genesis of NEGIS is locally increased basal melt rates at the onset area that **enable** basal sliding as basal melt water forms a subglacial hydrological system (Fahnestock et al., 2001a; Christianson et al., 2014; Franke et al.).”*

  Revised:

  > *“One hypothesis for the genesis of NEGIS is locally increased basal melting at the onset area that **enables and enhances** basal sliding (Fahnestock et al., 2001a; Christianson et al., 2014; Franke et al., 2021) and forms a subglacial hydrological system.”*

- *Line 24: In my opinion it is not the formation of the subglacial hydrological system that drive the sliding but more the increase in subglacial water pressure.*

  This is correct. We will rephrase it into two sentences, one referring to the publications that refer to the basal properties in the sense of detecting a wet base and discussing this to be a key player in formation of NEGIS, and a second sentence referring to the water pressure and publications that use/discuss this.

  Version 1, Line 23:

  > *“One hypothesis for the genesis of NEGIS is locally increased basal melt rates at the onset area that enable basal sliding as basal melt water forms a subglacial hydrological system (Fahnestock et al., 2001a; Christianson et al., 2014; Franke et al.).”*

  Revised:

  > “One hypothesis for the genesis of NEGIS is locally increased basal melting at the onset area that **enables and enhances** basal sliding (Fahnestock et al., 2001a; Christianson et al., 2014; Franke et al., 2021) and **forms a subglacial hydrological system. The coupling with basal sliding is facilitated via the water pressure, so that the sliding velocity rises with increasing water pressure (e.g., Beyer et al., 2018; Smith-Johnsen et al., 2020).**”

- *Line 25: I would remove “system” here.*

  Agreed

- *Line 35: It should be stated here that this number is also tied to other parameters of the model. Another interesting point to touch upon might be the sensitivity of the NEGIS system to Geothermal Heat Flux as presented in Smith-Johnsen et al. (2020b).*

  This is correct, the friction parameter also has a considerable impact on the dynamics of the ice stream.

  Revised:

  "By utilizing a coupled subglacial hydrology and ice sheet model, Smith-Johnsen et al. (2020b) demonstrated the large impact of an uncertainty in geothermal heat flux on the flow of NEGIS arising from the subglacial hydrological system, hence basal melting and water pressure, as well as from friction."

- *Line 73: A reference would be nice here to give a justification for those parameters.*

  We will add **Fujita et al. (2000)**. There was a typo in the propagation velocity, which is 168,914 km/s, not 168,194 km/s.

- *Line 87: The sentence starting on this line is hard to understand and could be rephrased.*

  We will rephrase the sentence. Thanks!

  Version 1, Line 87**:**

  "We found a linear fit u_z(z) to match the curve of the cumulative vertical displacements of the remaining segments within the ice (below a depth of 250 m to exclude layers affected by firn densification) [equation]."

  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 limit (depth at which the noise-level of the ApRES measurement prevents an unambiguous estimation) of h ≈ 1450 m were excluded (~45 % of all segments). Furthermore, outliers were filtered out (~7 %). We found a linear fit u_z(z) [equation] that best matches the cumulative vertical displacements of the remaining ~400 segments within the ice."

- *Line 97: “despite the fact that the no-slip boundary condition at the base is likely unrealistic...”*

  Agreed

- *Line 103: The notation for the vertical strain is not consistent throughout the manuscript, it is first introduced as ∆H_ε_zz but also appears as ∆H_ε_zz and ∆H_ε*

  Thanks for pointing this out. We will change all “∆*H_ε_zz*” to “∆*H_*ε”.

- *Line 104: It should be stated clearly here that the Dansgaard Johnsen approximation have been discarded at this point.*

  We comment on this in the general comment #1.

- *Line 111: ∆H^dj here should be ∆H^const_εzz*

  Agreed.

- *Line 112: From the text I am not sure here what the surface refers to. Is it the topographic surface or the base of the ice cave on which the radar is set-up. I expect this is the latter but that should be clarified.*

  Thanks for pointing this out. In this case *“surface”* meant position of the radar. We will change the sentence.

  Version 1, Line 112:

  > *“The firn densification – the intercept of the linear fit at the surface (see Fig. 2) – occurring below the radar is 0.074 m/a.”*

  Revised:

  > *“The firn densification – the intercept (see Fig. 2) of the linear fit at z = 0 m (the elevation of the ApRES) – occurring below the radar is 0.074 m/a.”*

- *Line 126: “as large as the one of...”*

  Agreed

- *Line 131: “Instead of comparing...”*

  Agreed

- *Line 139: “slightly” here seems like an understatement when the values you show are almost twice as large as the previous estimates.*

  We agree with this point and will remove *“slightly”*.

- *Line 141: “side” here should be “site”.*

  Corrected.

- *Line 172: Remove “are”.*

  Agreed.

- *Line 176: I would prefer “thick” here over “vertically extensive”.*

  Yes, this is a good idea.

- *Line 179: I wonder why the thermal conductivity for ice is taken at 273.15 K and not the pressure corrected melting point.*

  Many thanks for raising this point. We fully agree that the conductivity should be calculated with the pressure corrected melting point. We calculated the thermal conductivity with the pressure corrected melting point and willupdate the computed values based on this number.

  Version 1, Line 179:

  > “Next, we aim at constraining the individual terms for which we use the following material parameters: ρi = 910 kg/m^3, the latent heat of fusion, L = 335 kJ/kg, and the thermal conductivity for ice at **273.15 K κ(273.15 K) = 2.07 W/(m K)** (Greve and Blatter, 2009).”

  Revised:

  > “Next, we aim at constraining the individual terms for which we use the following material parameters: ρ^i = 910 kg/m^3, the latent heat of fusion, L = 335 kJ/kg, and the thermal conductivity for ice at **the pressure melting point of 270.81 K κ(270.81 K) = 2.10 W/(m K)** (Greve and Blatter, 2009).”

- *Line 184: I am puzzled by scenario (ii). Which heat flux is raised? It seems that the raise is lower than the final heat flux or is there an issue with units?*

  With scenario (ii), we want to state a number by how much the required heat flux from scenario (i) would be increased if the interior ice is not tempered. We agree that this could be written more clearly. We additionally corrected a mistake regarding the unit of the temperature gradient and will remove the lower limit of 10^-3 K/m.

  Version 1, Line 184:

  > (ii) Considering grad T to be between 10^−1 and 10^−3 m/a raises the required heat flux into the ice by 0.207 to 0.0207 W/m^2 .

  Revised:

  > (ii) Considering grad T to be **less than 10^−1 K/m**, **this increases the required heat flux from scenario (i) by up to 0.21 W/m^2, as this additional heat is required to warm the ice to the pressure melting point.**

- *Line 191: The description of the bounds for the basal velocity here could be clearer.*

  We will add the number of the surface velocity.

  Version 1, Line 191:

  > *"To constrain the sliding velocity we assume it to be maximum the surface velocity and minimum half of the surface velocity. "*

  Revised:

  > *"To constrain the sliding velocity, we assume it to be maximum the surface velocity **of 57 m/a** and minimum half of the surface velocity."*

- *Line 193: The value here is expressed in milliwatts when all other values up to this point or in Watts, it might make it easier to read if the units were consistent. This also applies to lines 196 and 202.*

  Thanks for raising this point. We will change all units to Watts.

- *Line 197: Consider rephrasing as follows: "from the roughness of the ice shelf base to a maximum roughness ten time larger..."*

  We will follow the suggestion and change the sentence.

- *Line 200: Consider rephrasing as follows: “We consider a speed similar to the one of the ocean...”*

  We will change the sentence as suggested.

- *Line 203: Remove “by”*

  Agreed.

- *Line 206: “we demonstrate” isn't the “we” missing?*

  We will rephrase this to ‘makes evident’ to avoid confusion.

- *Figure A1: It would be nice to state the lines' colours to their scenarios in the caption of this figure.*

  Agreed.

References

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