Review of 'Indication of high basal melting at EastGRIP drill site on the Northeast Greenland Ice Stream'

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1 General comments

This study presents estimated basal melt rates at the EastGRIP drill site obtained by ApRES measurements of two subsequent years. The manuscript is well structured and the writing is of high quality. The results presented are relevant and contribute to the ongoing scientific discussion about the driving mechanisms of ice flow and the role of the basal properties in the Northeast Greenland Ice Stream (NEGIS). The authors introduce the ApRES method and explain how the basal melt rates can be derived from repeated measurements, followed by a description of the processing steps applied to the ApRES data. The results are discussed and put into context through comparison to other studies and by discussing the energy balance at the base.

I think that the presented study provides data and results which are highly relevant and contribute to the current discussion about the basal state at the EastGRIP drill site. The presented basal melt rates are surprisingly high and might indicate how much the basal conditions of the NEGIS differs from its surrounding ice. I have some general comments and questions as well as specific feedback which hopefully is valuable to improve the quality of the paper.

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 ma^{-1} 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 $\dot{m}$, 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. The authors also point towards the study of Smith-Johnsen et al. (2020a), stating that they found a geothermal heat flux of 0.97 Wm^{-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 ma^{-1} and a geothermal heat flux of 0.97 Wm^{-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).

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), Vaňková 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.

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. 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.

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 en 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.

2 Specific comments

- Perhaps change the title to 'Indication of high basal melting at *the* EastGRIP drill site *in* 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

- Line 3: change 'are largely unknown' to 'is largely unknown' when referring to 'amount'
- Line 5: 'These findings' instead of 'these finding'
- Line 14: 'can only be reproduced well by such models if' instead of 'can only be represented well if'
- Line 15: should it be 'inability' instead of 'ability'?
- Line 20: perhaps '.. has already led to *ice flow* acceleration and *increased* mass loss?'
- 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.
- Line 23: 'enable basal sliding *due to* a subglacial hydrological system' ?
- Line 25: just 'subglacial water' instead of 'subglacial water system'
- 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'.
- Line 33: heat flux instead of heat fluxes.
- 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.
- Line 59-79: I find the structure of this part a bit confusing. First you define $\Delta H_{\varepsilon_{zz}}$, then you describe all three quantities $\Delta H, \Delta H_f, \Delta H_{\varepsilon_{zz}}$ followed by a description of how the individual quantities are estimated. I suggest moving line 64-66 to the beginning of the paragraph.
- Line 66: You could also say here to what depth the densification processes are limited.
- 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.
- Line 76: the word 'measurement' is used four times in this sentence
- Line 77: should it not be 'time-consecutive measurement'? Also in line 131
- Line 82: measurement instead of measurements
- Line 91: 'scenarios to estimate $\Delta H_{\varepsilon_{zz}}$' instead of 'scenarios in order estimate a range $\Delta H_{\varepsilon_{zz}}$'
- 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?
- 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?
- Line 106: Here you could refer to Fig. 2
- Line 152: ρ and ρ_i seem to be undefined.

- Line 153: Different notation in equation and the text e.g. $\mathbf{q}$ vs q and ω vs $\mathbf{w}$
- Line 157: undefined term $\mathbf{v}^{SW}$ and $\mathbf{t}^{SW}$. 'SW' is used here as superscript while used as subscript in q_{SW}

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)
- Figure 1: Is this red-green colormap suitable for readers with colourvision deficiencies?
- Inconsistent notation of H_{ϵ} and H_{ε}
- Perhaps consistently use either 'melt rate' or 'melting rate' throughout the text
- I believe that Tab. should be spelled out as Table, whereas Equation (3) should be abbreviated as Eq.(3).
- Figure 2: red and gray points are missing in the figure legend
- Heat fluxes are sometimes stated as mWm^{-2} and sometimes as Wm^{-2}

References

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