Authors point-to-point response on Referee Comment #2 to tc-2021-37

1. General Comments

#1

The authors refer several times to earlier studies by MacGregor et al. (2016) and by Fahnestock et al. (2001) which derived basal melt rates of 0.1 m/a or higher in the NEGIS. Both of these studies are based on radiostratigraphy methods using a 1D model of ice flow. While this type of model is valid for slow-moving areas, the local layer approximation (Waddington et al., 2007) in the NEGIS and its vicinity is not justified because the isochrone depths and shapes are considerably affected by ice flow dynamics. This is also emphasized by MacGregor et al. (2016), i.e. "...we restrict our interpretation of radiostratigraphy-inferred values of m , h, and Φ to the portion of the GrIS where we consider the local layer approximation to be acceptable for reflections younger than 9 ka, i.e., the region where depth–age relationships may be represented reasonably by 1-D models that neglect horizontal gradients in ice flow". I consider it important to point out the restricted validity of these previous results in the NEGIS system when referring to the above-mentioned studies.

We understand the concern of the reviewer and will incorporate this into our manuscript. Basically, our measurements indicate that they are off by 90 % and melting is higher than this type of study suggests. Taking the perspective that the basic message of the radiostratigraphy method is to detect where considerable melting is taking place, they may, however, still do a very good job. Our observation confirms a high melt rate where the radiostratigraphy method suggested high melt rates, too.

We want to comment that in future optimisation (e.g. inverse modelling) approaches may be able to constrain basal melt rates by optimising the mismatch between modelled and observed isochrones. The work that has been done by the radiostratigraphy community to develop the basis for such types of approaches has an enormous value. Its limitations today - and this is a very valid point of the reviewer - are not small, but this approach may in general to become the best strategy to survey ice sheet wide basal melt rates.

Version 1, Line 25**:**

> *"First estimates of basal melt rates by Fahnestock et al. (2001a) and later by MacGregor et al. (2016) are based on the interpretation of chronology in radiostratigraphy. Both studies found melt rates of 0.1 m/a and more – which is extremely large for inland ice."*

Revised:

> *"First estimates of basal melt rates by Fahnestock et al. (2001a) and later by* ***Keisling et al. (2014) and*** *MacGregor et al. (2016) are based on the interpretation of chronology in radiostratigraphy.* ***All three studies*** *found melt rates of 0.1 m/a and more – which is extremely large for inland ice.* ***However,***

these estimates may be prone to limited validity given the assumptions about the flow regime and constant accumulation rate."

#2

The authors also point towards the study of Smith-Johnsen et al. (2020a), stating that they found a geothermal heat flux of 0.97 W/m^2 to be necessary to reproduce the velocities of the NEGIS. While it is true that this result was obtained by Smith-Johnsen et al. (2020a) I think the context in which the reference is used here is misleading. Smith-Johnsen et al. (2020a) found such a high heat flux necessary to reproduce the NEGIS in their model with specific settings for basal parameters. However, they were also able to reproduce the ice stream with much lower basal heat flux in other studies (Smith-Johnsen et al., 2020b). From my point of view, the introduction gives the reader the impression that basal melt rates of 0.1 m/a and a geothermal heat flux of 0.97 W/m^2 are likely in the NEGIS as these numbers were suggested by several previous studies. This is problematic because the fact that a heat flux of this order of magnitude exceeds the mean continental background by far (e.g. Alley et al., 2019) is neglected and the low probability as well as the restrictions of these previous results remain undiscussed (see e.g. Bons et al., 2020).

Indeed, our intention is to inform the reader that a melt rate of 0.1 m/a has been estimated from previous observations and that a model also needed such a melt rate in order to simulate flow velocities similar to that of the NEGIS. However, a geothermal heat flux of 0.97 W/m^2 was required in the study from Smith-Johnsen et al. (2020a). Since Smith-Johnsen et al. (2020a) were aware that this amount of heat is too high to be explained by the geothermal heat flux alone, we will remove "geothermal" in the according sentence. Furthermore, we will add a sentence about the results presented in *Smith-Johnsen et al. (2020b).*

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."

We have a detailed discussion in our manuscript highlighting how other sources of heat contribute to melting at the ice sheet base, including a discussion of geothermal heat flux versus heat flux from the subglacial hydrological system. All subglacial hydrology models so far (that we are aware of) and incapable of simulating a thermal regime. This is important to keep in mind when going into such details of assessing the geothermal heat flux of such approaches. A coupled ice-sheet-hydrology system is currently coupled via the water pressure that affects sliding. The message from studies like Smith-Johnsens is that a considerable amount of basal melt is needed to produce a certain amount of water supply into the hydrological system to build up a water pressure in a magnitude and distribution leading to ice stream flow as observed. What would be needed to solve this is a subglacial hydrological model that resolves the water layer in the way that both, the velocity of the water, pressure AND temperature is computed. The lower boundary of that system would have a geothermal heat flux going into the system and provides a heat flux into the ice. This water system is unlikely laminar and turbulence may well play an important role. Now given that this needs to be done over a large area, this type of modelling is not around the corner. Due to a good reason why groundwater hydrologists are not running Navier-Stokes models on catchment scales. But even if we imagine due to enormous computing power in future to be feasible, we are still lacking the bed topography over that area. Radio echo sounding does 'only' give us the ice sheet base, not the bedrock topography. That would mean we would need seismics on the catchment scale (if seismics can resolve the thickness of the water layer adequately) or another method that does indeed survey the bedrock topography on catchment scale.

From our perspective, studies like Smith-Johnsen et al (2020a) should not be overly stressed on the geothermal heat flux that was needed to produce the velocity field of NEGIS, it is more about what melt rates are required to obtain a water pressure that is needed to sustain sliding leading to the observed surface velocities. This approach has its limitations, that is correct, but from our perspective more focus in the discussion should be on the basal melt rates and water pressure needed to produce NEGIS reasonably well, than the geothermal heat flux and/or friction parameter needed to produce these melt rates.

Smith-Johnsen et al. (2020b) shows the sensitivity of NEGIS to geothermal heat flux, but they did not show the velocity field and they did not state at all that they are able to reproduce the ice stream with a lower basal heat flux. Therefore, it remains unclear to us what the reviewer is referring to.

Version 1, Line 32**:**

> *"Smith-Johnsen et al. (2020) forced an ice model with a locally increased* ***geothermal*** *heat fluxes below the EastGRIP drill site and found that a heat flux of 0.97 W/m^2 (corresponding to a basal melt**ing** rate of 0.1 m/a (Fahnestock et al., 2001a)) is necessary to reasonably reproduce the velocities of NEGIS."*

Revised:

> *"Smith-Johnsen et al. (2020) forced an ice model with a locally increased heat flux below the EastGRIP drill site and found that a heat flux of 0.97 W/m^2 (corresponding to a basal melt rate of 0.1 m/a (Fahnestock et al., 2001a)) is necessary to reasonably reproduce the velocities of NEGIS."*

#3

The basal melt rates in this study are derived from changes in the measured ice thickness, which is assumed to be a function of basal melt rate, vertical strain and firn densification. The authors thereby refer to similar studies by Nicholls et al. (2015), Vankova et al. (2020) and Stewart et al. (2019) which use ApRES measurements to infer basal melt rates of ice shelves. A major difference between the application of ApRES on ice shelves and ice sheets is that the measured ice thickness on ice sheets is affected by the surface and bed topography as a result of ice flow, while the same measurement remains independent on lateral topography on floating ice shelves. In slow-moving areas of ice sheets, e.g. at ice domes, or if the method is applied over a short period of time, the effect of topography on the measurements might well be negligible. However, given the high ice-flow velocities and the distinctive bed and surface topography in the NEGIS, I am concerned about the fact that the impact of surface and bed slopes on the measured ice thicknesses at the EastGRIP drill site are not taken into account.

Many thanks for this point. We are happy to pick up this discussion and go here into some depth, as we have had such discussions a couple of times with community members already and it might be good to clarify a few points.

With ApRES measurements, we do measure the basal slope, but relative to the instrument and not as an absolute slope of the base. The measurement of the basal melt rate with ApRES is based on the ice thickness evolution equation. This equation is derived by vertically integrating the continuity equation over the vertical from the base to the surface, and the kinematic boundary conditions for the ice surface and the ice base. Kinematic boundary conditions are describing the motion of singular surfaces, such as the ice-atmosphere and ice-bedrock/hydrological system.

The resulting equation reads as

$$\frac{\partial H}{\partial t} = -\mathrm{div}\,\boldsymbol{Q} + a_s - a_b$$

with Q the volume flux, a_s the surface mass balance and a_b the basal melt rate. It is worth noting that this equation is independent of the configuration. It is the same for an Eulerian and Lagrangian reference system, hence the same in reference or present configuration. This equation means that an ice thickness change over time consists of stretching or compression, thus strain, at this particular site, and the accumulation/ablation at the surface and base.

Now let us consider a subglacial undulation. The ice will move over this protrusion by ascending and descending, so the ice base is exhibiting a slope. To some extent this will be accompanied by a slope at the surface. In all cases - with or without slope - the ice thickness H is the distance between the elevation of the ice surface h_s and ice base h_b: H = h_s - h_b. While crossing a bedrock undulation, the ice is deforming and it is this deformation of the ice plus the accumulation and ablation at the upper and lower surface, resulting in an ice thickness change.

This change in ice thickness may or may not come with a slope in surface and basal topography, which depends on the particular situation. A similar situation is given if ice passed over an area of higher water pressure p_w, leading to an increase in sliding velocity. This too will lead to a volume flux.

Now, as the volume flux equals for incompressible material the integral of the vertical strain over the ice column, a measurement of the vertical strain over depth is equal to a measurement of the volume flux over the ice column. This is what the concept of ApRES measurements is based on. Airborne surveys can in principle take the same strategy and redo the survey at exactly the same location a couple of times and derive the vertical strain, too. But a single airborne or ground based survey is lacking information of the vertical strain and needs to make assumptions on the volume flux and those estimates are then requiring slopes. All that is circumvented by (A)pRES measurements by directly measuring the vertical strain. The only disadvantage one may face with an (A)pRES system is that due to the low power of the transmitted radar burst, the absorption might be too large to measure strain down to the base, which requires then assumptions on the strain profile in that missing part. This is discussed in the manuscript in detail.

We hope that this extensive answer is shading some more light into that and helps to resolve the concerns. In the manuscript, we will add a paragraph starting from the ice thickness evolution equation, discussing all components and introducing the vertical strain rate.

#4

The evaluation of different scenarios for the vertical strain distribution are important to understand the sensitivity of the results towards the underlying assumptions. But I find it confusing that three scenarios are introduced but the results are only presented for two of them, since the Dansgaard-Johnsen model is discarded. I suggest to either include the results of the Dandgaard-Johnsen distribution in the manuscript or leave it out completely.

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 will follow your advice and remove this part entirely from the methods as well as from the results. We 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."

#5

Furthermore, as the vertical strain in the lower part of the ice column is considered the major uncertainty, it would make sense to me to provide the average between the different scenarios as result and consider the deviation from the mean as uncertainties. The errors provided in this manuscript seem very low as they include only the uncertainties of the measurements and might be misleading, as the total uncertainty of the inferred basal melt rates is clearly larger.

> We fully agree with this point and will state the melt rate for each year as suggested as an average value: 0.210 ± 0.015 m/a (2017/18) and 0.167 ± 0.018 m/a (2018/19). Many thanks!

#6

The evaluation of possible sources to provide the energy for the obtained basal melt rates is very interesting and an important aspect of the paper. However, I think that some essential elements are missing in the discussion. The suggested melt rates are larger than the present-day observed accumulation rates which has a considerable effect on the mass balance of the ice sheet. If such high melt rates were to persist over an extended period, I would expect to see evidence, e.g. in erosion of deep internal reflectors observed in radargrams. Radar images recorded in the vicinity of the study area do not show an extensive drag-down of internal layers compared to the surrounding (e.g. Keisling et al., 2014). It follows that the melt rates of the suggested order of magnitude must either be very local or a recent development. I believe that a more thorough discussion of these scenarios and the implications of the obtained results would add to the impact of the paper.

> We divide our answer to this point in three parts: (1) surface elevation change/thinning, (2) drag down of internal layers and (3) erosion of deep internal reflectors.

1. The mass balance is not driven by surface mass balance (accumulation) and basal melt rate alone. The change in mass balance the reviewer refers to, is an ice thickness change. As shown for point #3, the ice thickness evolution equation contains a term arising from the volume flux. If basal melt is larger than surface accumulation, the volume flux may still lead to no thickness change. A brilliant example for this is the 79N Glacier, where the floating tongue has basal melt rates in the order of tenths of meters per year and surface mass balance is negative, but still for a long period of time in the satellite observation there was no ice thickness change - the volume flux was large enough to sustain mass loss on the upper and lower surface. By no means the comparison of surface mass balance to basal mass balance alone is sufficient to conclude on ice thickness or surface elevation change.
2. Drag-down of internal layers is not solely due to variation in basal melt rates. Differences in sliding speeds and its feedback on viscosity, is changing internal layering as well (Fig. 4 in Leysinger Vieli et al., 2007, Fig. 6 in Gudlaugsson et al., 2016). No draw-down of layers may indicate basal melt

rates of similar magnitude or small gradient over the distance of the radar profile and/or no enhanced local sliding.

3. It is indeed an interesting question if such high melt rates are a recent phenomenon, or a variability in melt rates on time scales of millenia, centuries, decades and of course also the spatial variability is a very important question! With respect to terminology: every non-material singular surface experiences erosion, thus melting at the base, as well as erosion by dry friction is erosion of a layer. However, we think the reviewer means with erosion a disappearance of a deep layer along flow, a gradual drag-down with eventual disappearance. The scenarios under which this would happen are: (i) increasing melt rate along flow downstream, (ii) increasing sliding speed downstream plus constant melt rate and even (iii) increasing sliding speed and decreasing melt rate downstream, (iv) destruction of layers by increasing deformation downstream, (v) turbulent mixing of the lower layers appearing downstream. Scenarios under which no (gradual) erosion of layers are taking place are (a) constant melt rate, (b) constant sliding speeds, (c) freeze-on of subglacial water downstream. Consequently, there is no unique implication neither from erosion nor no erosion of layer to the magnitude of melt rates, nor its gradient along flow.

   In addition, to our knowledge the lowermost part of the ice stream is not resolved well in the recent airborne campaigns to answer this. Maybe the new generation of radar that is currently developed will shade more light into that. Although it may be very difficult with a 'single' transect in time to distinguish between erosion and disappearance of a layer by destruction due to high shear strain at the base (the new generation of radar would also be most suitable to conduct a similar type of measurements as with the ApRES with a system with more power allowing to resolve layers further down and constraining the vertical strain further). So, we conclude, that there is neither evidence so far for NEGIS of erosion of layers nor no erosion of layers.

2. Specific comments

Perhaps change the title to 'Indication of high basal melting at the EastGRIP drill site in the Northeast Greenland Ice Stream'?

We appreciate your suggestion and are happy to take over "***the*** *EastGRIP drill site*" but prefer to stick with "***on*** the Northeast Greenland Ice Stream".

- *Line 1: change 'interior of the ice sheet' to 'interior of ice sheets' as it refers to ice streams in general*

  Agreed

- *Line 3: change ’are largely unknown’ to ’is largely unknown’ when referring to ’amount’*

  Agreed

- *Line 5: ’These findings’ instead of ’these finding’*

  Agreed

- *Line 14: ’can only be reproduced well by such models if’ instead of ’can only be represented well if’*

  We will change the sentence as suggested

- *Line 15: should it be ’inability’ instead of ’ability’?*

  Yes, “inability” is correct. Many thanks!

- *Line 20: perhaps ’.. has already led to ice flow acceleration and increased mass loss?’*

  We will change the sentence as suggested.

- *Line 22: I would rather say that the general ice flow dynamics and its driving mechanisms are important to understand and not only the bed 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: ’enable basal sliding due to a subglacial hydrological system’ ?*

  We will change this sentence this way, due to what is indeed stated in the cited papers and to address Reviewer 1’s comment.

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 25: just 'subglacial water' instead of 'subglacial water system'*

  Agreed

- *Line 28: Perhaps change this sentence to: 'The cause for such intensive melt was attributed to a high geothermal heat flux which possibly originates from the passage of Greenland over the Iceland hot spot'.*

  We agree and will change the sentence as suggested.

- *Line 33: heat flux instead of heat fluxes*.

  Agreed

- *Line 77: It is not clear what the noise-level depth limit h is until the reader looks at Fig. 2. In the results it is stated that the vertical displacement can be estimated to a depth of 1450 m (assuming to be equivalent to h). Perhaps you can already write that h = 1450 m here.*

  Thanks for raising this point. We agree with your suggestion and will add the depth.

  Revised:

  > *"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)."*

- *Line 59-79: I find the structure of this part a bit confusing. First you define ∆Hεzz , then you describe all three quantities ∆H, ∆Hf,∆ Hεzz followed by a description of how the individual quantities are estimated. I suggest moving line 64-66 to the beginning of the paragraph.*

  As described in general comment #3, we will renew the introduction of this chapter and introduce the ice thickness evolution equation.

- *Line 66: You could also say here to what depth the densification processes are limited.*

  We will follow your suggestion and add the depth to which densification significantly affects the vertical displacement.

  Revised:

  > *"Thus, ∆H is independent on the surface mass balance, a_s = 0 m/a, but influenced by firn densification **that significantly affects the vertical displacement in the upper ~100 m.**"*

- *Line 68: 'The vertical gradient of the vertical displacement is the vertical strain' seems a repetition of Eq.(4) and if so can be discarded.*

  We will follow your suggestion and remove the sentence.

- *Line 76: the word 'measurement' is used four times in this sentence*

  We agree that the structure of the sentence could be improved. We will change the structure of this section slightly and mention the contrast to the method used by Vanková et al. (2020) a few sentences later.

  Version 1, Line 74**:**

  > *"To derive vertical displacements of layers within the ice as well as for the basal return from the ApRES time series, we used a modification of the process described by Vanková et al. (2020). Both methods are based on estimated phase differences derived from cross-correlation of individual depth segments. **In contrast to Vanková et al. (2020), we compare the first measurement with each repeated measurement instead of pairwise time-consecutively measurements to reduce measurement errors**. Here, the ApRES time series is used to achieve a reliable estimation of the annual mean basal melt rate. First, we divided the depth profile into 6 m wide range segments with a 3 m overlap from a depth of 20 m below the antennas to 20 m above the ice base and a wider segment of 10 m (-9 to +1 m) around the basal return, characterized by a strong increase in amplitude. Each depth segment of the first measurement (t1) was*

cross-correlated with the same segment of each repeated measurements (ti).”

Revised:

“To derive vertical displacements of layers and of the basal return from the ApRES time series, we modified the processing of Vanková et al. (2020) ***to reduce measurement errors (details below)****. Both methods are based on phase differences estimated from cross-correlation of individual depth segments. Firstly, we divided the depth profile into 6 m segments with a 3 m overlap from a depth of 20 m below the antennas to 20 m above the ice base and a wider segment of 10 m (-9 to +1 m) around the basal return, characterized by a strong increase in amplitude.* ***In order to derive vertical displacements****, each depth segment of the first measurement (t_1) was cross-correlated with the same segment of each repeated measurement (t_i).* ***This is in contrast to Vanková et al. (2020), who derived displacements from pairwise time-consecutive measurements (t_(i-1) – t_i). “***

- *Line 77: should it not be ’time-consecutive measurement’? Also in line 131*

  We will change this in both lines as suggested.

- *Line 82: measurement instead of measurements*

  Agreed

- *Line 91: ’scenarios to estimate $\Delta H\varepsilon zz$ ’ instead of ’scenarios in order estimate a range $\Delta H\varepsilon zz$’*

  We will change the sentence as suggested.

- *Line 99: I’m confused by the way this is written. What is the reason behind assuming the shear flow onset being at the noise level? And why is overestimating the basal melt rate desirable?*

  Our intention was to look at an extreme case: the shear flow would start at the point where we have no observations. If it would start further below, the effect on the basal melt rate would be lower. This is why we had chosen the ‘end’ of our observations as an onset of shear flow. The reviewer is entirely right in saying that this appears somewhat confusing. We will follow the reviewers general comment #4 and remove this part from the methods and the results.

  (Please note, this point was also raised by Reviewer 3 and is therefore also in that point2point answer)

- *Line 101: It is not clear to me what is averaged here. If the measurement period extends only over 2 years I'd expect to get two mean annual values. Where do the 65 records come from?*

  Yes, we agree, this was formulated rather confusingly.

  Version 1, Line 101**:**

  > *"In order to be less dependent on a single measurement, we averaged the annual mean values of ε_obs, ∆H_ε_zz, ∆H_f and ∆H from the last 65 records (roughly 25% of the measurements)."*

  Revised:

  > *"In order to be less dependent on a single measurement, we compute for each of the last 65 days (records; roughly 25% of the measurements) of a year an annual melt rate and compute from these 65 melt rate estimates a mean annual value by averaging."*

- *Line 106: Here you could refer to Fig. 2*

  Agreed

- *Line 152: ρ and ρi seem to be undefined.*

  Indeed! Both are the ice density and this has been corrected now. Many thanks!

- *Line 153: Different notation in equation and the text e.g.* ***q*** *vs q and ω vs* ***w***

  We will correct this. Thanks!

- Line 157: undefined term v^sw and t^sw . 'sw' is used here as superscript while used as subscript in q_sw

  Thanks for pointing this out! We will correct this throughout the section.

3. Technical comments

- *Section 2.3: inconsistent tenses, e.g. 'Firstly, we divided the depth profile' (line 79) vs 'Next, we estimate the vertical strain' (line 86)*

  This is correct. We went through the entire text and hope that we found now all occasions of inconsistent tenses.

- *Figure 1: Is this red-green colormap suitable for readers with colourvision deficiencies?*

  We updated the colormap slightly and checked the new figure from someone with red-green color blindness.

- *Inconsistent notation of Hε and Hε*

  We will correct this.

- *Perhaps consistently use either 'melt rate' or 'melting rate' throughout the text*

  We will change "melting rate" to "melt rate".

- *I believe that Tab. should be spelled out as Table, whereas Equation (3) should be abbreviated as Eq.(3).*

  Thanks! We will change the spelling as suggested.

- *Figure 2: red and gray points are missing in the figure legend*

  We will add both to the legend.

- *Heat fluxes are sometimes stated as mW/m^2 and sometimes as W/m^2*

  Thanks for pointing this out! We changed all units to W/m^2.

References

Alley, R. B., Pollard, D., Parizek, B. R., Anandakrishnan, S., Pourpoint, M., Stevens, N. T., MacGregor, J. A., Christianson, K., Muto, A., and Holschuh, N.: Possible Role for Tectonics in the Evolving Stability of the Greenland Ice Sheet, Journal of Geophysical Research: Earth Surface, 124, 97–115, https://doi.org/10.1029/2018JF004714, 2019.

Bons, P. D., de Riese, T., Franke, S., Llorens, M.-G., Sachau, T., Stoll, N., Weikusat, I., and Zhang, Y.: Comment on "Exceptionally high heat flux needed to sustain the Northeast Greenland Ice Stream" by S. Smith-Johnson et al., The Cryosphere, 14, 841–854, 2020, TheCryosphere Discussions, 2020, 1–5, https://doi.org/10.5194/tc-2020-339, 2020.

Christianson, K., Peters, L. E., Alley, R. B., Anandakrishnan, S., Jacobel, R. W., Riverman, K. L., Muto, A., and Keisling, B. A.: Dilatant till facilitates ice-stream flow in northeast

Greenland, Earth and Planetary Science Letters, 401, 57–69, https://doi.org/10.1016/j.epsl.2014.05.060, 2014.
Dansgaard, W. and Johnsen, S.: A flow model and a time scale for the ice core from Camp Century, Greenland, Journal of Glaciology, 8, 215–223, https://doi.org/10.3189/S0022143000031208, 1969.
Fahnestock, M. A., Abdalati, W., Joughin, I., Brozena, J., and Gogineni, P.: High geothermal heat flow, basal melt, and the origin of rapid iceflow in central Greenland, Science, 294, 2338–2342, https://doi.org/10.1126/science.1065370, 2001a.
Franke, S., Jansen, D., Beyer, S., Neckel, N., Binder, T., Paden, J., and Eisen, O.: Complex Basal Conditions and Their Influence on Ice Flow at the Onset of the Northeast Greenland Ice Stream, Journal of Geophysical Research: Earth Surface, 126, e2020JF005689, https://doi.org/10.1029/2020JF005689, 2021.
Gudlaugsson, E., Humbert, A., Kleiner, T., Kohler, J., and Andreassen, K.: The influence of a model subglacial lake on ice dynamics and internal layering, The Cryosphere, 10, 751–760, https://doi.org/10.5194/tc-10-751-2016, 2016.
Keisling, B. A., Christianson, K., Alley, R. B., Peters, L. E., Christian, J. E., Anandakrishnan, S., Riverman, K. L., Muto, A., and Jacobel, R. W.: Basal conditions and ice dynamics inferred from radar-derived internal stratigraphy of the northeast Greenland ice stream, Annals of Glaciology, 55, 127–137, https://doi.org/10.3189/2014AoG67A090, 2014.
Khan, S. A., Kjær, K. H., Bevis, M., Bamber, J. L., Wahr, J., Kjeldsen, K. K., Bjørk, A. A., Korsgaard, N. J., Stearns, L. A., Van Den Broeke, M. R., et al.: Sustained mass loss of the northeast Greenland ice sheet triggered by regional warming, Nature Climate Change, 4, 292–299, https://doi.org/10.1038/nclimate2161, 2014.
Leysinger Vieli, G.-M., Hindmarsh, R., and Siegert, M.: Three-dimensional flow influences on radar layer stratigraphy, Annals of Glaciology, 46, 22–28, https://doi.org/10.3189/172756407782871729, 2007.
MacGregor, J. A., Fahnestock, M. A., Catania, G. A., Aschwanden, A., Clow, G. D., Colgan, W. T., Gogineni, S. P., Morlighem, M., Nowicki, S. M., Paden, J. D., et al.: A synthesis of the basal thermal state of the Greenland Ice Sheet, Journal of Geophysical Research: Earth Surface, 121, 1328–1350, https://doi.org/10.1002/2015JF003803, 2016.
Nicholls, K. W., Corr, H. F., Stewart, C. L., Lok, L. B., Brennan, P. V., and Vaughan, D. G.: A ground-based radar for mea-suring vertical strain rates and time-varying basal melt rates in ice sheets and shelves, Journal of Glaciology, 61, 1079–1087, https://doi.org/10.3189/2015JoG15J073, 2015.
Smith-Johnsen, S., de Fleurian, B., Schlegel, N., Seroussi, H., and Nisancioglu, K.: Exceptionally high heat flux needed to sustain theNortheast Greenland Ice Stream, The Cryosphere, 14, 841–854, https://doi.org/10.5194/tc-14-841-2020, https://tc.copernicus.org/articles/14/841/2020, 2020a.
Smith-Johnsen, S., Schlegel, N.-J., de Fleurian, B., and Nisancioglu, K. H.: Sensitivity of the Northeast Greenland Ice Stream to GeothermalHeat, Journal of Geophysical Research: Earth Surface, 125, e2019JF005 252, https://doi.org/doi.org/10.1029/2019JF005252, 2020b.
Stewart, C. L., Christoffersen, P., Nicholls, K. W., Williams, M. J., and Dowdeswell, J. A.: Basal melting of Ross Ice Shelf from solar heat absorption in an ice-front polynya, Nature Geoscience, 12, 435–440, https://doi.org/10.1038/s41561-019-0356-0, 2019.

Vanková, I., Nicholls, K. W., Corr, H. F., Makinson, K., and Brennan, P. V.: Observations of tidal melt and vertical strain at the Filchner-RonneIce Shelf, Antarctica, Journal of Geophysical Research: Earth Surface, 125, e2019JF005 280, https://doi.org/10.1029/2019JF005280, 2020.

Waddington, E. D., Neumann, T. A., Koutnik, M. R., Marshall, H. P., and Morse, D. L.: Inference of accumulation-rate patterns from deep layers in glaciers and ice sheets. Journal of Glaciology, 53(183): 694–712, 2007.