

Review of: *Exceptionally High Geothermal Heat Flux Needed to Sustain the Northeast Greenland Ice Stream*

Submitted to: *The Cryosphere Discussions*

Reviewer: Nicholas Holschuh

General Comments:

This study highlights the role of subglacial hydrology in Northeast Greenland Ice Stream (NEGIS) dynamics. The authors show how both the total melt-water supply and melt-water pathways might explain the overall pattern of ice flow, and provide a compelling case for the use of models that capture dynamic basal hydrology instead of those that define a set of unchanging bed properties from an initial inversion. I believe this paper will make a valuable contribution to both our understanding of NEGIS models, and our projections of future NEGIS behavior.

However, I believe the text needs to more clearly state that this is (at its core) a model sensitivity study. The tone of the paper shifts between making broad physical interpretations (e.g., “Our findings ... confirm previous studies [which found a geothermal flux at NEGIS of 1 W/m^2]” -- line 289/90) and making narrower interpretations grounded in the model limitations (“the minimal geothermal heat flux value needed to initiate the ice stream **in our model** is...” -- line 293). I know the authors are aware that each of their statements has an implied caveat (that this is only a model) but it would be easy for the reader to misinterpret the results, and read the presented conclusions as unambiguous constraints on the physical system, given the language used. This includes the title, which I think overstates the conclusions of the paper and should be modified. I have highlighted statements that should be scaled back to more modest claims in the line item comments.

In addition, I am curious about the other output fields of the model. For any model that relies on a substantial basal melt anomaly, I think it is important to show the surface elevation field that is produced. If there is a measurable surface depression at the site of the plume according to the model, that would highlight an important source of disagreement between model and data, as there is no surface depression at the onset of NEGIS. It is likely that the radar methods of Fahnestock and MacGregor overestimate the actual basal melt rates at NEGIS – if similar melt rates applied in this model produce a surface profile much different than the real NEGIS, that must be presented. Regardless, it is impressive that the flow-speed pattern **can** be explained by large volumes of basal melt, but a fuller comparison of model and data will help the reader understand if it **does** explain the flow-speed pattern.

I leave the paper convinced that reproducing basal hydrology is important for reproducing NEGIS in ice sheet models, but the constraints on specific values for heat flux and the area spanned by the anomaly should be taken with a grain of salt.

Line-Item Corrections:

Line #: 10-11	This statement, in isolation, is too strong. It should include something like "Within our model experiment, a minimum heat flux value ... was required to reproduce observed NEGIS velocities."
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Line #: 22	"information that is needed"
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- Line #: 30-32 | One thing that we found in a modeling study of NEGIS we performed was that the shear margins are likely characterized by a complex velocity and viscosity structure. What did you do for your viscosity initialization in this model? Does it evolve with ice temperature? I am not trying to imply it needs to be cited here, but you may find some of the results from our study interesting and relevant: Holschuh, N., Lilien, D., & Christianson, K. (2019). Thermal Weakening, Convergent Flow, and Vertical Heat Transport in the Northeast Greenland Ice Stream Shear Margins. *Geophysical Research Letters*, 46, 8184–8193.
<https://doi.org/https://doi.org/10.1029/2019GL083436>
- Line #: 40 | Unless there is extraordinary need, you should not cite work in review. It makes it impossible for a reader to evaluate this statement, as it has not been vetted by the peer review process.
- Line #: 43-44 | Again, I would remove references to papers in review. Without more context, I cannot tell what this sentence means, and I cannot evaluate the claim. What do you mean by uncertainty in the ice flux, our observations of ice thickness and velocity near the grounding-line are quite good?
- Line #: 47 | This paragraph should include the statement that you make in line 221-224, making very clear to the reader you do not think a mantle plume is presently beneath NEGIS. You are simply using a plume model to generate feasible scenarios that can be tested with the model. Without the sentence at 221, It would be easy to walk away from this paper thinking you believe there is a mantle plume presently under NEGIS (which would require substantially more evidence to justify).
- Line #: 55 | How was the model changed from Schlegel to the in review paper? If you are including those modifications here, it is important that the reader know what they are, but they cannot be determined as the paper referenced is not published. This is a case where an in review citation may be acceptable, but you need to include the salient details from the paper in the text here.
- Line #: 58 | Could you provide justification for your choice in sliding law here?
- Line #: 87-88 | This statement does not agree with the seismic results collected at the onset of NEGIS, where there was no apparent relationship between topography and till strength. You should reference whether or not this argument is observationally substantiated. It would be helpful to include discussion here from Christianson et al: Christianson, K., Peters, L. E., Alley, R. B., Anandakrishnan, S., Jacobel, R. W., Riverman, K. L., ... Keisling, B. A. (2014). 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>

- Line #: 101 | The plumes discussed here are not very consistent with MacGregor et al 2016, who find large areas of basal melt ($> 100\text{km} \times 100\text{km}$) well upstream of NEGIS. I think the agreement between Fahnestock and MacGregor throughout the manuscript is generally overstated.
- Line #: 137 | Clarify what you mean here, Fahnestock and MacGregor did not have identical results.
- Line #: 163-164 | Here is an example of potentially misleading language -- you show the elevated heat required by your model to initiate NEGIS. Much less heat may be required if the bed were uniformly weaker, if you included fabric evolution or imposed viscosity transitions, if the water transmissivity at the bed were lower, etc. All of the values you provide are contingent on the physical processes included in the model, the assumptions about the flow law form and parameters, and the experimental design.
- Line #: 168 | "met" should be "melt"
- Line #: 173-174 | "This shows that plumes with a restricted extent, $\sim 50\text{km} \times 50\text{km}$, produce model results more consistent with the observed flow behavior in the upstream reaches of NEGIS." -- something that clarifies that this is not a necessary condition for NEGIS.
- Line #: 197-198 | Perhaps change this sentence to read "the geothermal heat flux needed to induce the observed upstream velocity of NEGIS in our model is ~ 970 , consistent with values presented in Fahnestock et al. (2001)." What you are stating here (and in your next sentence) is essentially "high melt water production rates are required to drive fast flow in the upstream region of NEGIS, assuming the absence of other variations in bed strength driven by substrate heterogeneity". I think that last caveat is important to make here and elsewhere in the paper; you are forcing all of the variation to be driven by hydrology, but it need not be the only property that varies in space.
- Line #: 211-212 | The comparison with Jarosch and Gudmundsson (2007) here seems odd, as they apply their geothermal flux anomaly over $\sim 500\text{m}$. No one would argue that their anomaly could exist at the scale of your plume. However, their results do highlight something that I think you should present to your reader -- substantial melt anomalies manifest in the ice sheet surface. I imagine the ice sheet surface in your models has a similar (albeit smaller) melt feature as the one in Jarosch and Gudmundsson. If so, somewhere in this work you should state that localized, substantial melt under NEGIS would be visible at the ice sheet surface, but is not apparent in altimetry data. Any discrepancy (or, if present, agreement) in the effect of basal melt on the ice surface profile must be discussed.

- Line #: 218-219 | This seems to imply that your results differ because you are fitting to velocities instead of temperatures, but that is not the primary factor. Greve has no constraints near the onset of NEGIS, while your study does. If the anomaly you argue for existed, Greve would have no way of knowing with the data he has available. Greve's data set is actually a much more direct measure of geothermal flux -- if he had broader observational coverage it would be hard to argue with his results.
- Line #: 221-223 | As stated earlier, this sentence should come much sooner in the paper. Without additional data, we have no means of explaining why there might be a heat flux anomaly at NEGIS, and it is not likely a modern plume.
- Line #: 227-228 | MacGregor et al. have abnormally high melt rates in several places in Greenland, including over a broad region upstream of NEGIS and in SW Greenland. This citation here seems inconsistent with the statement made.
- Line #: 273-277 | A broader discussion of the role of the friction law would be useful. What if you used a non-linear sliding law? What direction would that change your results? It would be useful for the reader to understand how the plume characteristics you describe would need to vary to reproduce NEGIS using a range of different model set-ups.
- Line #: 290 | "confirms previous studies" is too strong. "is consistent with" would be better.