Response to Reviewers

December 20, 2019

General response

Our thanks to Nicholas Holschuh and Signe Hillerup Larsen, their suggestions greatly improved the manuscript. We are grateful for their insights in both observations and modelling of NEGIS. The major changes to the manuscript are outlined here:

- We moderated our language in our statements and included an extended caveat section in the discussion. We also modified the abstract to underline that we present model results, and the conclusion now includes model caveats and uncertainties. With a more detailed abstract and conclusion including model assumptions, we avoid being too assertive, allowing the reader to think critically about the numbers presented. For these reasons, we choose to keep the title as is.
- Following your suggestions, we have provided more details on the comparisons to previous studies throughout the manuscript. In addition, we included a figure of the gridded melt dataset from Macgregor et al. (2016), interpolated onto our model mesh. This allows for an improved and more direct comparison. We also carefully restructured the section comparing our geothermal heat flux results to previous findings in the discussion, to clearly state how high the implied value presented here would be.
- We included a new equation on the viscosity
- Following recommendations from Irina Rogozhina during Smith-Johnsens PhD defence and discussion thereafter, we have decided to make some additional changes regarding terminology. We explain our high values (970 mW/m^2) by advective heat transport (hydrothermal circulation). The term “geothermal heat flux” is inaccurate, as it only comprises pure conductive heat transfer. We removed ’geothermal’ from the title, and expanded the abstract, discussion and conclusion to include this.

On behalf of the authors,
Silje Smith-Johnsen (PhD)

RC1: Nicholas Holschuh

Comment

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.

Response

We agree that this is an interesting point to investigate and we looked at the model surface of Ctrl and plume970 and compared them to observations (Scambos & Haran, 2002). We found that our Ctrl simulation underestimates the surface elevation over the model domain. To disentangle the surface lowering caused directly by introducing the plume, we investigated surface differences between the plume970 and the Ctrl simulation. We found that there is no evidence of a local surface depression above the plume. We do observe a regional surface lowering of the entire domain upstream of the plume, and a slight thickening downstream (and hence the surface becomes closer to the observed surface in the downstream area). The lack of a local surface signal from the plume in our model thus agrees with observations.

These findings are in line with previous idealized experiments we have conducted where we found that the plume melts the above lying ice creating a local depression, if no subglacial hydrology model is included. However if one includes a subglacial hydrology model like this study, the ice is allowed to slide in response to the extra water added at the base. The surface signal of the plume becomes more regionally dispersed due to both effective pressure changes and most importantly the advection of ice downstream distributes the surface signal. We included a statement of the surface results in L293-295.

Comment

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.

Response

Thanks, done

Comment

Line #: 22
"information that is needed"

Response
Thanks, done

Comment

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., and 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

Response
Yes, ice viscosity is temperature dependant, and evolves through time with changes in temperature. This is now included in equation 2 in the ice sheet model description, L73. For temperature we initialize with prescribed surface temperatures and the basal boundary condition, and we solve a thermal steady state. No climatological spin-up is used, and therefore the overall thermal state may be too warm. We also use the 3D Higher-Order approximation to compute vertical velocities, important in the shear margins. As stated by the reviewer Signe Hillerup Larsen, we have a rather coarse mesh in the shear margins and may thus underestimate the strain heating that is occurring here. We included a caveat section on how this may influence our results in the discussion (L276-279).

Comment

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.

Response
Thank you, we removed the Smith-Johnsen et al. A as this is still in review. We chose to keep the Smith-Johnsen et al. B, as this manuscript is now accepted (Smith-Johnsen, Schlegel, de Fleurian & Nisancioglu, accepted).

Comment

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?

Response
In this paper we show how uncertainties in model inputs (GHF) propagate through the ice flow model and cause a large range of modelled mass (ice) flux through NEGIS, and therefore large uncertainties. This is relevant for future predictions, as we do not have observations. We chose to keep the Smith-Johnsen et al. B, as this manuscript is now accepted (Smith-Johnsen et al., accepted).

Comment

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).

Response

Thank you this is a good point. We have edited the paragraph accordingly by including this statement earlier in the paper, L50, and removed it from the discussion.

Comment

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.

Response

Thank you for pointing this out to us. The most important change is that the thermomechanical ice flow model is coupled to a subglacial hydrology model. We changed the sentence to include this, and keep the reference to Smith-Johnsen et al. (accepted), as it is now accepted.

Comment

Line#: 58

Could you provide justification for your choice in sliding law here?

Response

This is the most commonly used sliding law in ISSM. It was used by (Schlegel, Larour, Seroussi, Morlighem & Box, 2015) and (Smith-Johnsen et al., accepted), so to avoid a complete new model set-up with following spin-up, we decided to keep it. Instead of justifying the choice of sliding law here, we included a discussion on the implications of using this in the new caveat section of the discussion (L269-275).

Comment

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

Response

Thank you for this reference. We do compare our friction coefficient distribution to roughness observations in L97-98. The friction coefficient includes everything unknown at the bed, in addition to till strength. We are aware of the limitations of using this simple approach for the friction coefficient, and we therefore mention these limitations in the new caveat section in the discussion.

Comment

Line#: 101

The plumes discussed here are not very consistent with MacGregor et al 2016, who find large areas of basal melt (> 100km x 100km) well upstream of NEGIS. I think the agreement between Fahnestock and MacGregor throughout the manuscript is generally overstated.

Response

We tried to explain that the plumes here compares well to the northeastern branch of the anomaly of Macgregor et al. (2016). We have made this clearer by removing the references to figures within their paper, and instead included a plot of the Macgregor et al. (2016) gridded dataset for our model domain in a new Figure 6. This improves the understanding of the reader, and allow for a more direct comparison of the basal melt rates from our 970plume experiment to the dataset.

Overall in the paper we have modified the comparison of the GHF and basal melt to previous studies, by providing more details for each comparison.

Comment

Line#: 137

Clarify what you mean here, Fahnestock and MacGregor did not have identical results.

Response

Thank you, we mean the maximum magnitude of geothermal heat flux (970 mW/m^2) proposed by Fahnestock, Abdalati, Joughin, Brozena and Gogineni (2001). We have clarified this, and we removed the Macgregor et al. (2016) reference.

Comment

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.

Response

Thank you, we toned down the statement by writing "indicate" in stead of "show", and we included "in our model". In addition, we have included a caveat section in the discussion where we provide several reasons for why we may overestimate the geothermal heat of the plume, due to model uncertainties and assumptions (friction law, shear margin softening, subglacial hydrology parameters).

However, if the bed were uniformly weaker the entire domain would speed up, resulting in an even lower surface, and increase the underestimation of ice thickness. In addition, the outlet glaciers are too fast in our model, and a uniformly weaker bed would intensify the problem. We agree that this is a very simple estimate of basal friction, and we also included this in the caveat section in the discussion.

Comment

Line#: 168
"met" should be "melt"

Response

Thanks, done

Comment

Line#: 173-174
"This shows that plumes with a restricted extent, 50km x 50km, 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.

Response

Thanks, done

Comment

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 regions 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.

Response

We agree, and changed the sentence to what you suggested. However, the next statement is not exactly true, as our friction coefficient is spatially varying and not uniform, and represents everything that varies at the base. We show the importance of spatially varying bed properties by running two simulations where we have a spatially uniform friction coefficient (simulation "Uni Ctrl" and "Uni 970") where the velocity pattern is less confined and less similar to observations.

We agree that it is important to state that we keep everything constant in our model and only vary GHF, and try to explain the observed velocities by subglacial hydrology, despite the many model assumptions and uncertainties. We stated this at the beginning of the new caveat section in the discussion, L267.

Comment

Line#: 211-212

The comparison with Jarosch and Gudmundsson (2007) here seems odd, as they apply their geothermal flux anomaly over 500m. 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 features 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.

Response

Thank you, we agree and have removed this reference. In addition, Iceland is generally not a representative comparison as it is located on a mantle plume and a spreading ridge, thus an extreme geothermal heat flux example. This is a very interesting topic. As mentioned earlier, by comparing the plume970 simulation to the Ctrl, to disentangle the direct impact of the plume, we do not see a significant local surface depression. We think this is due to the hydrology dispersing the signal, and most importantly the advection of ice redistributes and dampens the surface signal. However, we think the case would be different if the ice above the local plume was not sliding. It would be interesting to test our plume in an area where a local mantle plume would not trigger fast flow, only local melt, to see if the surface expression would look different.

Comment

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.

Response

This is true and a good point, and we removed this reasoning from the paper. We compare our GHF values to the highest estimate of Greve, and clearly state that this is from NGRIP. We have restructured this entire section and the following section to be more reader friendly.

Comment

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.

Response

Thank you, as indicated earlier, we moved this sentence to the introduction L50.

Comment

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.

Response

Thank you, we removed the statement as it did not contribute to the section. We did, as mentioned, include a plot of the melt anomaly by Macgregor et al. (2016) (Fig 6) for an improved, direct comparison of both magnitude and spatial extent.

Comment

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 arange of different model set-ups.

Response

Yes this is a caveat of our model set-up, and based on your recommendation we extended the discussion of the linear friction law in the new caveat section of the discussion (L269-275). We agree that the plume would change given a different model setup, and this is discussed in more detail in the section starting at L267.

Comment

Line#: 290

"confirms previous studies" is too strong. "is consistent with" would be better

Response

Thank you, we changed this to your suggestion.

RC2: Signe Hillerup Larsen

Comment

1. Structure of method and result section: a) The storyline in the experiment and result section does not match. In the results section the focus is on the study testing the hypothesis of the existence of a geothermal heat flux anomaly of 970 mW/m2. The rest of the experiments are described as sensitivity studies to this main hypothesis. This is not the story line in the experiment section.

Response

Thank you for noticing this, we have changed the storyline in the experiment section to match the one in the results. More specifically we removed the range of GHF in the sensitivity studies in the beginning of the experiment section. The storyline in the experiment section is now the following: first we present the 970 plume experiment; explain why we need a Ctrl; and finally we present the sensitivity simulations and explain their purpose. In the results we start with the Ctrl in order to explain the background values for all the simulations.

Comment

2. Results section: a) Presentation of results: I think it's a good idea to use the 50 m/yr contour to compare results. Maybe add some meta text in the beginning explaining that this is your approach and if possible add the observed contour line on all result plots for comparison?

Response

Good suggestion, we added a description of how we evaluate the performance of each model simulation using these contours in L138-140. We agree, and originally tried to include both modeled and observed velocity contour in the results plot. However it was messy and too much information in one plot. We therefore decided to show the observed velocity contour on all the result figures apart from the velocity figures where we plotted the modeled velocity contour.

Comment

b) In the first paragraph of the results section the Ctrl simulation is described as a way to obtain the basal melt rate, and then in the same paragraph the resulting velocity field is explained. I find this a bit confusing. Maybe just stick to the explanation about the velocity field, because the method to obtain N is already described in the methods section.

Response

Thank you expressing this, we removed the methods part. In fact, we removed all the part of this section concerning methods to avoid unnecessary repetition.

Comment

3. Discussion section:

a) the discussion is purely focussed on the ice/bed interface, but I am wondering about how the resulting flow pattern depends on uncertainties within the ice such as viscosity and the fact that shear margins are not resolved by the 15km grid. Thus a short discussion of ice viscosity, shear margins and model resolution should in my opinion be included.

Response

Thank you, this is very good point that we did not include originally. We added a caveat section where we discuss how we could obtain similar high velocity as in the 970 experiment, by changing other parameters in the model and then getting away with lower geothermal heat flux values. In L276-280 we discuss the softening of shear margins and how we may overestimate the lateral drag.

Comment

> b) The aim is to have a model that is independent of present day observations. This is not strictly met in the way N is obtained, which is clearly explained. However, the bedmap is also based on modelling using present day velocity observations, which could bias the results, this makes the basal friction coefficient relate to observed velocities in a more diffuse way. This should also be mentioned somewhere.

Response

Thank you, we agree. We included this caveat in L289-290.

Comment

> 4. Conclusions: a) Conclusions appear a bit too conclusive, and the authors should make an effort to make it clearer that they are aware that this is a relatively simple test of the hypothesis that a geothermal heat flux anomaly could explain the onset of NEGIS.

Response

We modified the conclusion and added a sentence on model caveats, allowing the reader to understand how the number presented is dependent on model uncertainties (L323). As explained above we added a section in the discussion where we suggest other ways we could trigger fast flow of NEGIS in our model, apart from the geothermal heat flux.

Comment

> Line#: 60-65
> Effective pressure is defined in words twice.

Response

Thank you, we fixed that.

Comment

> Line#: 153-154
> The last sentence of the paragraph makes it sound a bit like that the 970 mW/m^2 experiment represents reality. Maybe just explain how the ice stream signature becomes weaker with lower forcing.

Response

Thanks, we toned down and included 'given our model set-up' in this statement.

Comment

Line#: 199

I am wondering if the width of the modelled ice stream could be related to model underestimating viscosity?

Response

This is a very good point, and may explain the more diffuse modelled velocity pattern and lack of sharp gradients in the shear margins. We added your suggestion about width in the shear margin viscosity discussion, L279-280, thanks.

Comment

Line#: 212-213

The sentence starting with: 970 mW/m^2 is only...should be moved to methods section.

Response

We agree that it is too late to include here. We find it more a result than a method, as this is computed by the plume model and not prescribed. We removed this statement from the discussion, as it is not important. We generally restructured the section in the discussion where we compare our findings to previous studies, and try to better explain why our values are so high.

Comment

Line#: 222

Maybe refer to Martos et al, 2018 or other paper that describes the continental passage over the Icelandic hotspot. This information should probably be included in the introduction or methods section.

Response

Thank you, we agree and we moved this statement to the introduction (L50). And for the high background geothermal heat flux due to Iceland plume we refer to Rogozhina et al. 2016 and Martos et al. 2018 (L38).

Comment

Line#: 281 By inverting for basal friction you not only create a basal friction map that cannot evolve in time, you also place all uncertainty from the model viscosity for example in the basal friction map.

Response

Yes this is true, everything uncertain in the model is blamed on the spatially varying 'bed properties'.

Comment

Figure 1:

Include the place names used in the text e.g. Storstrømmen and Zachariæ.

Response

Great suggestion, we included this in Figure 1c, where we introduce EGRIP and the model domain.

Comment

Figure 2, 3, 4 and 5:
Maybe show the observed (white) 50 m/yr contour in all the velocity plots where only the modelled contour is shown.

Response

As stated above, we originally tried this, but the figure was not clear so we avoided this.

Comment

References:
The reference to the Fox Maule paper or data is incomplete.

Response

Well spotted, we completed this reference, thank you.

References

Fahnestock, M., Abdalati, W., Joughin, I., Brozena, J. & Gogineni, P. (2001, dec). High Geothermal Heat Flow, Basal Melt, and the Origin of Rapid Ice Flow in Central Greenland. *Science*, *294*(5550), 2338–2342.

Macgregor, J., Fahnestock, M., Catania, G., Aschwanden, A., Clow, G., Colgan, W., . . . Seroussi, H. (2016). A synthesis of the basal thermal state of the Greenland Ice Sheet. *Journal of Geophysical Research: Earth Surface*(121), 1328–1350. doi: 10.1002/2015JF003803

Scambos, T. A. & Haran, T. (2002). An image-enhanced DEM of the Greenland ice sheet. *Annals of Glaciology*, *34*, 291–298. doi: 10.3189/172756402781817969

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