Review comments:

tc-2023-102
Evaluating different geothermal heat flow maps as basal boundary conditions during spin up of the Greenland ice sheet by Zhang et al., 2023

Submitted to: The Cryosphere

Review by: Lu Li

Zhang et al., 2023, present an exciting study that uses an ice sheet model to evaluate seven different geothermal heat flow models in relation to the thermal state of the Greenland ice sheet. They employ an ice sheet model combined with different geothermal heat flow maps and utilize two distinct ice sheet model initialization methods: constrained and unconstrained spin-up. This approach tests the impact of both the geothermal heat flow model and the initialization method on the thermal state of the Greenland ice sheet. Their findings indicate that both the geothermal heat flow and the initialization method significantly influence the modelling results, affecting the thermal state, velocity, and thickness of the ice sheet. As such, they recommend that future ice sheet model intercomparisons account for the effects of both the geothermal heat flow model and the model initialization method.

The overall contribution of the manuscript is substantial, which would be very interesting for the entire community, especially in the near future as international intercomparison of ice sheet models will require the incorporation of this critical information. This work is definitively worth being published in The Cryosphere.

However, my major concern with this paper is that the figures in the paper do not seem to match the text in the main body, making it very hard for readers to follow the main content of this manuscript. At the same time, the figures presented do not support what the authors describe in their paper (Figure 6&7). Therefore, I strongly suggest that the authors carefully revise their figures and their figure captions before the manuscript enters the next stage of publication.

Another comment relates to the resolved thermal state, geometry and velocity of the Greenland ice sheet, particularly when considering the results derived from unconstrained and constrained spin-up methods. In line 138, the authors mentioned that the unconstrained spin-up is more physically-based. Furthermore, in line 335, they pointed that it is necessary to fully resolve the influence of the geothermal heat flow boundary condition on ice sheet geometry and velocity. In comparison with the constrained spin-up – which factors in the implications of geothermal heat flow, geology, and hydrology on the friction coefficient – I wonder if the unconstrained spin-up might overemphasize the impact of geothermal heat flow on the ice sheet by neglecting other components (geology, hydrology etc…) which is important to ice sheet flow. I recommend that the authors add a detailed discussion on this.

I'll begin by addressing my concerns about the figure, followed by some general comments on this paper.

Comments about the figure and figure caption:

All the figures cite Artemieva 2018 should be Artemieva 2019

Lines 537-539, Figure 1: The figure caption indicates that these heat flow maps represent anomalies from their ensemble mean. However, all the figures display only positive colormap labels for these so-called heat flow anomalies. This suggests that either the figure caption is incorrect, or the colormap label should indicate both positive and negative values.

Line 539: mW m-2 to mW m^{-2}

Figure 5: I recognize that it's a detailed figure with abundant information, could you please label the geothermal heat flow models directly in sub-figures a-g? This would make it more straightforward for readers to find the differences between models. Additionally, could you modify the labels for borehole measurements? Consider using a different colour for measured borehole temperatures or introducing distinct labels. This would help readers quickly identify regions where the model predictions align with borehole measurements, and where they do not.

Figure 6: Could you please verify if the correct images have been included in the figure? Upon a simple visual inspection, Figure 6 (d) appears identical to Figure 6 (g). Additionally, on Line 570, '106 Pa yr m^{-1}' to '10^6 Pa yr m^{-1}'

Figure 7: This is arguably the most confusing figure in the entire paper. The authors label it as 'Case 2'. However, within the main text, it seems to refer to 'Case 1' for the spin-up initialization. Observing the figure closely, models with the highest geothermal heat flows appear to also have the highest friction coefficients, which correspond to the highest surface velocities. Conversely, maps with the lowest geothermal heat flows seem to correlate with the lowest basal friction coefficients and the lowest surface velocities. Yet, in the main text from lines 199-203: "Perhaps counterintuitively, the highest surface ice velocities are associated with the lowest geothermal heat flows (Figure 7). For example, the high and low heat flow end members of the Lucazeau [2019] and Colgan et al. [2022] maps yield, respectively, low and high ice-velocity end members. Similarly, within the Rezvanbehbahani et al. [2017] simulation, the low heat-flow anomaly in southeast Greenland results in a high ice-velocity anomaly." This text description does not align with the figure. I strongly advise the authors to thoroughly review and revise this figure."

Figure 8: It's hard to distinguish between the dashed line and the solid line for different model results. Could you change the color for each model to make it clearer?

Figure 9: Line 589: You compared with Case 2 in Figure 2. Did you mean to refer to Case 1 in Figure 2?

Figure 10: Line 594: The figure caption mentions that the units in all plots are "°C below the pressure-melting-point temperature." Does this mean that the warmer colours in Figure 10 represent temperatures below the pressure-melting-point and the cooler colours represent temperatures above the pressure-melting-point? Could you verify if this is what you intended to show?

Figure 11: Can you confirm if all the ice thickness anomalies are within 100 meters? If so, please include this detail in your figure caption. Additionally, could you comment on the statistics regarding the ice thickness anomalies in the main text?

Detail comments:

Line 52: Tectonic age, might change to tectonic setting?

Line 63-64: Both latter methods then infer heat flow from the respective isotherms by applying a thermal model. Could you provide a brief comment on what the "thermal model" entails in this context? For instance, is it a lithospheric model with constant crust heat production, or something else?
Line 99-102: The potential influence of geothermal heat flow boundary condition on basal ice temperature also remains unclear. For example, basal ice that is 1℃ below pressure-melting-point temperature deforms approximately ten times more than…

It seems you are referring to the influence of geothermal heat flow boundary condition on basal ice rheology or basal ice deformation. I suggest modifying the text to align with this context.

Line 107-109: We also discuss the pros and cons of these seven Greenland geothermal heat flow products in the specific context of potential utility for ISMIP7 Greenland ice flow simulations.

Could you check if the statement is accurate? It seems the major discussion is about the impact of difference heat flow models for specific locations and in paleo ice sheet simulation. Sorry if I missed that, I didn't come across a discussion on the pros and cons of these seven heat flow products in the context of ISMIP7.

Line 117: basal shear stress is weighted using a grounding-line parameterization.
Could you be clearer what do you mean by groundling-line parameterization?

Line 120: minimize misfit against observed present-day ice thickness.

Sorry if that's a silly question, could you please comment on why did you decide to use ice sheet thickness as the initial condition to modify the basal friction coefficient instead of the ice sheet surface velocity? Or perhaps a combination of thickness and velocity for the nudged spin-up? Is this choice a result of the ice sheet model you're employing, or is there another rationale behind it? Could you also discuss the potential impacts arising from different ice sheet model initialization methodologies?

Line 136 -139: Is there any citation to support this statement and could you express why that the transient initilation is more physically – based method to the ice sheet model initialization? And also why ISMIP7 protocol will encourage fully transient spin ups?

Line 141-142: an idealized vertical englacial temperature profile. Could you be more specific what's is an idealized vertical englacial temperature profile?

Line 159-161: Could you comment why did you chose 'Model 1' with a deep Moho? What might be the implications of choosing 'Model 2' with a shallow Moho for the heat flow model? I have a similar query regarding the Gogineni 2022 model with and without NGRIP.

Line 224-225: Could you be clearer about what do you mean in here? Are you referring that spatially Case 2 is similar compare with case 1. But the model result within Case 2 using different GHF model is different?

Line 232-234: Please list the heat flow names. Basal ice temperatures are better resolved by Case 1 spin up for three heat flow maps (for example…), and better resolved by Case 2 spin up for two heat flow maps (XX), with the remaining two heat flow maps (XX) yielding the same RMSE under both spin ups.

Line 237-247: There are a lot of locations mentions in the text. Could you show the location in maps, so reader could refer to the locations?

Line 248-253: Could you present the velocity difference figure? (Including it in the Supplementary material or the main figures would be beneficial). Also, in line 251, it seems you're discussing velocity and ice thickness differences. Why mention lower ice temperatures and not the velocity variances?

Line 263: The apparent association of higher ice velocities with lower geothermal heat flows under Case 1 spin up outwardly appears to be a clear artifact of nudging the basal friction coefficient during spin up. For what I see in the figure, apparent higher ice velocity with high geothermal heat flow, but with high friction coefficient. Could you please either check your statement or check your figure.

Line 333-336: While most recent ice sheet simulations projecting Greenland's future sea-level contribution have largely focused on nudged spin ups, our simulation ensemble unsurprisingly suggests that unconstrained transient spin up is required to fully resolve the choice of geothermal heat flow boundary condition on ice sheet geometry and velocity.

That's similar to what I said in the main comments. The unconstrained transient spin-up highlights the impact of geothermal heat flow on the ice sheet, as no other factors (such as geology or hydrology, etc.) are considered in the model run. In the constrained run, all factors can be modelled into the friction coefficient. My concern is whether the unconstrained spin-up might overamplify the impact of geothermal heat flow on the ice sheet, given that there are no constraints on other factors that also affect ice sheet flow.

Reference:

Artemieva [2018] should be Artemieva [2019].

Please correct the reference:

Artemieva, I. Lithosphere structure in Europe from thermal isostasy. Earth-Science Reviews,

373 188, 454–468, https://doi.org/10.1016/j.earscirev.2018.11.004, 2019.

To:

Artemieva, I. M. (2019). Lithosphere thermal thickness and geothermal heat flux in Greenland from a new thermal isostasy method. Earth-Science Reviews, 188, 469-481.