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

Review Sensitivity of Totten Glacier Dynamcis to Sliding Parameterizations and ice shelf basal melt rates.

In this study, the authors assessed the effect of applying three different basal sliding parameterizations (linear Weertman, non-linear Weertman and regularized Coulomb sliding) and different sub shelf basal melt rates on the modelled evolution (future retreat) of Totten Glacier, East Antarctica. This is an interesting paper, applying a sensitivity analysis on a less studied but nevertheless important area of the Antarctic Ice Sheet, with some relevant results. I enjoyed reading it! It general it is well written, but lacks quantification to back up some key claims. Furthermore, a better justification/explanation on the inversion procedure and for the choice of Full Stokes would be beneficial, see the two major points below.

Major points:

- The authors apply the Full Stokes approximation and argue that this is necessary or justified by considering the spatially varying velocity fields or the velocity gradients. I like and support the use of a Full Stokes solver because, as the authors mention in this manuscript, all deviatoric stresses are resolved. This is especially relevant at small scales and at or close to the grounding line. Only, a major disadvantage is the runtime: the authors mentioned to be limited to 35 model years. To me this is very short, when assessing sensitivities to parameterizations you would like to see longer runtime/more retreat. For this study, I would prefer to see longer simulations with a simpler approximation (DIVA, BP, SSA+SIA), also considering the idealized forcing. If this is outside of the scope/computational expenses, I would at least like to see a comparison with a single iteration with a simpler approximation to quantify the added value of the Full Stokes solver (in terms of for example stresses at the grounding line, ice surface velocity error). Another suggestion could be to use realistic oceanic forcing (from the ISMIP6 project for example), because to me the closer-to-reality FS solver is partly counteracted by a further-from-reality melt parameterization.

Reply: (1) It is expensive to run all the experiments with a full-Stokes model. So 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.
(2) We also compared a single iteration in the prognostic run between a simpler approximation, B-P model, and Full-Stokes model, restarting from the same steady state. Switching from Stokes to BP halves the speed in the shelf, hence full Stokes is required in this context to achieve acceptable accuracy. We add one sentence in the discussion “Switching from Stokes to Blatter-Pattyn for a single iteration in the

prognostic run halves the speed in the shelf, which suggests the necessity of using the full-Stokes model."

(3) On your suggestion of using ISMIP6 ocean forcing, we found a presentation in EGU2020 (Haubner et al., Changes on Totten glacier dependent on oceanic forcing based on ISMIP6, EGU2020-9208) which investigated effects of ISMIP6 scenarios on the Aurora Basin. They used the ISMIP6 sub-shelf (non-local) basal melt parameterization driven by different CMIP models. They found different trends (mass loss or mass gain) with different choices of ocean model, hence there is a very large across-model spread in the ISMIP6 ocean forcing, so although some realizations may be close to reality, their distribution is wide. To help verify our melt rates we include an additional recent satellite-based estimate (Adusumilli et al., 2020) in our revision. "Adusumilli et al. (2020) estimated satellite-derived high-resolution time-averaged (2010-2018) basal melt rate map for most ice shelves in Antarctic, and found the basal melt rate of Totten ice shelf is generally 20-40 m yr^{-1} with a maximum of 80 m yr^{-1} near the grounding line (Fig. 3d)."

We plot the satellite-based estimates of sub-shelf melt rate by Adusumilli et al. (2020) in the updated Fig. 3(d). We find its spatial pattern and magnitude is close to our sub-shelf melt parameterization with the maximum value of 40 m/yr (Fig. 3b). This adds more confidence in our sub-shelf melt parameterization, and we do not think oceanic forcing from ISMIP6 are significantly closer to reality than our sub-shelf melt parameterization.

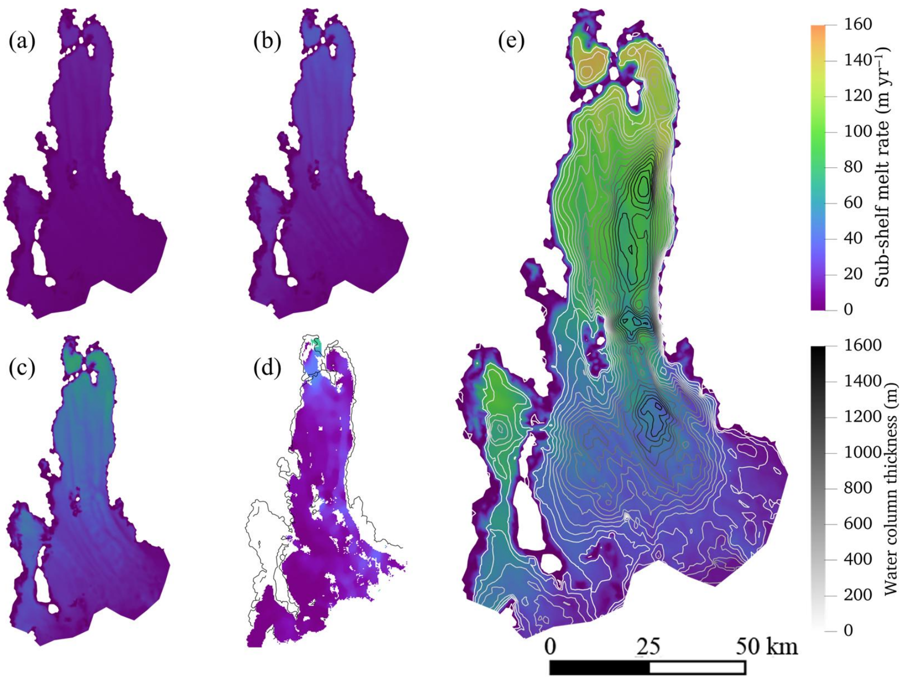

Figure 3. Spatial distribution of modelled sub-shelf melt rate with the maximum value of **(a)** 20 m yr^{-1}, **(b)** 40 m yr^{-1}, **(c)** 80 m yr^{-1}, **(e)** 160 m yr^{-1} with coloured contours indicating water column thickness under the ice shelf with 100 m intervals.Plot **(d)** shows the satellite-based estimates (Adusumilli et al., 2020).

- I am confused by your inversion method. You both mention Gladstone et al

2019 and Gladstone and Wang 2022 as sources for your inversion. The former links to an empty DOI, the latter to a general description on inversion in Elmer Ice applied to Pine Island Glacier. I would like to see more precisely what you did and what equations where used. For example: did you use inverted beta's from Gladstone et al 2019? How did you interpolate them to your grid? Are the runs similar enough that you can 'copy' an inverted field from one simulation to the other? What equations did you use from Gladstone and Wang 2022? See more detailed comments below.

Reply: The doi to this paper (Gladstone, R., Zhao, C., and Zwinger, T.: ISMIP6 Projections-Antarctica read me file, Zenodo, https://doi.org/10.5281/zenodo.3484635, 2019.) is not an empty doi. Co-authors in Europe can open it. But authors based in China cannot open it. So, the problem may be caused by internet limitations. Anyhow, we attached this paper as supplement with the revision, so you can download it.

To answer "I would like to see more precisely what you did and what equations where used. For example: did you use inverted beta's from Gladstone et al 2019? How did you interpolate them to your grid? "

We rewrite this section in the revision

"The following simulations are carried out in series as part of the initialization procedure. We firstly do a steady state simulation with $E_\eta=1$, and the linear Weertman sliding parameterization, expressing the sliding coefficient by $C_{\mathrm{LW}} = 10^\beta$ where β is from the output of a whole Antarctic ice sheet inversion for the year 2015 (Gladstone et al., 2019), and linearly interpolated from their mesh to the finer mesh in this study. Then we relax the free surface of the domain by a short transient run of 1 year with a small time-step size of 0.1 year to reduce the non-physical spikes in the initial surface geometry (Zhao et al., 2018; Gladstone and Wang, 2022; Wang et al., 2020). Taking the results from surface relaxation, we use the variational inverse method (Morlighem et al., 2010) to adjust the spatial distribution of basal friction coefficient β to minimize the mismatch between the magnitudes of the simulated and observed (Fig. 1c; Table 1) surface velocities. We obtain the optimal spatial field of β, an updated modelled ice velocity field, and the stress field.".

To answer "Are the runs similar enough that you can 'copy' an inverted field from one simulation to the other? " We do not 'copy' the inverted beta field of Gladstone et al. (2019) to our simulation. We only take it as an initial estimate and we interpolated it from their mesh to our finer mesh.

To answer "What equations did you use from Gladstone and Wang 2022?", the inversion for basal friction coefficient and the stress enhancement factor follows the same approach as in Gladstone and Wang (2022). The inversions make use of Tikhonov regularization (Gillet-Chaulet et al., 2012). Two separate regularization parameters, λ_β and λ_η, are used for the drag inversions and viscosity enhancement factor inversions. The total cost function, J_{tot}, is the sum of misfit, J_0, and weighted regularization term for the drag inversion:

$$J_{tot} = J_0 + \lambda_\beta J_{reg}.$$

λ_β would be replaced by λ_η for the viscosity enhancement factor inversion.

Line by line specific points:

- Ln 35: a discussion on a marine ice sheet instability like retreat could be added here. Is TG susceptible to MISI?

Reply: TG is not susceptible to MISI at present. But it is potential threat. We change the revision to "The mass imbalance of TG and the existence of deepening topography extending far inland have raised concerns that this subbasin has the threat of significant future grounded ice loss caused by marine ice sheet instability if the grounding line were to continue retreating (Li et al., 2016; Morlighem et al., 2020). ".

- Ln 37: where on the continental shelf? All around the AIS? Or only close to TG? I was in the understanding that it currently is mainly present in the Amundsen Sea Embayement.

Reply: Yes you are right, so we remove this statement.

- Ln 47: How much is 'significant retreat'? Under which climate warming scenarios?

Reply: For the eastern grounding zone, Sun et al. (2016) found retreat of 10 km over 200 yrs driven by HadCM3 under A1B. Pelle et al. (2021) found retreat of about 10 km by 2100 under SSP585. We add this information in the revision. "These studies project that TG will experience continuous and significant retreat on its eastern sectors by 10 km at 2200 under the A1B scenario (Sun et al., 2016) and 10 km by 2100 under the SSP585 scenario (Pelle et al., 2021)."

- Ln 60: 'A nonlinear Weertman…' I do not understand this sentence, could you split it up or rephrase?

Reply: We change this sentence "A nonlinear Weertman sliding parametrization with an exponent of 1/3 applies in the lack of large enough obstacle sizes to induce regelation processes, in which case sliding is dominated by ice-deformation on the largest occurring roughness features (Fowler, 1981)." to "If the obstacle size is not large enough to induce regelation processes, sliding is dominated by ice-deformation on the largest occurring roughness features, and a nonlinear Weertman sliding parametrization with an exponent of 1/3 applies (Fowler, 1981)".

- Ln 74: what could be added here is the notion that Weertman sliding was originally developed for slow ice on hard beds (East Antarctica) and coulom sliding more for faster outlet glaciers such as Pine Island and Thwaites. I believe this is featured in the ABUMIP paper by Sainan Sun.

Reply: Thanks. We already said "The Weertman sliding parameterization is formulated assuming that temperate ice slides perfectly on a hard bed, ..." in this paragraph. We checked the ABUMIP paper, then add in the revision "It has been suggested that regularized Coulomb sliding parameterization is better suited for fast flowing areas such as Pine Island Glacier in the Amundsen Sea Embayment (Joughin et al., 2019)."

References:

Joughin, I., Smith, B.E., and Schoof, C.G.: Regularized coulomb friction laws for ice sheet sliding: application to pine island glacier, antarctica. Geophys. Res. Lett., 46(9), 4764 – 4771, 2019. doi: 10.1029/2019GL082526.

- I couldn't make sense of some of the equations in the method section due to what I expect is a PDF rendering error: some symbols appear as questionmarks. I am not sure where the error lies, but this made it impossible for me to assess these. It happened in Ln 142 – Ln 155

Reply: Sorry. The equations in Ln142 – Ln155 is the Glen's flow law. We will double check the pdf file, and make sure it will not happen next time.

- Ln 81: under which warming scenarios?

Reply: Yu et al. (2018) used different ice shelf melt scenarios: the ice shelf melt rate is parameterized as a function of ice shelf basal elevation and is set to zero above 150 m depth. The ice shelf melt rate linearly increases to a maximum at certain depth. They change the value of maximum to 40, 80, 120, 160 m/a. We change to "Yu et al. (2018) showed that the use of the Budd sliding parameterization produces more grounding line retreat and more VAF loss than with the linear Weertman sliding parameterization with then sub-shelf melt rate linearly dependent of ice shelf basal elevation and the maximum basal melt rate is larger than 80 m/a in simulations of Thwaites Glacier." in the revision.

- Ln 91: 'Has more complete physics' is a very broad statement and one that does not bring enough credit to other modelers groups in the world. I would rephrase this to something like 'Elmer/Ice has the option to run with the full stokes deviatoric stress tensor, which is an unique feature of this model'.

Reply: We do not intend to belittle other models, but the fact is that simplifications neglect some physics. We change it to "Since full-Stokes models consider all the components of the 3D deviatoric stress tensor, their physics is more complete than the simplified models (e.g., vertically integrated 'L1L2' approximation, 2D Shelfy-Stream stress balance approximation) that have been used in previous TG simulations..."

- Ln 91: what are 'the simplified models'?

Reply: We change it to "the simplified models (e.g., vertically integrated 'L1L2' approximation, 2D Shelfy-Stream stress balance approximation)"

- Fig 1: consider adding ice thickness as well, either as contour lines in the first plots or as a separate panel. Also, to remove the amount of numbers showed in these plots you can remove some of the axis labels. For example, in the top row, figure a,b and c share the same y-axis. Just showing it for figure a would be enough I think.

Reply: We improved Fig. 1 as below. We add ice thickness as a separate panel in Fig. 1a. The figures in each rows share the same y-axis. Another referee asked us to add an enlarged plot for each variable in Fig. 1.

- Fig 1c: I would suggest to use a logarithmic scale for this plot.

Reply: We used a logarithmic scale for plot (c).

- Fig 1e: Which reference did you use for these drainage basins?

Reply: We said in Fig. 1 caption that:

The solid black curve is the outline of Totten subregion, from MEaSUREs Antarctic Boundaries for IPY 2007-2009 from Satellite Radar, Version 2 dataset (Mouginot et al., 2017).

- Fig 1: the purple GL is almost invisible. Also, I would suggest to show the GL for the whole area, not just for Totten.

Reply: We have changed the grounding line to black. Also, we add enlarged subplots. The grounding line is clearer.

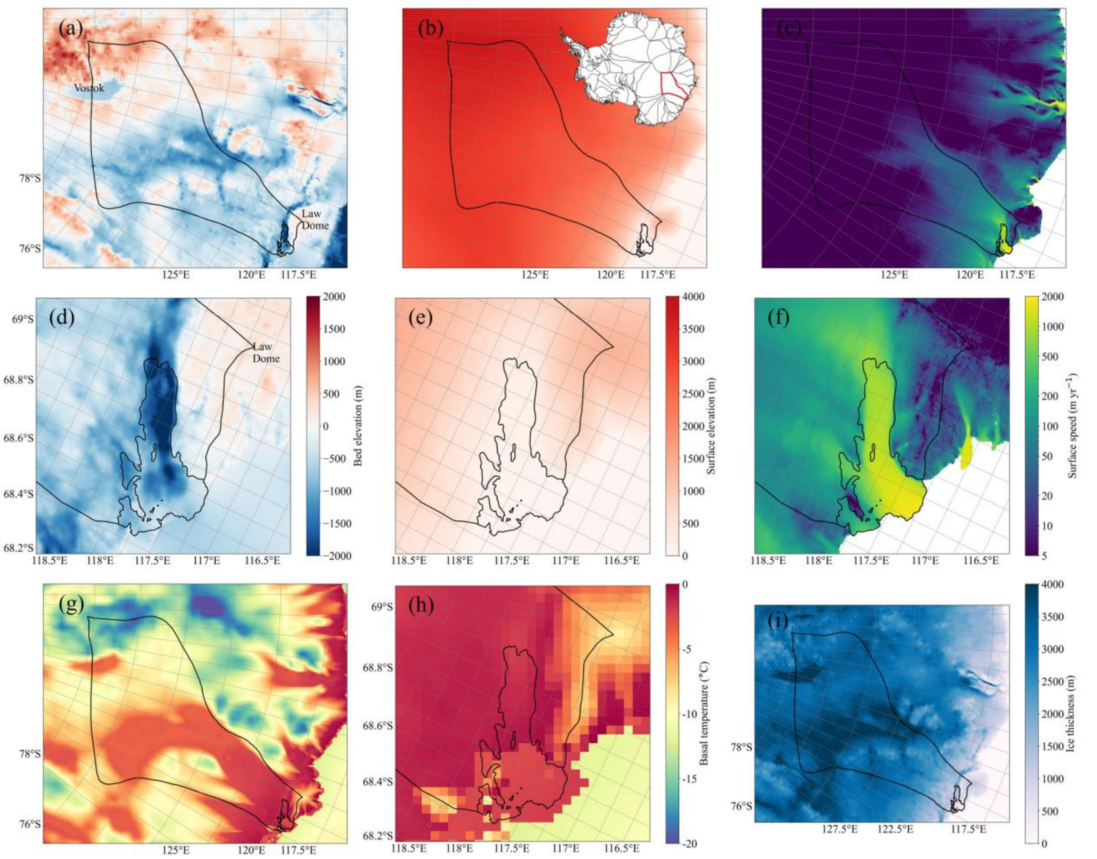

Figure 1. Bed elevation (a), surface elevation (b), surface ice flow speed (c), modelled basal temperature (g) and ice thickness (i) of Totten subregion and its surroundings. The solid black curve is the outline of Totten subregion, from MEaSUREs Antarctic Boundaries for IPY 2007-2009 from Satellite Radar, Version 2 dataset (Mouginot et al., 2017). The inland black curve is the grounding line The bed and surface elevations are from MEaSUREs BedMachine Antarctica, version 2 (Morlighem et al., 2020). The surface ice flow speed is from MEaSUREs InSAR-based Antarctic ice velocity Map, version 2 (Rignot and Scheuchl, 2017). Subglacial lake Vostok and Law Dome are marked in plot (a). The inset plot in plot (b) shows drainage basin divisions and the location of our domain (red curve) in Antarctica. The prescribed ice temperatures are taken from the output of the ice sheet model SICOPOLIS (Greve et al., 2020). (d)-(f) show the enlarged coastal region of (a)-(c), and (h) shows the enlarged coastal region of (g).

Fig 2: It is unclear to me which subfigure belongs where. The main blue discretization should be labeled (a), right? What is the use of showing the upper left red outline? Why are the observed velocities (are those ice surface velocities as well?) so much smaller than in Figure 1c? Also, you're scale bar looks off to me. Subpanel c should be about 400 km in width and maybe 500/600 km in lenght according to its own scalebar, while the red square in subpanel b is about 200 km in dimensions according to the bigger scale bar. I would also suggest to use a different color for the discretized elements in subfigure b. blue is already used in the two colormaps showing ice thickness and ice surface velocities.

Reply: Sorry to confuse you. We improved Fig. 2 as below. We reordered the subplots.

We changed the colorbar in plot (c) to be the same as Fig. 1c. But the color seems a bit darker because the background velocity map is covered with meshes. We corrected the scale bar. We use black for the discretized mesh.

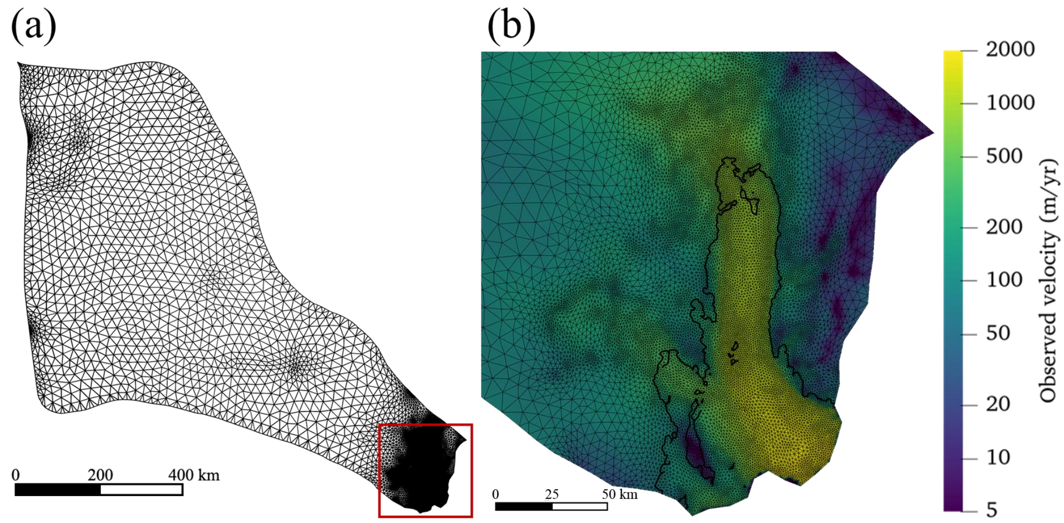

Figure 2. The refined 2D horizontal domain footprint mesh (a). Box outlined in panel (a) is shown in detail and overlain with surface ice velocity in panel (b). The solid black curves in (b) represent the positions of the grounding line.

- Ln 137: which inverse method? And what target variable did you use?

Reply: We improved this sentence in the revision as:

We solve the Stokes equation and use a variational inverse method (Morlighem et al., 2010) as implemented in Elmer/Ice (Gagliardini et al., 2013; Gillet-Chaulet et al., 2012) to determine the basal friction coefficient in the linear Weertman sliding parameterization along with a stress enhancement factor.

- Ln 159: You could add a discussion on the not represented GIA. For 35 years it likely does not matter, but it would still be nice to read that and why.

Reply: We change 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.

- Ln 170: You could make Eq 6 and Eq 7 one equation, and just use m=1 for the linear cases. This saves space and does not require you to write C_lw.

Reply: We prefer to use two equations for the linear and nonlinear Weertman sliding, because we need use the different coefficient, C_{LW} and C_{NW} in the following conversion between the friction coefficients.

- Ln 172: In Eq 8 the vector u_b is used twice. I think the first one should be the magnitude u_b.

Reply: Yes, the first one should be the magnitude of u_b. We changed it.

- Ln 181: You could add Leguy et al 2014 and Leguy et al 2021, where they used a parameterization based on the height above floatation to mimic some hydrological connection for ice resting on bedrock below sea level.

Reply: Thanks. We add the two references.

Leguy, G. R., Asay-Davis, X. S., and Lipscomb, W. H.: Parameterization of basal friction near grounding lines in a one-dimensional ice sheet model, The Cryosphere, 8, 1239 – 1259, https://doi.org/10.5194/tc-8-1239-2014, 2014

Leguy, G. R., Lipscomb, W. H., and Asay-Davis, X. S.: Marine ice sheet experiments with the Community Ice Sheet Model, The Cryosphere, 15, 3229 – 3253, https://doi.org/10.5194/tc-15-3229-2021, 2021.

- Ln 190: In what quantifiable way does this alter your simulations?

Reply: We did the reference run (with Coulomb sliding law and sub-shelf melt rate with maximal value of 80 m/a) using two choices of effective pressure: 1) assuming effective pressure is hydrostatic; 2) setting the effective pressure as 5% of the overburden. We ran both for 35 years. We compare the two effective pressures at the end of the simulation, see plots below (Fig. S1). We found that the assuming perfect hydrostatic balance results in at least twice the effective pressure as assuming 5% of overburden near the grounding line, and an order of magnitude larger over the far inland region.

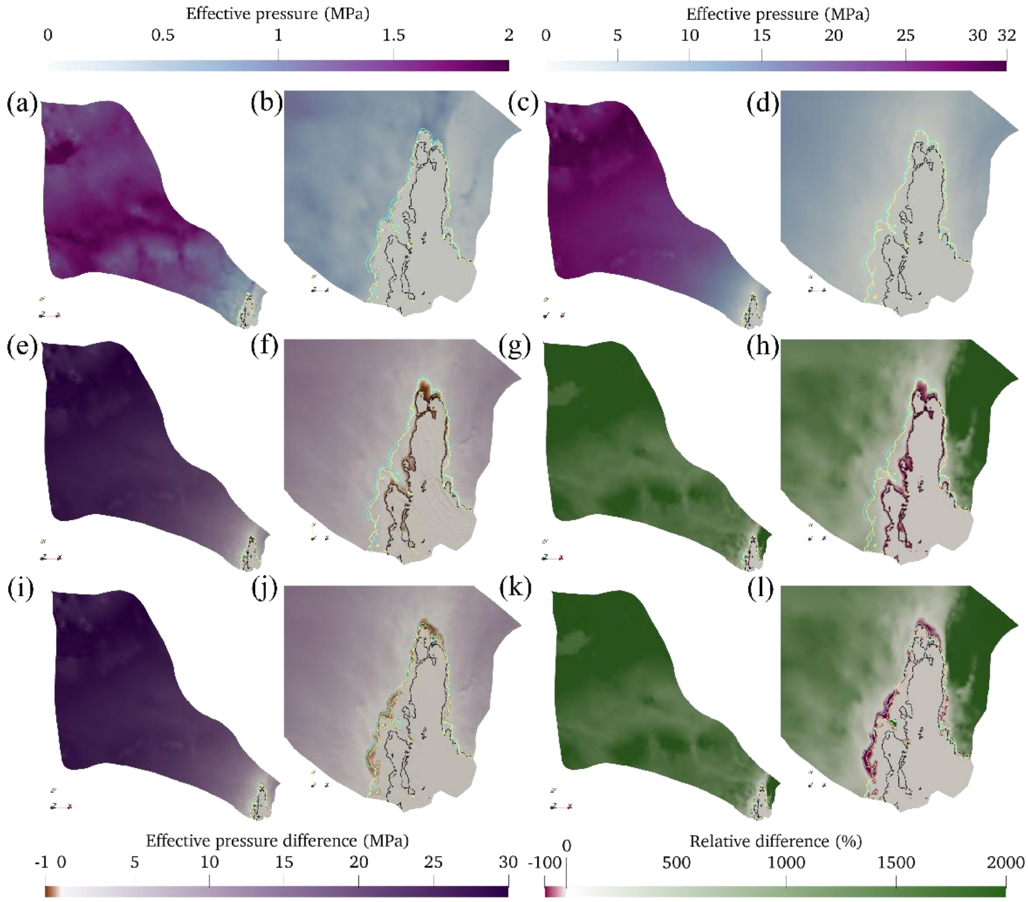

Fig. S1. Effective pressure set to 5% of overburden (a, b) and effective pressure assuming perfect hydrostatic connection and subglacial elevations (c, d) after 35 years simulation. Difference (e, f, i, j) and relative difference (g, h, k, l) between 5% of overburden and perfect connection at the beginning (e-h) and the end (i-l) of the 35 years simulation. The yellow and cyan curves represent the grounding line positions under 5% of overburden and perfect connection respectively after 35 years simulation. Note the different colorbar scales for (a, b) and (c, d).

We also compared the modelled surface velocity and grounding line position after the 35-year simulations using the two choices for effective pressures, see plots below (Fig. S2). The surface speed differs by ± 400 m/a mainly on the ice shelf and near the grounding line. There are significant differences in grounding line position after 35 years. The grounding line retreats more using the effective pressure under perfect hydrostatic connection. This grounding line position difference (Fig. S5) is similar to that between use of Coulomb sliding law and linear Weertman sliding law after 35 years (Fig. 6).

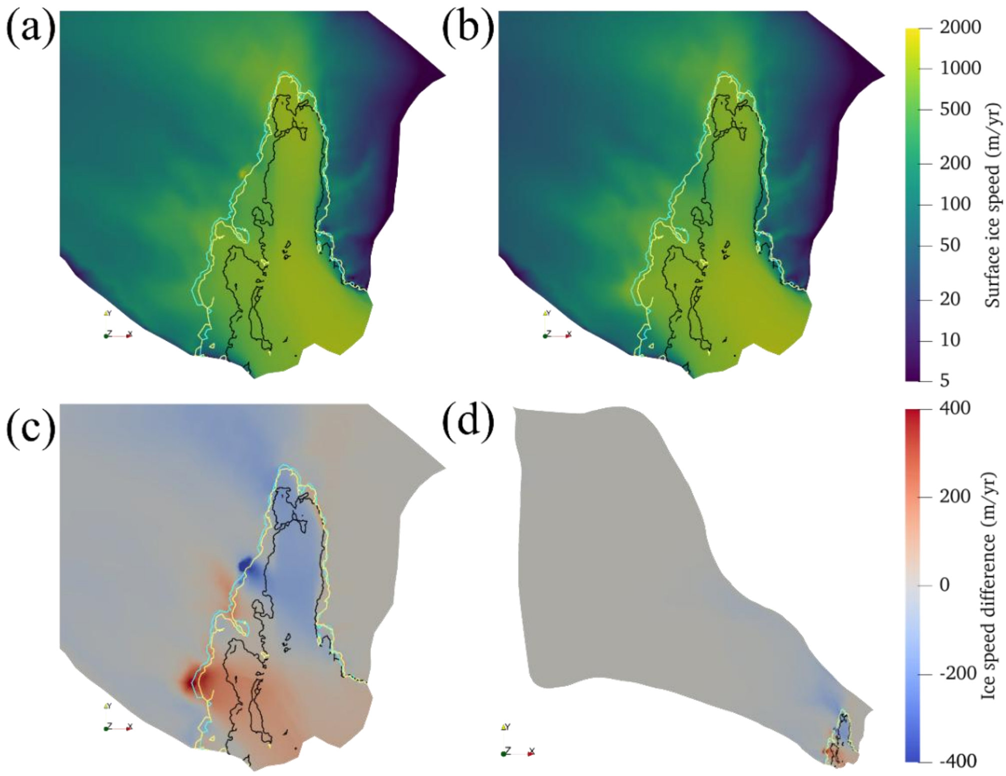

Fig. S5. Surface ice speed using 5% overburden effective pressures (a) and assuming perfect hydrostatic connection (b) after 35 years simulation. Difference (b — a) between (a) and (b) is shown in (c) and (d). The yellow and cyan curves represent the grounding line positions under 5% overburden and perfect hydrostatic connection respectively.

We change this sentence "This assumption alters simulation results significantly from those under the assumption of perfect hydrostatic connection" to
"This assumption decreases the effective pressure by an order of magnitude in most inland regions and halves it near the grounding line, compared with that under the assumption of perfect hydrostatic connection (Fig. S1). "
We add more text in the discussion.
"We compared the modelled surface velocity and grounding line position after 35 years of simulation using two choices of effective pressures: 1) assuming 5% of the overburden and 2) assuming perfect hydrostatic connection (Fig. S5). The surface speed differs by ±400 m/a mainly on the ice shelf and near the grounding line (Fig. S5). There are significant differences in grounding line position after 35 years. The grounding line retreats faster assuming perfect hydrostatic connection. This grounding line position difference (Fig. S5) is similar to that between use of Coulomb sliding law and linear Weertman sliding law after 35 years (Fig. 6)."

We also add Fig. S1 and Fig. S5 in the supporting information.

- Ln 191: What is a linear Weertman sliding inversion? Are there different inversions per basal friction law?

Reply: "The linear Weertman sliding inversion" means the inversion with the linear Weertman sliding parameterization. We only do inversion with the linear Weertman sliding law. We cannot do inversion for other types of basal sliding law directly. That is why we need convert the friction coefficients from linear Weertman sliding law to other sliding laws.
We changed it in the revision:
We apply the basal shear stress and basal sliding velocity field obtained from the inversion with the linear Weertman sliding parameterization to the other two sliding parameterizations, and estimate the sliding parameters there. In other words, we convert the local friction coefficient in the linear Weertman sliding parameterization to that in other sliding parameterizations.

- Ln 196-212: Am I understanding correctly here that, in the case of rewriting Weertman to Coulomb sliding, you could not find an exact solution for the free parameter in the coulomb sliding law and you had to revert to limits to find C_s with the inclusion of a smoothing term in Eq 14? If that is true, then, I like the elegance of this method but I am then not convinced that plugging the C_s you found in Eq 15 in Eq 8 will give you the same tau_b as Eq 7. Is that correct? And if so, can you then show that the deviation of tau_b is small and that it has no to little impact on a continuation simulation?
Reply: No, there is some misunderstanding. There are two free parameters in regularized Coulomb sliding, A_s and C_s. We can find an exact solution for them. Once given A_s, we can calculate C_s. So we firstly find a reasonable expression of A_s, which is Eq. 14. Then we find the exact solution of C_s (Eq. 15) which is dependent of A_s. We assume that the tau_b in Eq. 6 and Eq. 8 are equal. So C_s is also dependent on u_b, C_{LW}, and N.

Plugging the C_s in Eq. 15 into Eq. 8 will give us the same tau_b as Eq. 6 (note it is not Eq. 7). We do conversion from sliding coefficient in Eq. 6 to that in Eq. 8. The tau_b in Eq. 8 with C_s in Eq. 15 is the same as the tau_b in Eq. 6. So there is no deviation of tau_b.

By the way, sorry, there is a typo in Eq. 15, the exponent "1-m" should be "m-1". We corrected it in the revision.

- Ln 220: what is shallow and what is deep water?
Reply: We add numbers here: shallow water (<500 m), deep water (>1000 m).

- Ln 225: Eq 16: where did you get the (non linear!) regression, and how well does it do in representing the values from the WAOM? A scatter plot between the WAOM basal melt rates versus draft depths, with this linear regression fitted through would make this clearer.
Reply: Sorry, the description was wrong. It is not a linear regression of ice shelf bottom depth and ice shelf basal melting rate. We removed the words "linear

regression". We did linear regression of $1/(d+d_0)$ and ice shelf basal melt rate, where d is ice shelf bottom depth, and d_0 is a parameter to choose. We tried many values of d_0, and found that the linear regression of $1/(d+d_0)$ and sub-shelf basal melt rate has the best fit with the modelled result from WAOM when d_0=100.

We add in the revision:

We did linear regression between sub-shelf melt rate and $1/(d+d_0)$, where d is ice shelf bottom depth (unit: m), and d_0 is a tuned parameter. We found the best fit as follows with d_0 =100 m.

We add a scatter plot between the ice shelf bottom depth and WAOM modelled basal melt rate in the supplement (Fig. S2). We also plot the parameterization to account for the high melt rate in the deep water (Eq. 18), with M_{max} = 40 m yr^{-1}, and the total sub-shelf basal melt (Eq. (21)) with water column thickness along the flowline FL1 as an example (Fig. S2).

We add in the revision:

The parameterizations for M_d, M_e with M_{max} = 40 m yr^{-1} and the total sub-shelf melt with water column thickness along a flowline are illustrated in Fig. S2.

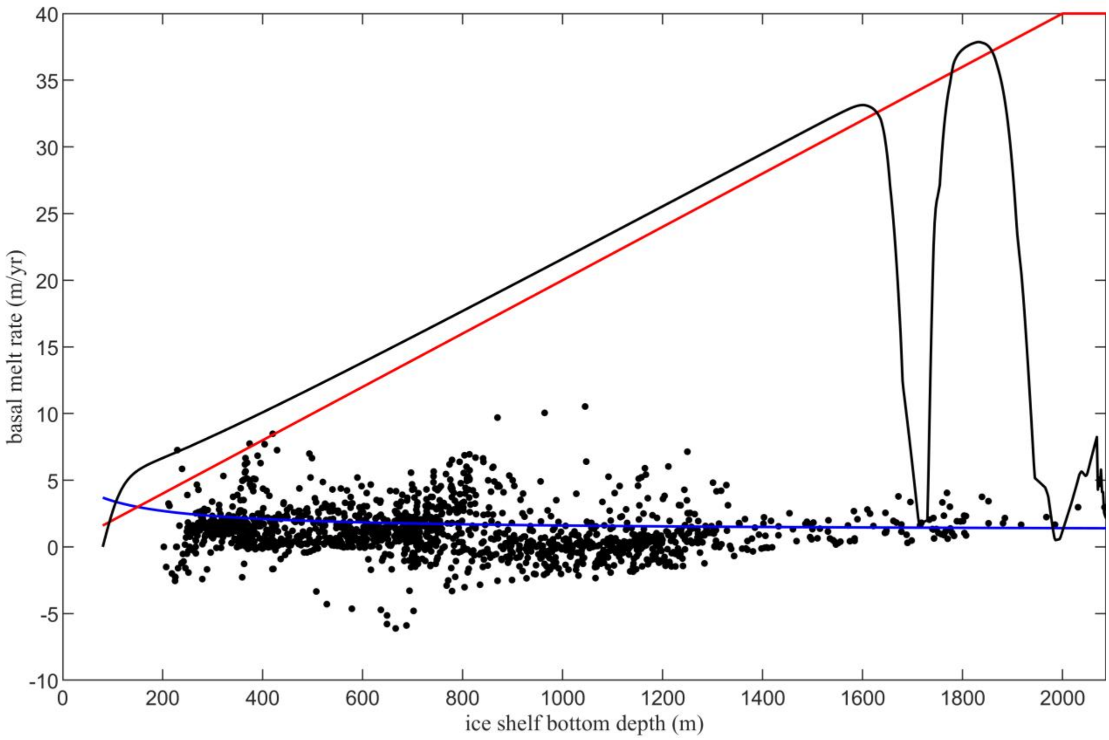

Fig. S2. Basal melt parameterization. Positive value of basal melt rate are melting and negative freezing rates. Whole Antarctic Ocean Model (WAOM v1.0; Richter et al., 2022) simulated basal melt rate (points) - which are far lower than spatially averaged satellite observational rates - see Section 3.3. The blue curve shows Eq. (17) tuned to WAOM modelled sub-shelf basal melt rates (d_0 = 100 m). The red curve is parameterized to account for the observed high melt rates in the deep water (Eq. (18)

with a M_{max} = 40 m yr^{-1}). The sub-shelf melt is set to zero for ice shelf bottom depth less than 80 m (Eq. (20)). The black curve is the total sub-shelf basal melt (Eq. (21)) with water column thickness along the flowline FL1.

- Ln 225: Eq 16: I am also not sure about the addition of d_0 here. In my view, d is always nonzero if there is ice present in a grid element. If there is no ice present, one does not need to calculate the melt rates. Adding 100 is quite substantial, say your draft depth is also 100 m (it typically is between 0 and 1000 m, order of magnitude), adding 100 to your denominator for the sake of preventing zeros will alter your results significantly. Can you not remove this d_0?

Reply: We need d_0 for two reasons: (1) ensure the denominator term is nonzero.; (2) we did linear regression between ice shelf basal melt rate and $1/(d+d_0)$, we tried many values of d_0 and found that d_0=100 m gives the best fit. In the revision, we change it to "We did linear regression between sub-shelf melt rate and $1/(d+d_0)$, where d is ice shelf bottom depth (unit: m), and d_0 is a tuned parameter. We found the best fit as follows with d_0=100 m".

- Ln 245 – 255: I am missing a discussion of the physical interpretation of the S_i and S_w values, just stating that the reflect the influence of cavity geometry and avoid numerical instability is in my opinion not enough. How does the cavity geometry influence the melt rates, and how is that reflected by S_w? Same for the numerical instability.

Reply: Regarding impact of S_i on numerical stability: A vanishingly thin ice shelf causes the tetrahedral elements to have a very high horizontal to vertical length scale, which increases the likelihood of instability. S_i prevents melting the ice shelf once the draft passes Z_s (i.e. no melting for thin ice).

Regarding impact of S_w on stability: A high step change in forcing across the grounding line (whether basal drag or ocean induced melt) causes problems for stability, but this is a main focus of Gladstone 2017. You could look there for more details. S_w prevents high melting right next to the grounding line, so reduces that step change.

Regarding cavity geometry influencing melt rates: a very thin water column restricts circulation (hence S_w). Observations of ocean induced melt always have peak rates at depth and very low melt rates for thinner parts of the ice shelf, hence S_i.

We add in the revision

And S_w approaches 0 when the water column thickness goes to zero near the grounding line, capturing the influence of cavity geometry on melt rate as a very thin water column restricts circulation. A high step change in forcing across the grounding line (basal drag or ocean induced melt) causes problems for stability (Gladstone et al., 2017). S_w prevents high melting right next to the grounding line, so reduces that step change.

A vanishingly thin ice shelf causes the tetrahedral elements to have a very high horizontal to vertical length scale, which increases the likelihood of instability. S_i is an ice-shelf depth-scaling parameter, used to prevent melting the ice shelf once the draft passes $|z_{i0}|$ given by ...

- Fig 3: consider adding the observations to this figure as well.

Reply: We assume the referee means satellite estimates of basal melt rate by "observation". since we already have an enlarged plot for modelled sub-shelf melt rate with the maximum value of 160 m yr^{-1} in plot (e), we replace the small figure of modelled sub-shelf melt rate with the maximum value of 160 m yr^{-1} with the satellite estimates by Adusumilli et al. (2020) in plot (d).

Reference：

Adusumilli, S., Fricker, H. A., Medley, B. et al. Interannual variations in meltwater input to the Southern Ocean from Antarctic ice shelves. Nat. Geosci. 13, 616–620 (2020). https://doi.org/10.1038/s41561-020-0616-z

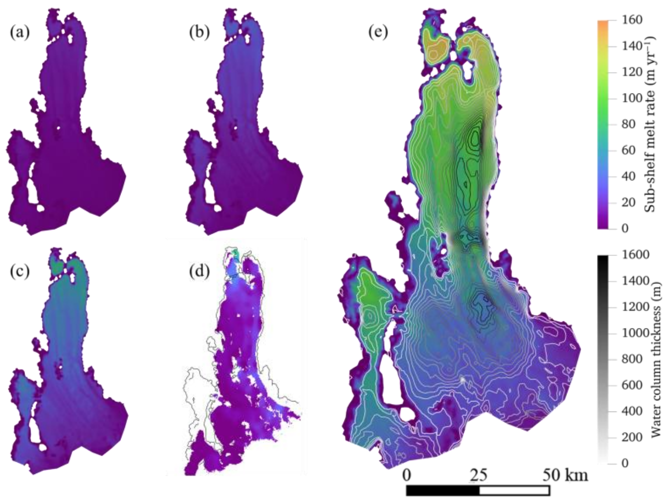

Figure 3. Spatial distribution of modelled sub-shelf melt rate with the maximum value of **(a)** 20 m yr^{-1}, **(b)** 40 m yr^{-1}, **(c)** 80 m yr^{-1}, **(e)** 160 m yr^{-1} with coloured contours indicating water column thickness under the ice shelf with 100 m intervals. Plot **(d)** shows the satellite-based estimates (Adusumilli et al., 2020).

- What is inverted for is clear to me, the basal friction parameters C. However, how this is done (nudging, data assimilation) and with what as target (ice thickness, velocity) is not clear to me. Are you using the inversion results of Gladstone et al 2019? The reference here runs to an empty DOI, so I could not check what inversion you are using. Immediately after that you state the resolution of that simulation to be 4-40 km. Your simulation uses up to 900 meter resolution, how are you interpolating the inverted values without introducing model drift? What's more (and possibly more important) you are using the newest Bedmachine dataset of Morlighem, 2020, Gladstone et al 2019 could not have used this, so they inverted a friction parameter using a different bedrockheight dataset. Also, your temperature profile from SICOPOLIS is different. You have to convince me now that you can take inverted fields from a different model run with different input datasets, approximations and parameterizations, and without problems use it in your own setup. You mention another study at the end of this paragraph (Gladstone and Wang, 2022) where some explanation is given but for Pine Island instead of Totten. I would suggest to add the equations you use from this paper and copy them to your study.

Reply: Sorry that you find an empty DOI for Gladstone et al. (2019), but it is the correct doi and it is there. Maybe it is caused by some internet problem. Anyway, we attach the paper as supporting information with the revision.

The initial value of basal friction coefficient, β, is from Gladstone et al. (2019), in which they used Bedmap 2 geometry. Our mesh is finer than that used in Gladstone et al. (2019). We bi-linearly interpolated the basal friction coefficient from Gladstone et al. (2019) to our mesh as an initial estimate of β. Then we use the inverse method to adjust the spatial distribution of basal friction coefficient β to minimize the mismatch between the magnitudes of the simulated and observed surface velocities. The method is the variational inverse method (Morlighem et al., 2010) implemented in Elmer/Ice (Gagliardini et al., 2013; Gillet-Chaulet et al., 2012), and the target is surface velocity. Then we got our own optimal value of β for the BedMachine geometry we used.

The inverse method procedure is the same as in Gladstone and Wang (2022). We add more equations in the revision.

Our temperature profile is taken from the output of the ice sheet model SICOPOLIS (Greve et al., 2020). Then we interpolated their value to our geometry.

- Ln 266: A diagnostic simulation tells me that you already did some kind of spinup or initialization. I would mention here your spinup procedure, and mention the inversion procedure as well.

Reply: The diagnostic simulation here means a steady state simulation, which can be viewed as part of the initialization. We change the word "diagnostic" to "steady state". But it does not include any inversion yet. The initialization includes an initial steady state simulation, surface relaxation, inversion for basal friction, and inversion for enhancement factor.

We re-organized this paragraph as

The following simulations are carried out in series as part of the initialization procedure. We firstly do a steady state simulation with $E_\eta=1$, and the linear Weertman sliding parameterization, expressing the sliding coefficient by $C_{\mathrm{LW}}=10^{\beta}$.

where β is from the output of a whole Antarctic ice sheet inversion for the year 2015 (Gladstone et al., 2019), and linearly interpolated from their mesh to the finer mesh in this study. Then we relax the free surface of the domain by a short transient run of 1 year with a small time-step size of 0.1 year to reduce the non-physical spikes in the initial surface geometry (Zhao et al., 2018; Gladstone and Wang, 2022; Wang et al., 2020). Taking the results from surface relaxation, we use the variational inverse method (Morlighem et al., 2010) to adjust the spatial distribution of basal friction coefficient β to minimize the mismatch between the magnitudes of the simulated and observed (Fig. 1c; Table 1) surface velocities.

- Ln 272: 'the inverse method'. What inverse method? Please specify.

Reply: It is the variational inverse method (Morlighem et al., 2010). We add it as above.

- Ln 275: If I get it correctly, you target surface ice velocities in your inversion, first with basal friction and then with the viscosity. It would be nice to read something here on this serial approach, why not in parallel? And what was the effect of this extra inversion step with the flow enhancement factor? Can the basal friction inversion alone not give the right ice surface velocities? Also, what was done by Gladstone et al 2019?

Reply: Yes, we target surface ice velocities in our inversion, first with basal friction and then with the viscosity, i.e. the enhancement factor. It is in series. We cannot invert for two variables as the same time. The inversion for viscosity is the inversion for enhancement factor. It is not an extra inversion step.

Some studies, e.g., Gladstone et al. (2019), use basal friction inversion alone to give the right ice surface velocities. We also consider enhancement factor because the viscous response of polar ice can be strongly anisotropic. On the coastal area, due to the large contrast of the stress regimes for the grounded part and for the ice shelf, the ice anisotropy induces an apparent hardening of the ice up to a factor 10 when ice moves from grounded to floating (Ma et al., 2010). The enhancement factor is often used to account for anisotropy effects. We add in the model description "The enhancement factor is often used to account for anisotropy effects (Ma et al., 2010). "

In Gladstone et al. (2019), they used BEDMAP2 geometry. The main target is minimizing the mismatch between modelled and observed velocities. Also a short surface relaxation removes extreme non-physical geometry artefacts and brings the shelves into floatation. The outputs are a spatially varying basal drag coefficient, 3D temperature field, and relaxed geometry. They carried out the following simulations in series as part of the initialization procedure (inversions tune basal friction parameter to MEASURES2 velocities):

1. Surface relaxation. 20 timesteps with dt = 0:001 a.
2. Inversion with relaxed geometry and constant temperature T = 20 C.
3. Steady state temperature simulation using the flow field from 2.
4. Inversion with the new temperature field from 3.
5. Thermo-mechanically coupled steady state temperature-velocity calculation using basal sliding coefficient from 4.
6. Further inversion using the latest temperature field from 5.
7. 10-year surface relaxation with a variable (increasing) timestep.

References;
Ma, Y., Gagliardini, O., Ritz, C., Gillet-Chaulet, F., Durand, G., and Montagnat, M.: Enhancement factors for grounded ice and ice shelves inferred from an anisotropic ice-flow model, Journal of Glaciology, 56, 805–812, https://doi.org/10.3189/002214310794457209, 2010.

- Ln 286: Why is regularized coulomb the most physically sound? Provide references.
Reply: We add the references: Tsai et al., 2015; Joughin et al., 2019.

- Ln 290: SMB has already been mentioned, consider removing it here.
Reply: Done.

- Ln 291: I would argue that, if computational expenses are too high when running Full Stokes, shift to a faster approximation (Hydrostatic, Blatter-Pattyn or for example DIVA) and run further into the future. 35 years is short to make statements about sensitivities to basal friction, maybe the simulations will start to deviate as soon as the grounding line retreats further (e.g. after 100-200 years). Or converge to some steady state upstream. Doing 35 year simulations in my opinion is particularly usefull when making state-of-the art projections of glacier retreat, with forcing from CMIP models. For sensitivity studies like this one, I would recommend to run longer.
Reply: Computational expenses are high for using Full-Stokes model. We compared the short simulation between full-Stokes and Blatter-Pattyn model, however, we found large difference in modelled ice velocity. Switching from Stokes to Blatter-Pattyn halves the speed in the shelf. Therefore, it is not useful to run Blatter-Pattyn model for longer times.

Since 35 years is short to make statements about sensitivities to basal friction, we tried our best to run it longer to 2100. We finished the runs with three different basal friction parameterizations and one ice shelf basal melt rate (max=80 m/a). We updated the results and Fig. 5-8 .

- Ln 296: you do not keep beta fixed right? You rewrite them to fit with other friction parameterizations. Please state so. Also, please make clear which beta (the one from Gladstone et al 2019 or one obtained after your own relaxation) was used.
Reply: We firstly obtain the optimal beta in the inversion with linear Weertman sliding

parameterization. Then we convert it to the coefficients in other sliding parameterizations. Then we keep them fixed in the prognostic simulations. We already said in section 3.4 "With the modelled basal shear stress and basal sliding velocity, we convert the basal drag coefficient from the linear Weertman parameterization to those representing non-linear Weertman and regularised Coulomb parameterizations using the method described in Section 3.2."

We made it more clear in section 4.1 "We obtain the inverted optimal spatial distribution of basal sliding coefficient exponent (Fig. 4a) in the linear Weertman parameterization and the stress enhancement factor (Figs. 4b-c) for the initial year 2015, and keep them fixed in all the prognostic simulations".

- Fig 4: the grounding line is very hard to see, consider changing the colors and/or the thickness. Also I would like to see the observed grounding line position next to the modelled one to asses how well your model performs.

Reply: We change the color of grounding line in Fig. 4. We also add enlarged plots for each variable in Fig. 4. The initial grounding line is determined by the ice sheet geometry from Bedmachine data using the flotation condition. The Bedmachine data is from merged observational data from several years. Hence we already show the observational grounding line. The inversion is a steady state simulation which does not change the initial (observed) grounding line.

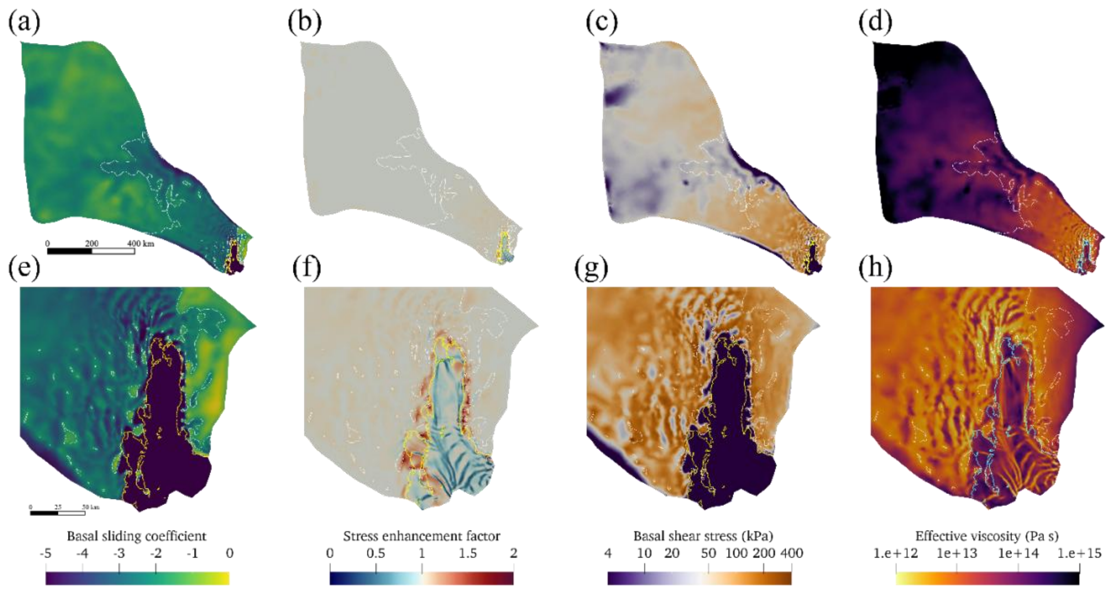

Figure 4. Spatial distribution of modelled **(a)** basal sliding coefficient β, **(b)** stress enhancement factor E_η, **(c)** basal shear stress τ_b, **(d)** basal effective viscosity, and their corresponding enlarged ones (e-h). The solid yellow (a-c; e-g) or cyan (d, h) curves represent the grounding line and the solid white curves in (a-d) show modelled basal speed contours of 10 m yr^{-1}.

- Ln 311: Why does the ice shelf decelerate? I would expect some speedup due to the loss of buttressing due to the loss of ice shelf thickness.

Reply: We change it to "In the reference run, ice shelf thickness upstream of the grounding line decreases significantly, and the ice velocity speeds up upstream of the grounding line but decelerates near the ice front of the ice shelf over the 85-year simulation".

- Fig 5: a difference plot would be more informative here, since the visual difference between beginning and end of the simulation is hard to see.

Reply: We update Fig. 5 to add the difference plot and extend the simulation result to 2100.

- Fig 5 c-f: I appreciate the honesty when saying that the vertical lines are Python artefacts, but I would still like them to be removed before publication.

Reply: We found the problem and fixed it. There are no artifacts in our new plots.

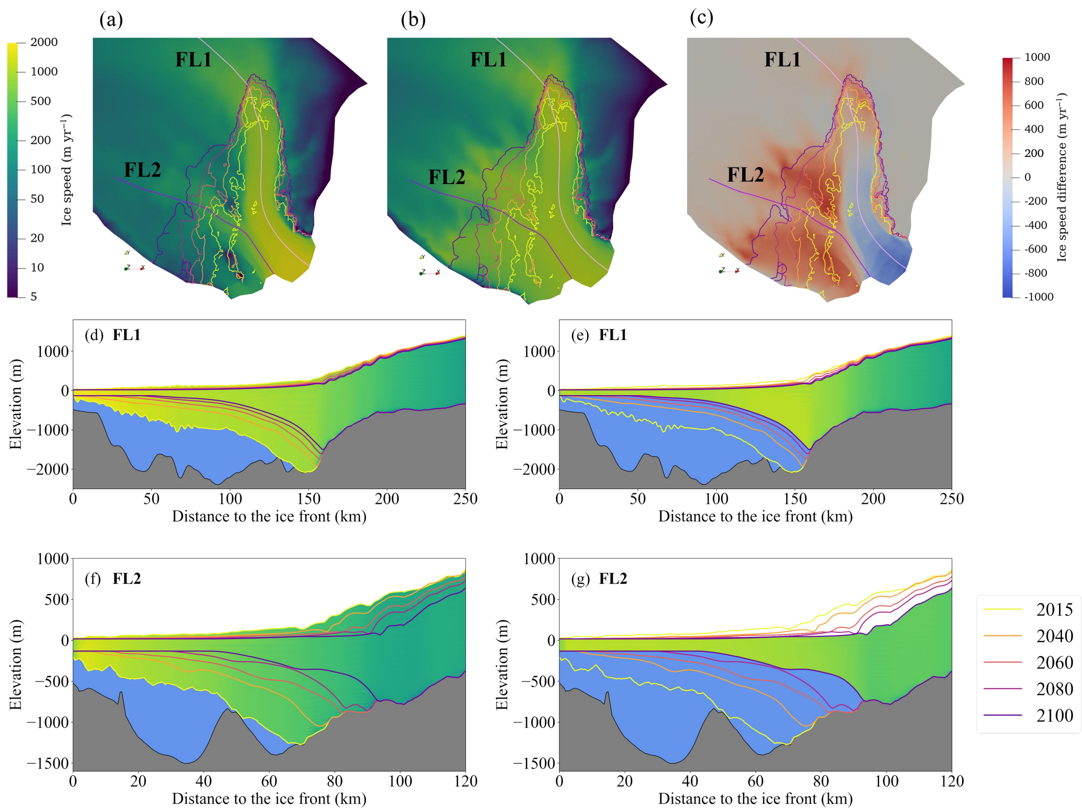

Figure 5. Surface velocity at the beginning of the initial year 2015 (a) and the end of the year 2100 (b) in the reference run. The surface velocity difference (2100 minus 2015) is shown in (c). The grounding line positions in the year 2015, 2040, 2060, 2080 and 2100 are shown in (a-c). Pink and purple solid lines in (a) and (b) represent flowlines FL1 and FL2 as labelled. The solid color portions of the figures show the ice flow velocity (upper colorbar) profiles along FL1 (c, d) and FL2 (e, f) in the reference run in the initial year 2015 (d, f) and the end year 2050 (e, g), with bedrock in dark grey and seawater in blue. The geometry change of TG is marked with colored solid lines for the years 2015, 2040, 2060, 2080 and 2100. The vertical elevations are exaggerated by a factor of 25.

- Ln 327-333: this conclusion, that there are various grounding line retreats for different sliding laws, is not what I got from reading your abstract in which you mentioned that the basal sliding law did not matter.
Reply: We update Fig. 6a with the result of longer years to 2100. We checked the abstract, to make the conclusion consistent.

- Ln 339: this is not neccesarely the case: less sliding and particular coulomb sliding will make it easier for ice to flow from far upstream to the grounding line, preventing the thinning at the grounding line.
Reply: We change it to "The grounding line retreats more using the regularised Coulomb sliding parameterization than with the nonlinear Weertman sliding parameterization, which might because the effective pressure used in the regularised Coulomb is reduced as the ice thins, leading to more basal sliding and faster ice speed. Moreover, the value of χ (Eq. 9) is below 1 in the fast flowing region from our posterior estimate, showing a reduced basal friction and enhanced basal sliding (Eq. 16) compared with a true regularised Coulomb sliding parameterization."

- Ln 353 – 355: I do not agree here. First, the magnitudes of the spatial velocity differences might imply something on horizontal shear and its derivatives, but why is that relevant? Also, if you want to show the spatial variability in the shear stresses, why do you not plot the shear stresses themselves? But the most important point: there are multiple other approximations that take either horizontal or vertical derivatives of the shear stresses into account, or combinations of them. You can pick Hybrid SIA+SSA for example, or the Depth Integrated Viscosity Approximation (DIVA), or Blatter-Pattyn, or the Hydrostatic Approximation. Those will all resolve the quantities you want to detect, with less computational expenses. This does not justify the need for a Full-Stokes model, and if you can only run for 35 years with Full Stokes, I would strongly suggest to run longer with a less computational heavy approximation, or at the very least rephrase and rethink why you chose Full Stokes in the first place.
Reply: As you said, the ice speed difference (surface minus basal) implies something about horizontal shear, or the basal drag. In fact, we found that the ice speed difference variation is very similar to the basal drag variation. In addition, the pattern of difference between the surface and basal ice speed implies high spatial gradients in basal velocity,

We agree we can show the basal drag directly. So we add a separate panel to show the basal drag.

As we mentioned earlier, "Switching from Stokes to Blatter-Pattyn for a single iteration in the prognostic run halves the speed in the shelf, which suggests the necessity of using the full-Stokes model". We extended the prognostic runs in the

Sliding parameterization group to the year 2100.

- Ln 355: Despite what differences? Also, you just argued that there is a huge difference in grounding line response (10 km vs 1 km), now you are writing the opposite. From figure 6, I conclude that there is quite a difference in grounding line retreat when using a different sliding law, contrasting your abstract.
Reply: Sorry for the confusion. We remove this sentence.

- Ln 355: Why is this clearly controlled by the topography? I cannot see this in Figure 6.
Reply: We mean they all mainly retreat along the eastern side, but little on the western side and southern side. That is a minor point. We remove this sentence.

- Ln 370: this conclusion seems to be a bit obvious, that melt rates directly influence the cavity thickness. Whats more interesting is the relation between sliding law and cavity thickness. Can you quantify this effect? Why does another friction parameterization lead to different ice shelf cavity thickness?
Reply: The sub-shelf cavity change is dominated by the sub-shelf melt rate. Considering the the relation between sliding law and cavity thickness, we can see from Fig. 7 and Fig. 8 that in the case of nonlinear Weertman sliding law, the sliding speed is slower than in the other two cases. This causes less grounding line retreat, hence thicker ice shelf near the grounding line. It also could cause less advection of ice through the shelf, hence thinner ice further downstream in the shelf.

We also note that the basal topography along FL 1 is prograde sloping bed with large slope upstream of grounding line, but the basal topography along FL 2 has more variations in slope and the slopes are smaller than that along FL1. Hence the ice is more stable along FL 1 than along FL2. The influence of different basal sliding parameterizations on sub-shelf cavity thickness is more obvious along FL2 than along FL1.

We change in the revision to:
The change of sub-shelf cavity thickness is dominated by sub-shelf melt rates, although different basal sliding parameterizations could yield different retreat of grounding line position, hence different sub-shelf cavity thickness near the grounding line. The influence of different basal sliding parameterizations on sub-shelf cavity thickness is negligible in prograde sloping bed with large slope upstream of the grounding line such as FL1 (Fig. 7), and obvious at locations with relatively small and variable slope such as FL2 (Fig. 8).

- Fig 6 and fig 7: I see much more grounding line retreat difference in Fig 6 compared to Fig 7, why is that?
Reply: Sorry, maybe it is because the plotting scale was wrong in Fig. 2c. Fig. 2c and Fig. 6 have the same scales. We corrected the plotting scale in Fig. 2c and added the

plotting scale in Fig. 6 in the updated plots.

- Fig 9: is there a seasonal cycle in your simulations? It shouldn't be because of the simplified melt parameterization.

Reply: There is no seasonal cycle in our simulation. The plot was made using the results of every timestep. So there is some oscillation. We change to use annual result to make the plot. Then it is smooth.

- Ln 396: I am missing a discussion on your inversion procedure: how did taking the fields from Gladstone et al 2019 influence your results? Was there any model drift or how did you remove it?

Reply: We only take it as an initial guess of basal friction coefficient. Then we use the inverse method to adjust the spatial distribution of basal friction coefficient to minimize the mismatch between the magnitudes of the simulated and observed surface velocities. Therefore, the basal friction coefficient from Gladstone et. al (2019) has no influence on our modelled result.

- Ln 433: 5% is extremely low in my opinion, I would suggest to take a look at the parameterization proposed by Leguy et al 2014 (Parameterization of basal friction near grounding lines in a one-dimensional ice sheet model)

Reply: It is low. But we get the ratio from subglacial hydrology modelling result (Dow et al., 2020). And we compared the simulations with two choices of effective presure in the reply to Ln 190.

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We read Leguy et al. (2014). They proposed a simple function for the effective pressure, $N(p)$, depending on a parameter p, that accounts for connectivity between the subglacial drainage system and the ocean: $N(p) = 0$ at the grounding line when $p > 0$, and $N(p)$ is the ice overburden when far from the grounding line. It is a nice function. But it faces a key challenge of choosing realistic values of p for the study domain. We would like to try that in a future study. We added some text in the discussion.

"Leguy et al. (2014) proposed a function for the effective pressure, N(p), depending on a parameter p, that accounts for connectivity between the subglacial drainage system and the ocean. It could produce a smooth transition between finite basal friction in the ice sheet and zero basal friction in the ice shelf, but it faces a key challenge of choosing realistic values of p for the study domain."

Reference:

Leguy, G. R., Asay-Davis, X. S., and Lipscomb, W. H.: Parameterization of basal friction near grounding lines in a one-dimensional ice sheet model, The Cryosphere, 8, 1239 – 1259, https://doi.org/10.5194/tc-8-1239-2014, 2014

- Overall: the short timescales are not discussed in this section, and its possible effect on the simulations. Barnes and Gudmundsson 2022 and Brondex et al 2017 and Brondex et al 2019 all conducted longer simulations, so you might find similar or non-simular results if you extend your simulations to match their lengths, typically 100-300 years.

Reply: We extend our simulations to the year 2100 with three different basal sliding laws. We find similar results as that by the year 2050. We updated the figures and text in the revision.

- Ln 445: I would not ask you for general statements, but what I would like to read is your thoughts on the fact that shapewise (in a basal friction versus basal velocity plot) the non-linear Weertman and coulomb sliding law look more like each other, than the coulomb sliding law and the linear Weertman. Why are then the modelled ice sheet responses so similar between the linear Weertman and Coulomb sliding law?

Reply: That is a good point. The regularised Coulomb law can provide a continuous transition between Coulomb friction regime and Weertman type friction regime, and the basal drag instantaneously switches from Coulomb friction at low effective pressure to Weertman type friction at high effective pressure. It depends on the parameter χ. We showed in our manuscript that in the limit of small χ, we recover a non-linear Weertman law (Eq. (11)), and in the limit of large χ, we obtain a Coulomb-type of friction (Eq. (13)). Also note that, Eq. (10) is equivalent to Coulomb-type of friction with a factor:

$$\tau_b = \left(\frac{\chi}{1+\chi}\right)^{1/m} C_S N.$$

The factor $\left(\frac{\chi}{1+\chi}\right)^{1/m}$ is about 0.6 if χ=0.275, 0.8 if χ=1, and 0.97 if χ=10. It means a reduced basal friction than that in a true regularised Coulomb law.

The regularised Coulomb law has a complicated form, and there are two free parameters, A_s and C_s. the parameter χ depends on A_s, C_s, basal velocity and effective pressure. In our approach, we firstly decide the form of A_s, then we calculate C_s using the prescribed A_s. Then we find the resulting value of χ is below 1 over the domain, and from 0.3 to 1 in the fast-flowing region with spatial variability and change with time, which means the basal friction in the fast-flowing region is 60%-80% of that in a true Coulomb regime. We also tried to use different form of A_s, but it is hard to get χ as a good transition from very small value in the slow-flowing region to a large (>10) value in the fast-flowing region.

During the work we also tried another approach: we firstly give a constant value of C_s, then we calculate A_s using the prescribed C_s. Then we hope to have both large and small value of χ, i.e. both Weertman type regime and Coulomb regime. However, the assumption of constant value of C_s is not reasonable, and the transition of resulting χ is still not good. How to design reasonable parameters (A_s and C_s) requires further investigation as a longer-term project.

Therefore, we still use the first approach, but we say clearly in the text that it is a regularised Coulomb law with a reduced basal friction .

- Ln 447: 'the maximal basal melt rates are'.
Reply: Done.

- Ln 449: this seems contradictory: is it consistent or are there large differences?
Reply: It is not contradictory. We found that the retreat occurs mainly along the eastern and southern grounding zones. This is consistent with previous studies. But our modelled retreat distance is different from previous studies. We rewrote the sentence as: "The modelled grounding line retreats when maximal basal melt rate is $\geqslant$ 40 m yr^{-1}, and mainly along the eastern and southern grounding zones. This is consistent with previous studies, although with different retreat distance."

- Ln 452: 'Melt' has a typo
Reply: Corrected.

- Ln 455 - 456: Earlier on I read this: 'The sensitivity of sub-shelf cavity thickness to basal sliding parameterization varies spatially', which seems to contradict this statement. Which is true?
Reply: We correct the sentence here:
The sensitivity of sub-shelf cavity thickness to basal sliding parameterization depends on basal topographic slope and ice thickness.

- Ln 465: consider publishing your scripts to make the figures and datasets of your simulations as well.
Reply: Okay.