

# ***Interactive comment on “Exceptionally High Geothermal Heat Flux Needed to Sustain the Northeast Greenland Ice Stream” by Silje Smith-Johnsen et al.***

**Signe Hillerup Larsen (Referee)**

shl@geus.dk

Received and published: 7 November 2019

General comments:

The paper builds on the hypothesis, put forward by Fahnstock (2001) amongst others, of the existence of a geothermal heat anomaly at the initiation of the North East Greenland Ice Stream (NEGIS). The Ice Sheet System Model is used as a tool to test this hypothesis. The model experiments presented are relevant and rigorous, while also building on a large modelling effort by the co-authors in previous publications.

The paper is in most parts clearly written and model setup is well described. The

Printer-friendly version

Discussion paper



discussion is thorough on the topic of how basal meltwater will affect ice flow patterns. Results and conclusion are convincing in a sense that the study makes a good case for a strong geothermal heat flux anomaly to be the reason for NEGIS to originate to far inland.

In discussing the results I am however missing some comments on uncertainties in ice flow viscosity which have been shown by a few studies (Van der Veen (2011) - Controls on the recent speed-up of Jakobshavn Isbræ, West Greenland; and Bondzio et al. (2017) - The mechanisms behind Jakobshavn Isbrae's acceleration and mass loss a 3D thermomechanical model study) to be important for maintaining ice stream flow. See specific comments on the paper content below.

The presentation of the experiments and results could also be improved as written in details in the specific comments on the structure of the paper.

Specific comments on the structure of the paper:

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/m<sup>2</sup>. The rest of the experiments are described as sensitivity studies to this main hypothesis. This is not the story line in the experiment section.

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?

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

[Printer-friendly version](#)[Discussion paper](#)

section.

Specific comments on content:

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 15 km grid. Thus a short discussion of ice viscosity, shear margins and model resolution should in my opinion be included.

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.

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.

Line by line comments:

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

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

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

212-213: The sentence starting with: 970 mW/m<sup>2</sup> is only... should be moved to methods section

[Printer-friendly version](#)

[Discussion paper](#)



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.

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.

Figures:

Figure 1: Include the place names used in the text e.g. Storstrømmen and Zachariæ

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

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

---

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-212>, 2019.

Printer-friendly version

Discussion paper

