Editor’s comments are in blue, our reply in black, quotes in the revised manuscript in red.

Dear Authors,

Thank you for your replies. Both reviewers agree that this could be a valuable contribution to the field if you address their major concerns sufficiently. Some of your responses to the main points are rather brief and do not always completely resolve the underlying concerns. I am listing some of the issues below and would like to ask you for a more complete reply to the reviewer’s concerns.

It is good that you picked up the reviewer’s suggestion to run longer simulations, but you did not mention the results of these simulations in your response. Please add a discussion of these results and their implications in your revised manuscript.

Reply: We extended the simulations using the full-Stokes model to the year 2100 with three different sliding parameterizations but the same sub-shelf basal melt rate. Sorry, we did not mention the results in our previous rebuttal, although we updated related figures (Fig. 6, 7, 8, 9) in the previous revision.

We add more about these results and their implications in section 4.1 “4.1 Initial state and reference run result” of the revision as below:

We select two flowlines for analysis: FL1 along the main trunk, and FL2 on the eastern branch (Fig. 5) of TG. Modelled annual averaged grounded ice thinning rate from 2015 to 2100 measured from the 2100 grounding line position and extending 20 km further upstream is 2.14~2.35 m yr^{-1} along FL1 and 2.88~5.06 m yr^{-1} along FL2.

We add more about these results and their implications in section 4.2 “Sensitivity experiments results” of the revision as below:

Between 2015 and 2100, when applying the regularised Coulomb and linear Weertman sliding parameterizations, the surface ice flow speed exhibits an acceleration upstream of the grounding line, and the ice thickness upstream of the grounding line experiences a gradual decrease. This ice flow speed acceleration and ice thinning are more pronounced in the eastern grounding zones compared to the southern ones (Fig. S4, Fig. S5). Conversely, when the nonlinear Weertman sliding parameterization is employed, the surface ice flow velocity undergoes a slight deceleration in the vicinity of the grounding line, while it experiences a slight acceleration at locations further upstream (Fig. S4). The surface lowering is much smaller using nonlinear Weertman sliding parameterization than that using the regularised Coulomb and linear Weertman sliding parameterizations. More specifically, the surface ice speed change and ice thickness change along FL1 and FL2 from experiments in the sliding parameterization group are shown in Table 3, Figure 7 and 8.

Table 3. Surface ice speed change, Δv, and ice thickness thinning rate, Δh, from the year 2015 to 2100 (2100 minus 2015) at the 2100 grounding line position and 20 km further upstream, along FL1 and FL2 from experiments in the sliding parameterization group.

		Along FL1		Along FL2	
	Sliding Parameterizations	Grounding Line 2100	20 km Upstream	Grounding Line 2100	20 km Upstream
Δv (m yr^{-1})	Regularized Coulomb	195	324	270	405
	Linear Weertman	165	240	200	537
	Nonlinear Weertman	-31	18	-39	-34
Δh (m yr^{-1})	Regularized Coulomb	2.14	2.35	2.88	5.06
	Linear Weertman	2.29	2.65	2.83	6.42
	Nonlinear Weertman	0.59	0.91	0.70	0.79

From 2015 to 2100, when applying both the regularized Coulomb and linear Weertman sliding parameterizations, the grounding line along FL1 retreats at an approximate rate of 0.14 km yr^{-1} (Fig. 7). Along FL2, the retreat rates under these two parameterizations are approximately 0.27 km yr^{-1} and 0.42 km yr^{-1} respectively (Fig. 8). When the non-linear Weertman sliding parameterization is used, the grounding line retreats at a rate of around 0.04 km yr^{-1} from 2015 to 2050. However, after 2050, the retreat is negligible.

The simulation grounded area loss rate between 2015 to 2100 from the Sliding parameterization group experiments are heterogeneous in both time and space (Fig. 5, 6, 9a).

The modelled grounded area loss using nonlinear Weertman after the year 2050 is much less than that before 2050. The modelled grounded area loss rate using regularised Coulomb becomes slower from 2050 to 2075 while the glacier retreats over rumpled terrain, and re-accelerates after the year 2075. The modelled grounded area using linear Weertman decreases at a nearly constant rate becoming gradually slower in the last two decades.

However, the modelled VAF using the three basal sliding parameterizations decreases almost linearly over time (Fig. 9c), due mainly to the dynamic ice flow. The modelled VAF loss of the TG sub-basin from the Sliding parameterization group experiments is equivalent to global sea level rise of 5.67 mm, 5.48 mm and 3.29 mm over the period 2015-2050 and 16.35 mm, 15.97 mm and 7.34 mm over the period 2015-2100 using linear Weertman, regularised Coulomb and non-linear Weertman sliding parameterizations, respectively (Fig. 9c).

We also add two figures in Supporting Information as below.

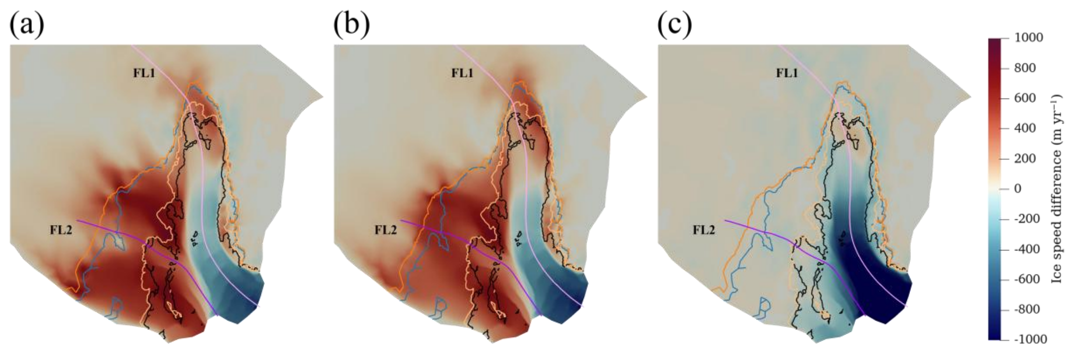

Fig. S4. The surface ice speed change from the year 2015 to 2100 (2100 minus 2015). The solid black line represents the initial grounding line position in the year 2015. The orange, blue and pink curves represent grounding line positions in the year 2100 from experiments using regularised Coulomb (a), linear Weertman (b) and non-linear Weertman (c) in the Sliding parameterization group (Table 2).

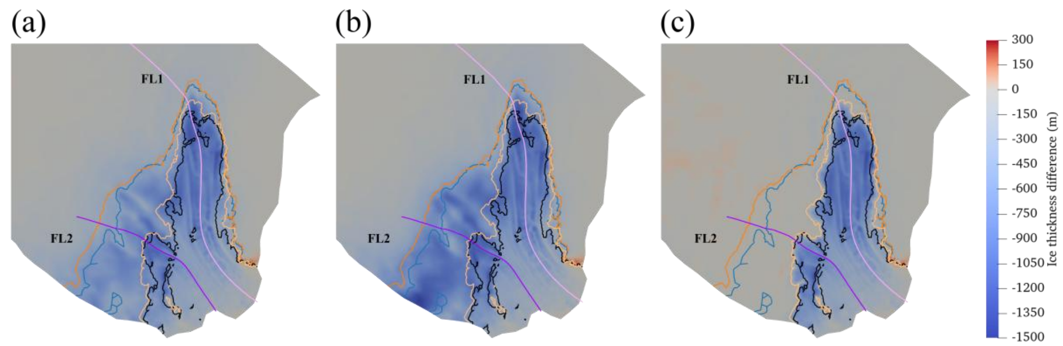

Fig. S5. The ice thickness change from the year 2015 to 2100 (2100 minus 2015). The solid black line represents the initial grounding line position in the year 2015. The orange, blue and pink curves represent grounding line positions in the year 2100 from experiments using regularised Coulomb (a), linear Weertman (b) and non-linear Weertman (c) in the Sliding parameterization group (Table 2).

The statement that BP leads to a 50% reduction compared to FS is a bit thin and I would encourage you to elaborate more on the model differences in case of BP and FS. It is also not clear whether you run BP until the year 2100 as well.

Reply: Sorry. We found a mistake in the setup of BP simulation that showed a 50% reduction. We corrected it. We only ran steady state iteration using the BP model, restarting from the steady state result of FS model. We did not run the BP model until the year 2100.

We do more comparisons between BP and FS as shown below. We compare the surface ice speed and basal ice speed between BP and FS, see the figure below.

The figure shows: Both models reveal similar surface and basal velocity patterns. The ice speed simulated using BP is several hundred meters per year faster than that simulated using FS near the grounding line, but several hundred meters per year slower at the ice shelf, especially near the ice front.

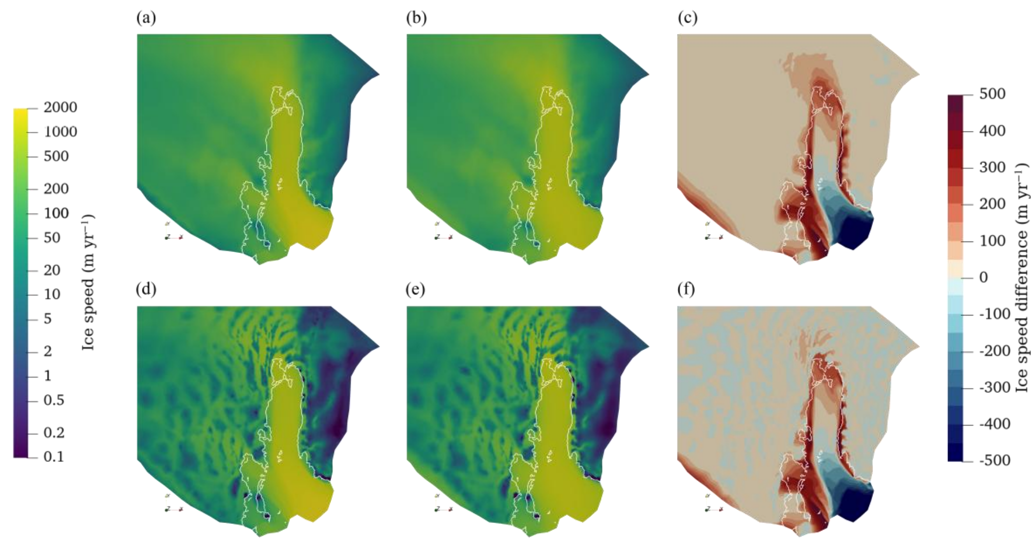

Fig. S7. The simulated surface (the first row) and basal (the second row) ice speed in steady state simulation using full-Stokes model (a, d) and Blatter-Pattyn model (b, e) restarting from the steady state result of full-Stokes model, and their difference (c, f) which is Blatter-Pattyn minus full-Stokes.

We found a reference, Rückamp et al. (2022), which compares the ice dynamics using full-Stokes (FS) and Blatter-Pattyn (BP) approximation. They found that both models simulate similar surface velocity patterns, but the BP solution exhibits higher flow speeds in the fast flow region and slower in the slow flow region. The differences between FS and BP increase with higher velocities, and are stronger when using a power-law friction than a linear friction law. The velocity difference between FS and BP we find in our results is consistent with Rückamp et al. (2022).

We add in the revision:

We ran a steady state simulation using the Blatter-Pattyn model, restarting from the steady state result of the reference run using the full-Stokes model and using the same basal sliding parameterization as in the reference run. We found that both models reveal similar surface velocity patterns (Fig. S7). The ice speed simulated using Blatter-Pattyn is several hundred meters per year faster than that simulated using full-Stokes near the grounding line, but several hundred meters per year slower at the ice shelf, especially near the ice front. This is consistent with the finding in Rückamp et al. (2022), which compares the full-Stokes and Blatter-Pattyn models applied to the Northeast Greenland Ice Stream. They also found that the discrepancies between full-Stokes and Blatter-Pattyn increase with higher velocities, and are stronger when using a power-law friction than a linear friction law. The speed difference in the vicinity of the grounding line is significant because marine ice sheet behaviour is largely controlled by feedbacks (involving both ocean-induced melt and basal resistance) close to the grounding line. The Blatter-Pattyn simulation predicting faster flow speed in this region would cause differences in the evolution over time of the system, hence we chose to focus on full-Stokes simulations for the current study. A comprehensive investigation into the time evolving impacts of the various approximations to the Stokes equations on evolution of the Totten Glacier would be valuable, but is not the focus of the current study where we use a complete representation of stresses.

Reference:
Rückamp, M., Kleiner, T., and Humbert, A.: Comparison of ice dynamics using full-Stokes and Blatter–Pattyn approximation: application to the Northeast Greenland Ice Stream, The Cryosphere, 16, 1675–1696, https://doi.org/10.5194/tc-16-1675-2022, 2022.

The major concern of reviewer 2 should be addressed more thoroughly. In your revision you merely state that you think that the differences in velocities derive from horizontal shear and that the ripples/oscillations in the differences between surface and basal speed are due to either rough terrain or basal friction coefficient. In figure 7 there is no sign of strong bedrock roughness/variability, and you don't provide an explanation for the ripples in the basal friction coefficient which are derived from inversion. A more thorough explanation of these model results would be welcome. In your response you state: "The variation in basal friction coefficient is related with the variation in basal velocity". While it is of course true that variability in the basal friction coefficient drives variability in basal sliding, no explanation is provided what the causes of the variations in basal friction are.
Reply: We agree this question by referee 2 is good point. Sorry we did not reply to it thoroughly.

We add more discussion in this revision as below. Although there is no direct evidence of strong bedrock roughness/variability, we provide an explanation for the ripples in the basal friction coefficient which are derived from inversion. The truth is, that inversion results do not directly give us the cause of any given pattern in the basal friction, they only allow us to infer that the pattern is there. Ripples, oscillations, or "ribs" have been found in other inverse models of both Greenland and Antarctica, starting with Sergienko and Hindmarsh (2013). We add a brief discussion of the ribs below:

Rib-like patterns in basal friction have been found in inversions in both Antarctica and Greenland (Sergienko and Hindmarsh, 2013; Sergienko et al., 2014; Wolovick et al., 2023). These ribs in the basal friction mimic, but do not exactly follow, similar rib-like patterns in the gravitational driving stress (Wolovick et al., 2023). Sergienko and Hindmarsh (2013) posited that basal traction ribs form from coupled instabilities in the till-water-ice system, and supported that supposition with process modeling which produced broadly similar rib-like patterns as those seen in their inverse model. However, strictly speaking, an inversion cannot tell us the definitive cause of any given structure in the inverted field, it can only inform us that the structure exists. Wolovick et al. (2023) found that rib-like structure in the basal drag was sensitive to the choice of regularization in the inversion, which should be expected for short-wavelength features. However, they also found that the ribs were present in both their base case inversion with the best optimal corner lambda value, as well as in their best combined basal drag map. We did not perform a full L-curve analysis in this paper, but we did test values of 10^3, 10^4, 10^5, 10^6, 10^8, 10^{10}, for the regularization parameter λ_β in the inversion, choosing 10^3, which is relatively small, and hence amenable to more variable basal friction fields. If we had chosen a larger lambda value, it is likely we would have seen fewer ribs in our inverted result. However, the fact that similar ribbed structure has been seen in many different inverse models applied to many different geographic regions- and that ribbed structure is also seen in the gravitational driving stress, which does not depend on any inverse model results- suggests that at least some of this structure must be genuine.

Reference:
Sergienko, O. V., & Hindmarsh, R. C. A.: Regular patterns in frictional resistance of ice-stream beds seen by surface data inversion. *Science*, *342*(6162), 1086–1089, 2023.
Sergienko, O. V., Creyts, T. T., & Hindmarsh, R. C. A.: Similarity of organized patterns in driving and basal stresses of Antarctic and Greenland ice sheets over extensive areas of basal sliding. *Geophysical Research Letters*, *41*(11), 3925–3932, 2014.
Wolovick, M., Humbert, A., Kleiner, T., and Rückamp, M.: Regularization and L-curves in ice sheet inverse models: a case study in the Filchner–Ronne catchment, *The Cryosphere*, 17, 5027-5060, https://doi.org/10.5194/tc-17-5027-2023, 2023.

I agree with the reviewer that close to the grounding line and upstream (a few kilometers) velocity differences appear to be zero (at least your figure 7 suggests this for the "initial" case). It would probably help if you highlighted the grounding line position in panels a-f as well to make this clearer.

Reply: There was some misunderstanding of the location where the velocity differences appear to be zero. What we thought is the place where the velocity is stably close to zero, rather than the isolated zeros in the large oscillations. Now we see the referee means the isolated zeros in the large oscillations. Then we agree with the reviewer that there are large oscillations of velocity difference within tens of kilometers upstream the grounding line, and the velocity differences are close to zero at places a few kilometers upstream the grounding line.

We add in the revision

The oscillations in the difference between the surface and basal ice speeds near the grounding line and upstream are caused by the rib-like pattern of basal friction and basal velocity. The difference between surface and basal speeds drops to zero, which means the basal friction approaches zero in the fast flowing trunks upstream of the grounding line (Figs. 7 and 8). The fast-flowing ice streams are supported by shear margins or isolated sticky regions through long-distance stress transmission (Wolovick et al., 2023).

We highlight the grounding line positions in all panels of Figure 7 and Figure 8.

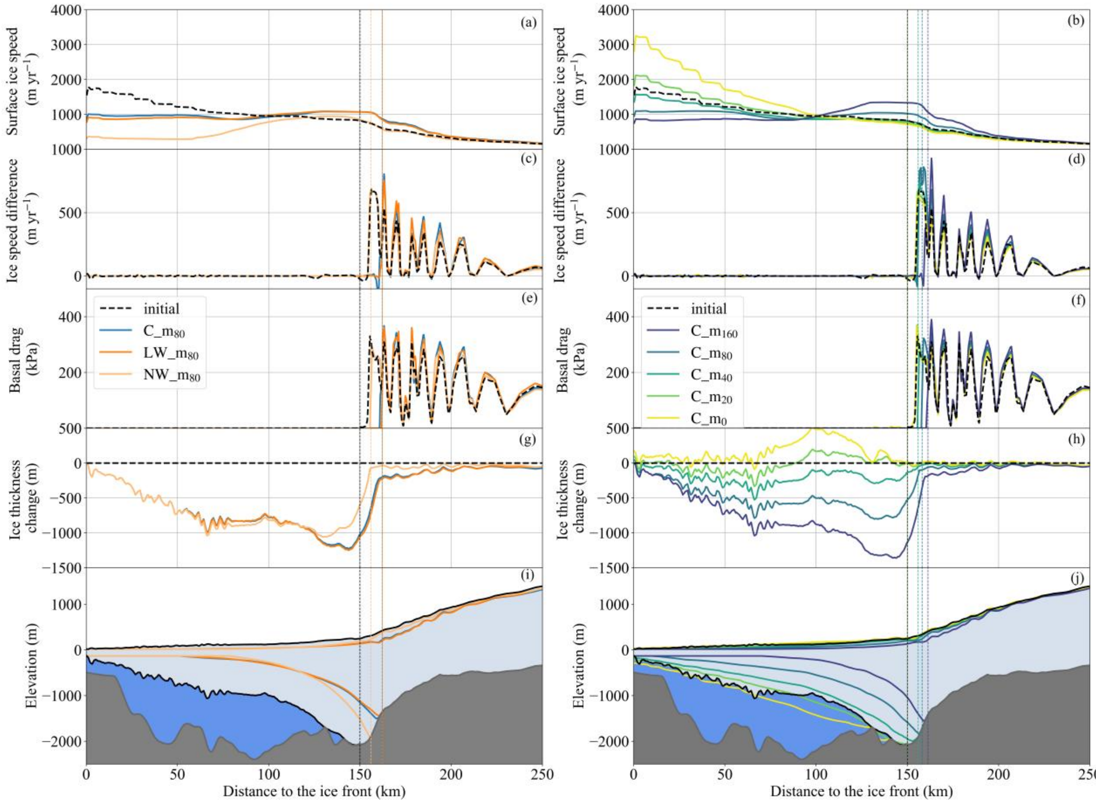

Figure 7. Surface ice speed **(a, b)**, ice speed difference **(c, d**; surface minus basal**)**, basal drag (e, f) ice thickness change **(g, h)**, and ice-sheet profiles **(i, j)** along FL1 in the initial year 2015 (black solid line) and the year 2100 (coloured solid line) from experiments (Table 2) in the Sliding parameterization group **(left column)** and the year 2050 (coloured solid line) in the Melt rate group **(right column)**. grounding line positions are marked with black vertical dashed line for the year 2015 and coloured vertical dashed line for the year 2100 in the Sliding parameterization group **(left column)** and the year 2050 (coloured solid line) in the Melt rate group **(right column)** . The figure is shaded to show the geometry of the TG along the flow line in 2015, where dark grey is the bedrock, light blue the ice shelf and dark blue seawater. The elevations are exaggerated by a factor of 25.

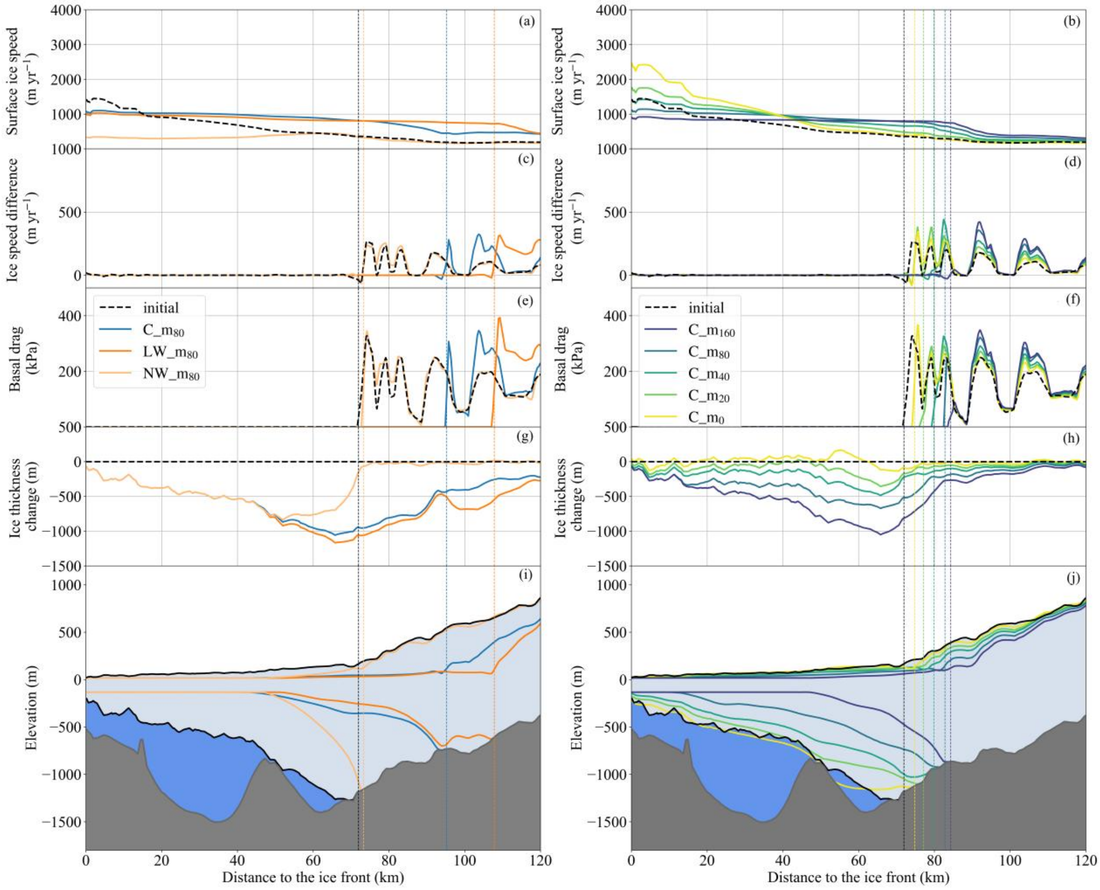

Figure 8. The same as Figure 7 but for FL2.

It is good that you fixed the python issues, but I am surprised by your statement that the oscillations in figure 7/8 are not relevant in figure 5. Figure 5 shows a transect and the ice speed along this transect through depth. Thus, both the surface as well as the basal ice speed is shown in Figure 5 and illustrated in Figure 7/8 in a different way. In your next reply to the reviewer you state that the variations in Figure 5 are indeed the ones you see in Figure 7/8. I would suggest that you address the reviewers' comments more clearly both in your response as well as in the revised manuscript especially regarding the question why the inversion produces the undulating basal friction coefficient patterns in absence of strong bedrock relief variability and no evident drastic changes in ice thickness/surface slopes.

Reply: What we mean in the earlier rebuttal is that the oscillations in Figure 7/8 are not relevant with the vertical lines in the old Figure 5, which are Python artifacts.
We see your meaning now. There are oscillations in basal friction coefficient and speed difference in Fig. 7/8, hence there should be also oscillations in the ice speed along the transect of flowlines in Fig. 5. In fact, there is spatial variability in ice speed along the flowlines in Fig. 5, but it is not very noticeable due to the log scale of the colorbar.

We do not know why the inversion produces the undulating basal friction coefficient patterns in absence of strong bedrock relief variability and no evident drastic changes in ice thickness/surface slopes. The inversion results do not directly give us the cause of any given pattern in the basal friction, they only allow us to infer that the pattern is there. Ripples, oscillations, or "ribs" have been found in other inverse models of both Greenland and Antarctica, starting with Sergienko and Hindmarsh (2013). We add in the revision:

Rib-like patterns in basal friction have been found in inversions in both Antarctica and Greenland (Sergienko and Hindmarsh, 2013; Sergienko et al., 2014; Wolovick et al., 2023). These ribs in the basal friction mimic, but do not exactly follow, similar rib-like patterns in the gravitational driving stress (Wolovick et al., 2023). Sergienko and Hindmarsh (2013) posited that basal traction ribs form from coupled instabilities in the till-water-ice system, and supported that supposition with process modeling which produced broadly similar rib-like patterns as those seen in their inverse model. However, strictly speaking, an inversion cannot tell us the definitive cause of any given structure in the inverted field, it can only inform us that the structure exists. Wolovick et al. (2023) found that rib-like structure in the basal drag was sensitive to the choice of regularization in the inversion, which should be expected for short-wavelength features. However, they also found that the ribs were present in both their base case inversion with the best optimal corner lambda value, as well as in their best combined basal drag map. We did not perform a full L-curve analysis in this paper, but we did test values of 10^3, 10^4, 10^5, 10^6, 10^8, 10^{10}, for the regularization parameter λ_β in the inversion, choosing 10^3, which is relatively small, and hence amenable to more variable basal friction fields. If we had chosen a larger lambda value, it is likely we would have seen fewer ribs in our inverted result. However, the fact that similar ribbed structure has been seen in many different inverse models applied to many different geographic regions- and that ribbed structure is also seen in the gravitational driving stress, which does not depend on any inverse model results- suggests that at least some of this structure must be genuine.

Please also make sure that you address the minor comments of both reviewers conclusively. Some examples below:
I agree with the reviewer's comment that the formulation "their physics is more complete than the simplified models" is not necessary here. It is enough to state that Full Stokes models include all stress components as opposed to approximations such as SIA/SSA etc.
Reply: Okay. We remove "their physics is more complete than the simplified models".

As suggested in the review process regarding the missing bedrock response to changes in ice load: it would be good to include some references which estimate the effect of e.g. 1D ELRA models or fully fledged GIA models on decadal to centennial ice sheet responses.
Reply: We add some references in the revision.
The bed for grounded ice is assumed to be rigid, impenetrable, and fixed over time since the ice sheet geometry change over decades is too small to affect lithosphere deformation (de Boer et al., 2014; Coulon et al., 2021).

References:
de Boer, B., Stocchi, P., and van de Wal, R. S. W.: A fully coupled 3-D ice-sheet-sea-level model: algorithm and applications, Geosci. Model Dev., 7, 2141-2156, https://doi.org/10.5194/gmd-7-2141-2014, 2014.

Coulon, V., Bulthuis, K., Whitehouse, P. L., Sun, S., Haubner, K., Zipf, L., & Pattyn, F.: Contrasting response of West and East Antarctic ice sheets to glacial isostatic adjustment, J. Geophys. Res.-Earth Surf., 126, e2020JF006003, https://doi.org/10.1029/2020JF006003, 2021.

Please quantify the costs for the Full Stokes, BP simulations either in the methods or supplements.

Reply: We add the costs in the revision as below.

These simulations were executed on a server equipped with 2 Intel(R) Xeon(R) Platinum 9242 CPUs operating at 2.30GHz and 384GB memory. Restarting from the steady state result using full-Stokes model in the reference run, the steady state simulation using Blatter-Pattyn model takes 1 minute and 11 seconds, while one timestep forward iteration using full-Stokes model with linear Weertman sliding parameterization takes 15 minutes.

As per the reviewers' comments please use the term "significant" only where it applies to a quantifiable statistical estimate.

Reply: We checked it. We replace "has significant effect on" to "has marked effect on". We replace "significant future grounded ice loss" to "notable future grounded ice loss". We replace "have a significant impact on" to "have an important impact on". We replace "show significant different transient behaviour" to "show notably different transient behaviour", etc.

Best regards,

Johannes Sutter