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

## Manuscript Review

"Sensitivity of Totten Glacier dynamics to sliding parameterizations and ice shelf basal melt rates"

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## Summary:

Ma et al. present an analysis of the effects of three different basal sliding parameterisations and four different ice-shelf basal melt parameterisations on the evolution of Totten Glacier. To do this they use the full-Stokes model capabilities of ElmerttIce and perform simulations over a 35-year period from 2015-2050. They find that grounding line retreat occurs when the maximum value in the basal melt parameterisation is greater than 40 m/a. They also find that the linear Weertman sliding law generates the most grounding line retreat, closely followed by the regularised Coulomb sliding law, with the non-linear Weertman sliding law producing the least grounding line retreat and mass loss over the 35-year simulation period.

The paper is well-written, and the results provide additional insights into the effects of commonly used basal sliding parameterisations on the dynamics of an important glacier in East Antarctica when simulated with a full-Stokes model. However, I have a major point that I would like to see addressed before publication, relating to the presentation and discussion of the full-Stokes model results.

## Major Comment:

My concern is with the presentation and analysis of the modelled ice velocities in Figures 5, 7 and 8, and in the text in lines 349-354.

Panels (c) and (d) of Figure 7 (and 8 to some extent) show large oscillations in the difference between the surface and basal ice speed along a flowline upstream of the grounding line. In some experiments, this difference approaches 800 m/a. This difference must be due to vertical shearing in the ice, and this result implies that over three-quarters of the total flow comes from vertical shearing in these fast-flowing ice stream regions. The fact that the difference between surface and basal ice speed then drops close to zero a few kilometres upstream/downstream also suggests that this large vertical shearing quickly disappears as a factor in the dynamics, and the flow is then dominated by basal slip without any clear variation in the surface or basal topography.

These estimates for vertical shearing in the ice are very large (typical values would be < 100 m/a) and don't appear to be physically plausible. It is also unexpected that there could be such profound changes in the dominant mechanism for flow over a few kilometres along a flowline without any appearance of this in the overall surface speed (as shown by the much smoother curves in panels (a) and (b) of Figure 7 when compared to panels (c) and (d)).

Reply: Firstly, we think the difference between the surface and basal ice speed comes from horizontal shear rather than vertical shear.

Secondly, we do not agree that “These estimates for vertical shearing in the ice are very large (typical values would be < 100 m/a) and don’t appear to be physically plausible.”

Panels (a) and (b) of Figure 7 are surface velocity, they are smooth because the surface topography is relatively smooth. But there could be large variation in basal velocity if the basal topography is rough. The basal velocity depends on ice temperature and also basal topography. For instance, a pinning point could slow down the ice velocity. Therefore, the difference between the surface and basal ice could have large variation. The largest difference between surface and basal ice speed, 800 m/a, happens with ice thickness of ~2 km and surface speed of 1 000 m/a. We think it is plausible with steep basal slope and thick ice. The oscillations correspond to changes of basal topography, basal friction coefficient and basal drag. We can also see the stripes pattern in the modelled basal friction coefficient upstream the grounding line in Fig. 4. The variation in basal friction coefficient is related with the variation in basal velocity (Fig. S6).

Thirdly, we do not agree that “The difference between surface and basal ice speed then drops close to zero a few kilometres upstream/downstream ...”
The difference between surface and basal ice speed does not drop to zero in the 50 km upstream along either FL1 or FL2, see Fig. 7 and Fig. 8. The difference between surface and basal ice speed is zero downstream the grounding line is because it is the floating ice shelf there, and the surface and basal ice speed in an ice shelf is the same.

Could there have been an issue with the post-processing of the model data? The artefacts in Figure 5 – that the authors attribute to plotting in Python – also lead me to this as a possibility. Whilst it is hard to tell definitively, it appears that the oscillations in basal and surface speed that are clear in Figures 7 and 8 are also visible in the ice velocities plotted in panels (c) and (d) of Figure 5 and are attributed to plotting artefacts there. The pronounced gradients in the surface and basal ice speeds (in both the vertical and horizontal dimensions) shown by the stripes of different shades of green appear to be in the same locations as some of the largest oscillations in Figure 7 panels (c) and (d).

Reply: Now we figured out the plotting issue in Python and fixed it. There is no artifact in the updated figures Fig. 5. The oscillations in Figure 7 and 8 is not relevant to Fig 5. The oscillations in Figure 7 and 8 are physically plausible, as we answered in the earlier reply.

Could the authors please verify that these results are not due to an error in the post-processing of the model data? Perhaps visualising the data in ParaView and comparing it with their Python generated plots might reveal potential discrepancies. A spatial map of the basal ice velocity would also be useful in understanding what is going on in the model output.

Reply: Yes, we verified it. The oscillations in Figure 7 and 8 are relevant to Fig 5. The oscillations in basal velocity correspond to those in basal friction coefficient and basal drag. You can see the stripes pattern in both basal shear stress and basal friction coefficient (Fig. 4). Anyhow, we made a spatial map of the basal ice velocity, see the plot below.

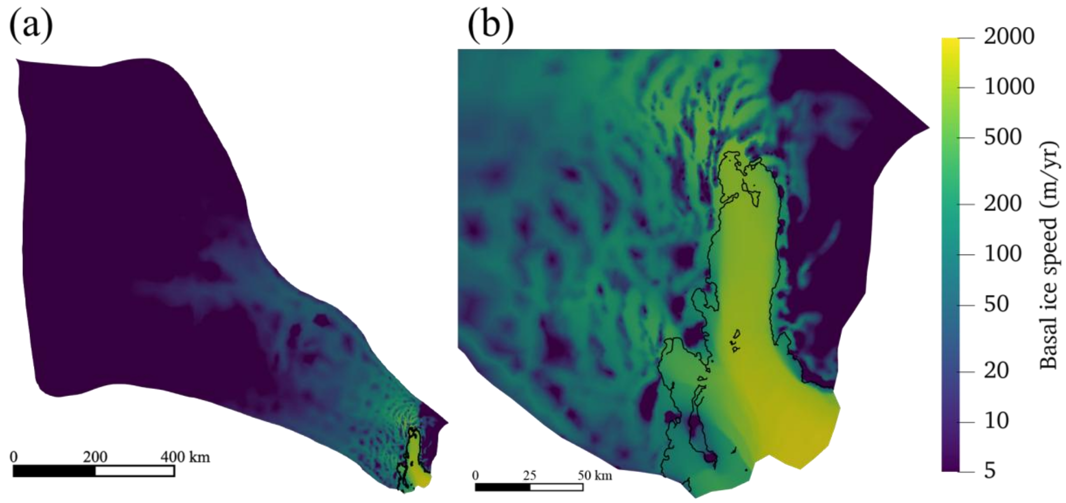

Fig. S6. Modelled basal velocity in the steady state (the initial year 2015).

If this is indeed the behaviour of the full-Stokes model in this region, then I think that the authors need to expand much more on these results. This would also require a physical explanation for the readers to understand what mechanisms could be driving such large rates of vertical shearing in the ice and the large variations in its contribution to the flow over just a few kilometres in the horizontal dimension, without any expression in the overall surface ice speed.

Reply: The ice is stress-balanced by the gravitational driving force, the basal friction and extensional stress divergence term. The gravitational driving force depends on ice thickness and basal slope. It could change greatly where the ice thickness or basal elevation changes dramatically. We infer the basal drag coefficient such that the modelled surface velocity matches the observed. The surface velocity changes smoothly. Hence, the basal drag should change greatly to offset changing gravitational driving force. Therefore, we can see stripes pattern in the inferred basal drag or basal friction coefficient.

We add more words for physical explanation for Fig. 7 and 8 in the revision:

The pattern of difference between the surface and basal ice speed implies high spatial gradients in basal velocity, and reflects the basal drag which must change in response to steep basal slopes or with large variations in ice thickness to balance the gravitational driving force.

Reference:
Morlighem, M., Rignot, E., Seroussi, H., Larour, E., Ben Dhia, H., and Aubry, D.: Spatial patterns of basal drag inferred using control methods from a full-Stokes and simpler models for Pine Island Glacier, West Antarctica, Geophys. Res. Lett., 37, L14502, https://doi.org/10.1029/2010GL043853, 2010.

**Minor Comments:**

Line 92: Can you explain why you expect to gain more information on basal processes from the full Stokes model compared to the range of approximations that are available and open used? It would be good to have more justification for the benefits of your use of full-Stokes given the fact that it limits your experiments to just 35 years.

Reply: Morlighem et al. (2010) compared the inferred basal drag from full Stokes (FS), shelfy stream (SSA) and the Blatter-Pattyn (BP) models, and found that near the glacier grounding line, SSA and BP exhibit a high basal drag (80 kPa), while the basal drag inferred from FS is less than 10 kPa. Therefore, FS is essential to infer a correct pattern of basal drag and to capture all higher order stresses in the grounding line region. They suggest that near the grounding-line of ice streams, treating ice flow with the complete physics of FS is essential.

We change the sentence here 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. Furthermore, full-Stokes models have been suggested as essential to infer a correct pattern of basal drag and to capture all higher order stresses near the grounding-line of ice streams (Morlighem et al., 2010). Therefore, we expect to gain more insight into basal processes using a full-Stokes model.

We also extend the simulations with full-Stokes model to the year 2100 with three different sliding parameterizations in the revision.

Figure 1: Could you show these plots zoomed-in on the area of interest (as in Figure 2 (c) and elsewhere)? As you have a 35-year experiment and only see limited grounding line retreat, much of the model domain is not of interest to your results, and by zooming out the reader loses much of the detail in bed elevation or flow speed that is important.

Reply: We made zoomed-in plots in Fig. 1.

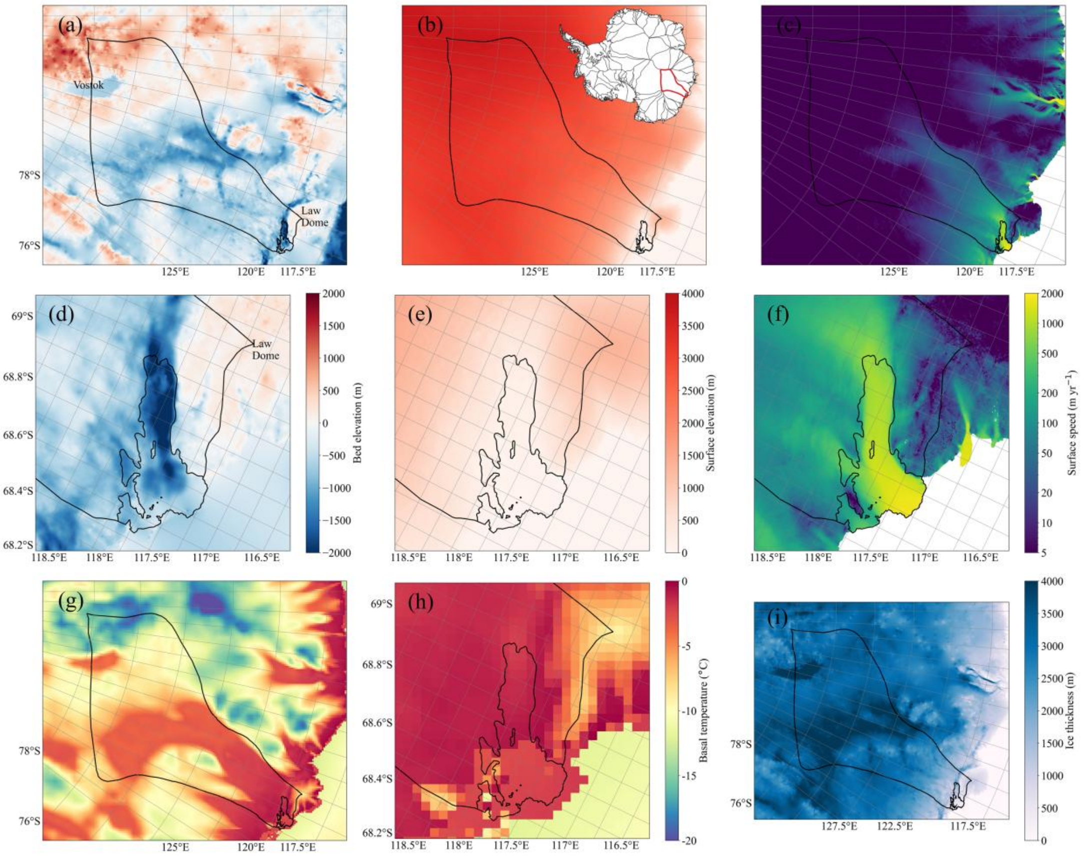

Figure 1. Bed elevation (a), surface elevation (b), surface ice flow speed (c), modelled basal temperature (g) and ice thickness (i) of Totten subregion and its surroundings. The solid black curve is the outline of Totten subregion, from MEaSUREs Antarctic Boundaries for IPY 2007-2009 from Satellite 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).

Figure 2: It's not clear what is gained by showing the inset in panel (b) here, I would consider removing it.
Reply: We removed panel (b).

Line 190: I would be interested to know what impact this choice has on your results, either discussed here or in the discussion section. It seems important given your use of a pressure-dependent sliding law and the impacts you hint at here.

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 effective pressure as 5% of the ice overburden is an order of magnitude smaller than that assuming perfect hydrostatic balance over the far inland region, and at least halves that assuming perfect hydrostatic balance near the grounding line.

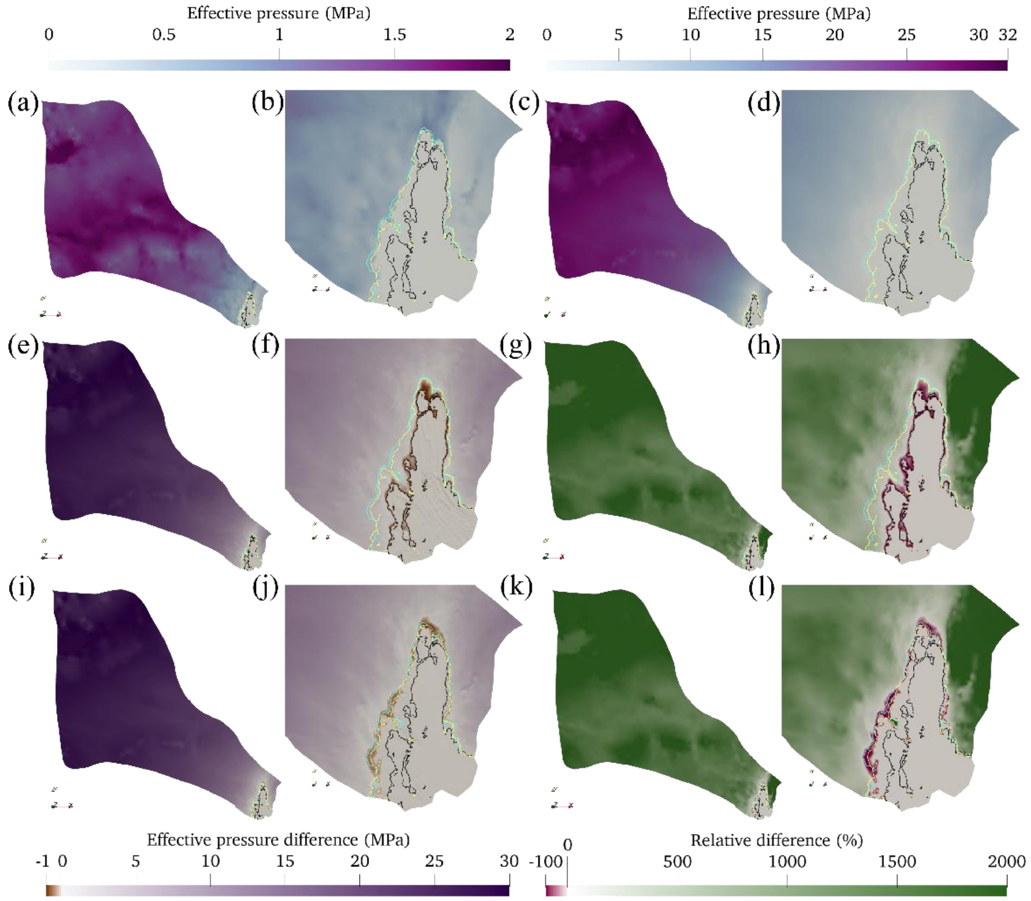

Fig. S1. Effective pressure set to 5% of overburden (a, b) and effective pressure assuming perfect hydrostatic connection and subglacial elevations (c, d) after 35 years simulation. Difference (e, f, i, j) and relative difference (g, h, k, l) between 5% of overburden and perfect connection at the beginning (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 35 year simulations using the two 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).

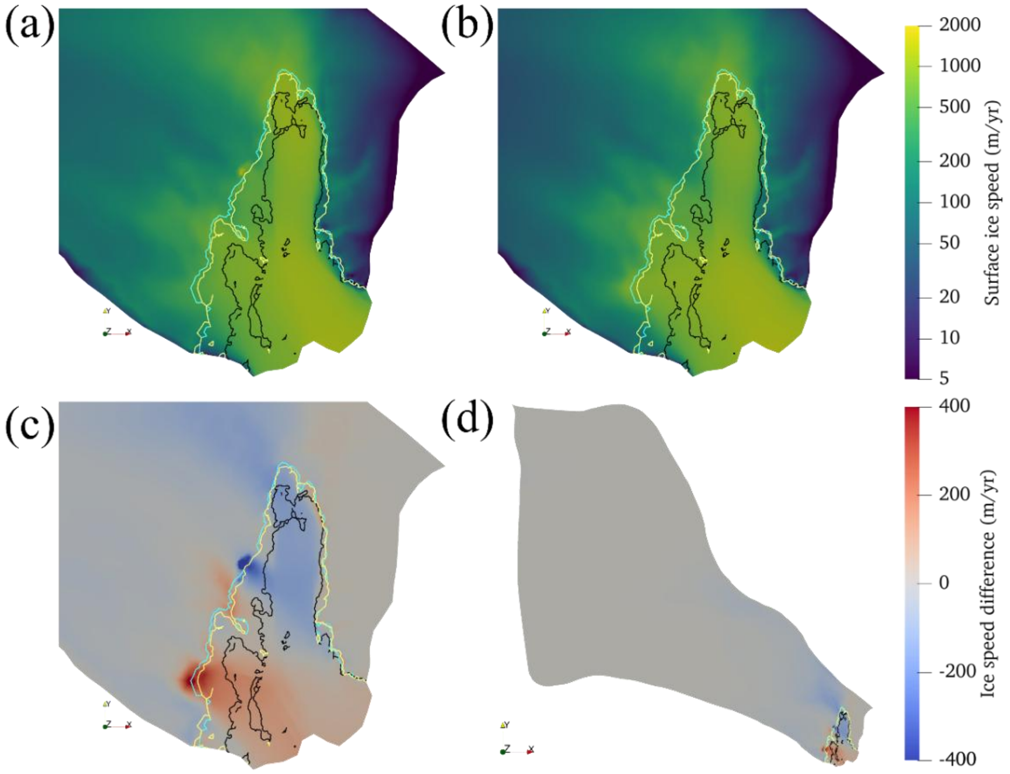

Fig. S5. Surface ice speed using 5% overburden effective pressures (a) and assuming perfect hydrostatic connection (b) after 35 years simulation. Difference (b − a) between (a) and (b) is shown in (c) and (d). The yellow and cyan curves represent the grounding line positions under 5% overburden and perfect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

Line 229: Why do you need to have d0 = 100 in Equation 16? d should always be > 0 on an ice shelf. Even if, for numerical reasons, it can become 0, why use d0 = 100 to correct for that? Did you use (d – 100) as the value for the ice-shelf bottom depth in your linear regression to account for this constant?
Reply: 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.

Line 279: You state that this initial state is representative of 2015, but the data sets used are mosaics whose data collection period spans decades (e.g. your Table 1 shows that the ice velocity is a mosaic of data from 1996-2016). Itim not sure that it is possible to state that your initial state is 2015 without using datasets timestamped to that year – especially for a region which has seen significant changes as you outline in your introduction.
Reply: We checked the user guide of BedMachine Antarctic dataset. It is said:
"The data were collected between 01 January 1970 and 01 October 2019. The nominal year of this data set is 2015—the year of the reference surface digital elevation model."

Line 291: The short timescale of 35 years makes it more important to state the benefits of using full Stokes for these experiments to balance this limitation (see earlier point).
Reply: We extend the simulations with full-Stokes model to the year 2100 with three different sliding parameterizations in the revision. We also addressed the benefits of using full Stokes in the earlier point.

Figure 4: Again, I would prefer to see plots zoomed-in on the region of interest, as in panels (c) and (f) so that the details of the basal sliding and basal shear stress can be seen. The colour scale for the stress enhancement factor colour bar (white around 0) seems different to the one in the maps (grey around 0). Finally, I am not sure of the benefit of plotting the 10 m/a basal speed contours and suggest removing them.
Reply: We show zoomed-in plots for all the variables on the region of interest in Fig. 4. We checked the colorbar of the stress enhancement factor. The color around 1 (we guess you mean 1 rather than 0) is grey. We need the 10 m/a basal speed contours. We define the fast flow region where ice speed >10 m $\text{yr}^{-1.}$

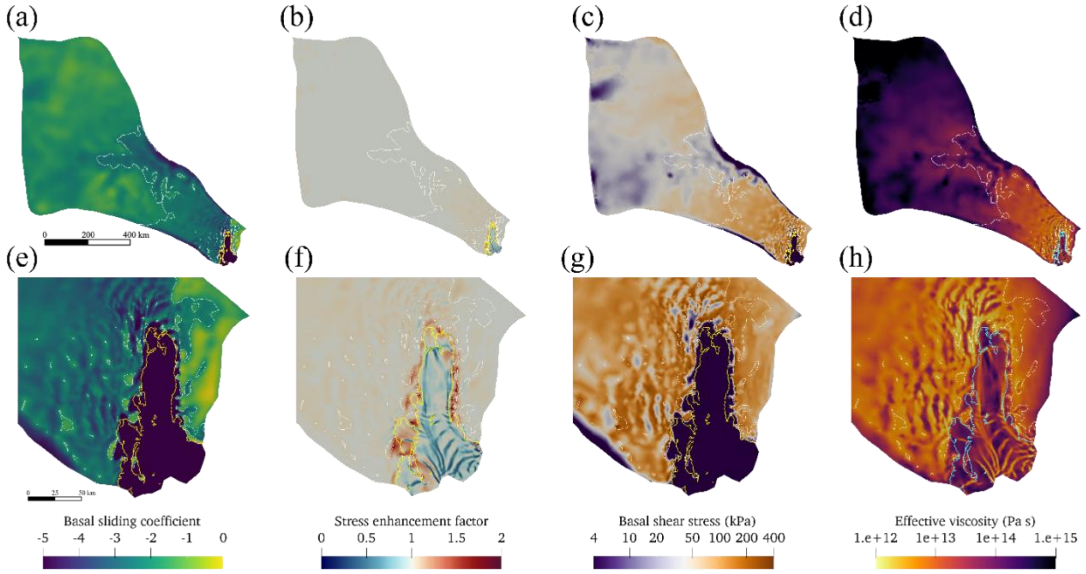

**Figure 4.** Spatial distribution of modelled **(a)** basal sliding coefficient $\beta$, **(b)** stress enhancement factor $E_\eta$, **(c)** basal shear stress $\tau_b$, **(d)** basal effective viscosity, and their corresponding enlarged ones (e-h). The solid yellow (a-c; e-g) or cyan (d, h) curves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

Figure 5: See my main comments here for possible data issues, but if these are genuine artefacts of plotting in Python then they need to be corrected in updated plots.
Reply: We figured out the plotting issue in Python and fixed it. There are no artifacts anymore.

Line 455: In the Results section (Line 366) you stated that the sub-shelf cavity thickness did depend on the basal sliding relation along FL2. This was surprising to me, and I would be interested to know what the physical mechanism linking the upstream conditions at the bed to the thickness of the iceshelf cavity could be, and also the strength of this relationship compared to the much more direct impact of different basal melt rates under the ice shelf. Please clarify this discrepancy here.
Reply: We compare the impact on sub-shelf cavity (or the ice shelf bottom elevation) of basal sliding law with the much more direct impact of sub-shelf melt rates using the old Fig. 7 and Fig. 8 during the same period 2015-2050. The ice shelf bottom elevation is very similar along FL1 and there is slightly difference along FL2 in the year 2050 and more difference along FL2 in the year 2100 (see the updated Fig. 8 in the revision) because the grounding line positions are different. The sub-shelf cavity change is dominated by the sub-shelf melt rate.

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 can see in the updated 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 causes less advection of ice through the shelf, hence thinner ice further downstream in the shelf.

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).