Reply to Reviewer 1:

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.

We thank the reviewer very much for this positive and encouraging support!

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.

Figure 7 is now changed and corrected.

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.

Thanks for the comments. The constrained spin-up ignores the bed state and in effect rolls up all the physics into a field of friction coefficients that are fixed in time. The unconstrained spin-up does not entirely neglect geology and hydrology, but treats the bed state in a simplified way. For example, the friction angle is a function of bed elevation (roughly parameterizing the transition from a hard to a soft bed), and the effective pressure depends on the basal water depth (which, however, is computed by a local till model and not as part of a distributed hydrology system). The local till model likely makes the basal friction overly sensitive to the local temperature, as opposed to a hydrology model that would spread the effects of basal melting more broadly. I added some text to make these points explicit. In the revised manuscript, we now add a new paragraph at the end of the Discussion section to include these uncertainties.

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

They are all corrected.

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.

Thanks for pointing out this mistake. They are actually not anomalies, but values of geothermal heat fluxes. We now corrected the caption.

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

Corrected.

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.

The geothermal heat flow models are labeled as suggested. We also label 5 deep ice borehole locations as 1-5 so that readers can see clearly their corresponding locations in the map. We do not label the left shallow boreholes as they are largely overlapped near the ice sheet margin and are hard to identify.

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$_{-1}$’

Thanks for pointing out this mistake. Now Fig 6d is corrected, and '106 Pa yr m^{-1}' has been changed 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."

Now we have corrected this part. Yes, Figure 7 should be for Case 1, and we renumber this figure as Figure 12, which is referenced in Line 250.

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?

Line colors are now changed.

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

Yes, it is now corrected.

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?

The caption was wrong. The warm colors here means the basal temperature for Case 2 are above that for Case 1, and cool colors mean the basal temperature for Case 2 are below that for Case 1. We now update the figure caption.

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?

No, the actual data range is much larger as the thickness differences at some locations near the ice sheet boundary are pretty big, but across the majority region of GrIS the difference is small. We now saturate the colorbar by [-150, 150] m in order to have a clearer look at the spatial pattern of ice thickness difference. We have put more information in the caption and in the main text.

Detail comments:

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

Changed.

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?

We added two sentences to further describe the method in Lines 64-68.

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.

We modified the sentence to "The geothermal heat flow boundary condition can significantly influence the basal ice temperature and thus change the ice flow rheology".

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 different 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.

The main goal of this study is to look at the impact of basal thermal boundary condition (geothermal heat flow) on the ice sheet model initialization (spin-up for the CISM DIVA solver), which is important for us to more accurately estimate the future changes of ice sheets and their impact on SLR. In the previous ice sheet model intercomparison studies, however, the importance of the basal thermal boundary condition was not clearly understood and accounted for. So we are hoping our study could be helpful for ISMIP7 design. We changed the phrase "ISMIP7 Greenland ice flow simulations" to "future Greenland ice flow simulations".

Line 117: basal shear stress is weighted using a grounding-line parameterization.

Could you be clearer what do you mean by groundling-line parameterization?

The grounding line parameterization (GLP) is a sub-grid method of indicating the location of the grounding line during the model run. By this method we can more accurately capture the fraction of each grid cell that is grounded and thus subject to nonzero basal shear stress. We modified the description to "basal shear stress is weighted in proportion to the grounded fraction of the cell using a sub-grid grounding-line parameterization." We also added a reference to Leguy et al. (2021), which describes the CISM GLP in detail.

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?

In general there are two initialization methods, data assimilation and spin-up. The data assimilation method indeed uses ice surface velocity to infer basal friction parameters based on an inversion method , whereas the spin-up method runs the model forward to reach an approximate steady state. In CISM, basal friction coefficients are nudged using a method similar to that of Pollard and DeConto (2012). This method has already been applied successfully to the Antarctic ice sheet by Lipscomb et al. (2021). For somewhat technical reasons, thickness is a more robust spin-up target than velocity. In our experience, adding a velocity target does not improve the results. In most regions, the spun-up velocity field is similar to observations, showing that steady-state thickness and velocity are closely tied. In regions where the thickness and velocity are out of balance (e.g., velocities have increased recently but the thickness has not had time to adjust), the spun-up velocities can be inaccurate, but this is not the case for most of Greenland. We revised the text to cite the earlier studies that used thickness as a nudging target and to point out that the spun-up velocities are in good agreement with observations.

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

It is because the nudged method adjusts basal coefficients as needed to fit a thickness target, without reference to the basal state (e.g., frozen or thawed). In the constrained initialization, however, the basal friction coefficient depends on the physical bed state (temperature, water depth, and elevation). We added text describing the unconstrained method in more detail, to make these differences clear.

A more physically based basal boundary condition can be preferable if the model is run several centuries into the future, i.e. over a period when basal conditions are likely to evolve. Since ISMIP7 will place a greater emphasis on long projections (beyond 2100), unconstrained spin-ups could be a useful complement to data assimilation and nudged spin-ups. We revised the text to make this more clear.

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

We used the initial temperature profile described by Lipscomb et al. [2019]. This profile is linear where the SMB is negative, and is based on advection–conduction balance where the SMB is positive. We added some specific text.

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.

For *Artemieva* [2019], we simply choose one of the two models. The author does not favor one of the models but the differences between both are also small enough to not include both models in our analysis. The maximum difference between both models is indeed 40 mW/m^2 but the mean difference is only 4 mW/m^2.

I guess you mean the Colgan et al. [2022] without NGRIP model. This is the model recommended by the authors. The machine learning results for the training without NGRIP yield a much more stable, and therefore also more reliable, result than the model trained with NGRIP included. We furthermore choose the GHF models for the spin ups to span a wide variety of scenarios. The case with elevated GHF around NGRIP is already covered by the Rezvanbehbahani et al. [2017] GHF prediction.

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?

Yes, both Case 1 and 2 show a similar spatial pattern of ensemble agreement, and we also do not deny their difference. We now add a reference Figure 4 here to avoid confusion.

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.

Heat flow names (citations) are added.

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?

We now add "(South Greenland)" as an explanation of "South Dome", and also add more detailed information in Figure 5.

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?

The ice speed differences are now included as Figure 13 in the end of the manuscript.

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.

Figure 7 is now corrected.

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.

We agree with the reviewer on this issue, and this is exactly what we discussed in the summary. Despite that the unconstrained transient spin-up is more physical, it depends much on the sliding law we choose and still needs more further studies in the ice sheet modeling community.

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.

Corrected.

Pollard, D. and DeConto, R. M.: Description of a hybrid ice sheet-shelf model, and application to Antarctica, Geosci. Model Dev., 5, 1273–1295, https://doi.org/10.5194/gmd-5-1273-2012, 2012.