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

Referee 1

Additional comments on 'Sensitivity of Totten Glacier dynamics to sliding parameterizations and ice shelf basal melt rates'.

Dear Yiliang Ma et al,

Thank you for this much improved manuscript, and taking many of our concerns into account. I would like to recognize the hard work (and computational expenses) the authors put in extending their simulations to 2100.

I have some additional minor comments, with the most emphasis on convincing me that your setup does not show, or shows very little, model drift. If there is drift (not unexpected when doing a data-assimilation initialization) I would like to see it quantified and discussed in how it affects your results and conclusions.
Reply: We have carefully considered the issue of model drift. But we do not think it is meaningful to keep the setup unforced for model drift evaluation. We need to force the model with the fixed present-day climate forcings, i.e, SMB and sub-shelf melt rate. We checked the SMB in different years, and found it does not vary much from 2015 to 2100. The estimates for present-day sub-shelf melt rate varies in earlier studies. It seems that our sub-shelf melt rate parameterization with maximal value of 80 m/a is close to the present-day estimates.

Therefore, a drift control simulation is almost the same setup as the reference simulation, C_m80, which is using Coulomb sliding law, SMB and sub-shelf melt rate with maximal value of 80 m/a. So, a model drift simulation is not needed. Furthermore, our paper is more a sensitivity study than future projection, so quantifying drift in the initialized model is not very important.

Ln 34: I would add (Schoof, 2012) as a reference to MISI here
Reply: Done.

Ln 126: You use time-varying SMB fields from CESM2 – CMIP6 ensemble, right? If so, please specify that you use 1995-2100 CESM2 anually varing SMB fields (and, is space allows, include a plot to show what the SMB looks like over the modelled period)
Reply: Yes, we used time-varying SMB fields modelled by CESM2 for the period 2015-2100. We specify 'time-varying' in the revision. There is no space to add SMB plot in the main text. But we add it in the supplementary, also shown as below.

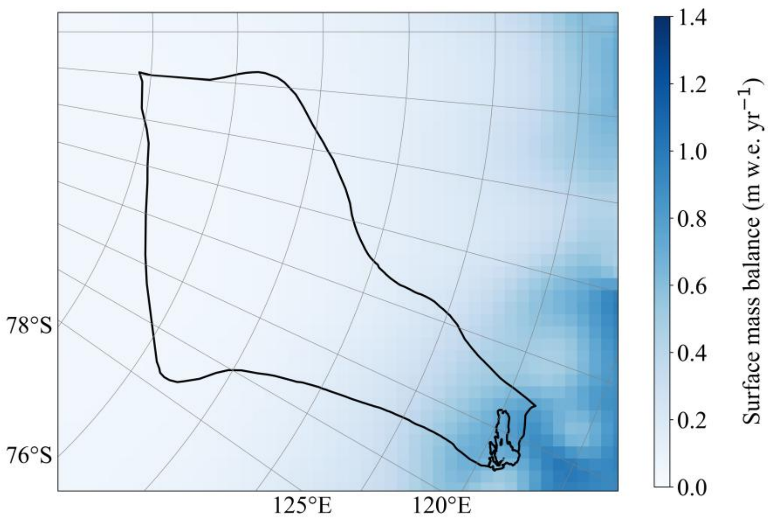

Fig. S1. The averaged SMB field for the period 2015-2100 simulated by CESM2.

Ln 146: ‘Version 9.0.’ should be mentioned before the citation and after ‘Elmer/Ice’
Reply: Corrected.

Ln 154 – 165: I am still seeing poorly-loaded question marks for the symbols used in your equations. I’ve added a screenshot below here. I double checked if this was due to my version of Adobe Acrobat and it was not. Please make very sure during the typesetting process that the symbols are represented correctly.

where $\boldsymbol{\tau}$ is the deviatoric stress tensor, p is the isotropic pressure, ρ_i is ice density, $\boldsymbol{g}$ is the acceleration due to gravity (0, 0, −9.81) m s^{-2}, and $\boldsymbol{v}$ is ice velocity. The effective viscosity, �for the ice-rheology,

$$\boldsymbol{\tau} = 2\eta\dot{\boldsymbol{\varepsilon}}, \qquad (3)$$

given by Glen’s flow law (Cuffey and Paterson, 2010),

$$\eta = \frac{1}{2}(E \cdot A(T))^{-\frac{1}{n}} \dot{\varepsilon}_e^{\frac{1-n}{n}} \qquad (4)$$

7

is dependent on ice temperature via the flow rate factor $A(T)$, defined by an Arrhenius equation. We use an exponent of n=3. $\dot{\varepsilon}$ denotes strain rate and the term $\dot{\varepsilon}_e = \sqrt{tr(\dot{\varepsilon}^2)/2}$ is the effective strain rate. The ice temperature distribution comes from the output of ice sheet model SICOPOLIS (Greve et al., 2020). We introduce ��= �$^{1/(2}$�, to obtain

$$\eta = \frac{1}{2}E_\eta^2 \cdot A(T)^{-\frac{1}{n}} \dot{\varepsilon}_e^{\frac{1-n}{n}} \qquad (5)$$

which ensures $\eta > 0$ for any value of ��. We refer ��to as the 2D vertically constant stress enhancement factor which is set initially to unity everywhere, indicating no stress enhancement, before performing the viscosity inversion. The enhancement factor is often used to account for anisotropy effects (Ma et al., 2010). Values of ��< 1 indicate softer and faster deforming ice, whereas higher values, ��> 1, mean stiffer ice.

Reply: Okay. It looks fine in our pdf. We retype the symbols and hope it can be displayed normally in your computer next time.

Ln 189: try to avoid starting sentences with a parameter symbol. You could change this to 'The parameter A_s….'
Reply: Done.

Ln 189: 'is the sliding parameter without cavitation' does not make it clear to me what the parameter does, what it's value is, or what it's units are. A general comment on this section: consider adding a table at the beginning or end of this chapter where you list all parameters, their units, and their values used.
Reply: We change 'the sliding parameter without cavitation' to 'the sliding parameter in the absence of cavitation', see Gagliardini et al. (2007). Its unit is $m\ a^{-1}\ Pa^{-3}$. We use a complicated formula to calculate it, see Eq. (15). So it is hard to know its value before simulation. According to our simulation results, it has a very wide value range from 10^{-16} to 10^{2} $m\ a^{-1}\ Pa^{-3}$.

We add a table to list all the parameters and their units.
Table 2. List of parameters used in Section 3.2 and their units.

Parameter	Description	Unit
C_{LW}	friction coefficients of linear Weertman	$m^{-1}\ yr\ Pa$
C_{NW}	friction coefficients of nonlinear Weertman	$m^{-1/3}\ yr^{1/3}\ Pa$
C_s	the maximum value reached by τ_b / N	No unit
A_s	the sliding parameter in the absence of cavitation	$m\ yr^{-1}\ Pa^{-3}$
q	friction law exponent, $q \geq 1$, we take $q=1$	No unit

Ln 205 – Ln 210: the rewriting of free parameters in friction laws has been done by (Brondex et al., 2017), please refer to that paper here in this paragraph as well.
Reply: We cite that paper here in the revision.

Ln 210 – Ln 234: The same question marks appear as shown in the screenshot earlier. Please pay special attention when compiling the final manuscript
Reply: Okay.

Ln 265 – Ln 279: I would write this part directly after Ln 244 – Ln 250. In that way, the flow of text would be more logical e.g. 'In shallow water we use…' , 'in deep water we use …' and then 'The WAOM estimates…'
Reply: The WAOM estimates is what we used for the shallow water. We get the point of the referee's suggestion. We reordered the paragraphs to make the shallow water estimate and deep water estimate closer. We move the different estimates from previous studies to

the end of this section, and add a sentence for transition: The modelled sub-shelf melt rates, with varying maximal values, cover most of the estimates from previous studies.

Ln 298 – 315: could you quantify or show if there is any model drift in the final initialization you used? Is this setup robust? I would like to see a line added to Fig 9 representing a simulation starting from 2015 and remaining unforced, or, if you do not want to change your main text too much, see it added to the supplementary materials. I would like to see that your setup is not drifting from 2015 – 2100 and that you can attribute all changes in TG's geometry to changes in basal friction and/or basal melting.
Reply: We think the definition of model drift is the differences over time in the modelled glacier behavior that would occur if we employed present-day climate forcings. For instance, Seroussi et al. (2019) estimated the drift in the control run, in which climate forcing is assumed to be similar to present-day conditions. So, we do not think it is meaningful to run an unforced model drift evaluation. For a drift control experiment, we need to force the model with the fixed present-day climate forcings, i.e., SMB and sub-shelf melt rate. The sub-shelf melt rate applied to the ice sheet can however change due to, e.g., variations in ice extent during the simulation.

We checked the SMB in different years, and found it does not vary much from 2015 to 2100. Our sub-shelf melt rate parameterization with maximal value of 80 m/a is close to the estimates of present-day sub-shelf melt rate.

Therefore, a drift control simulation is almost the same setup as the reference simulation (and the reason we chose it as a reference), C_m80, using Coulomb sliding law, SMB and BMB with maximal sub-shelf melt rate of 80 m/a. Furthermore, our paper is a sensitivity study rather than future projection, so quantifying drift in the initialized model is not as important as the differences between simulations.

Reference:
Seroussi, H., Nowicki, S., Simon, E., Abe-Ouchi, A., Albrecht, T., Brondex, J., Cornford, S., Dumas, C., Gillet-Chaulet, F., Goelzer, H., Golledge, N. R., Gregory, J. M., Greve, R., Hoffman, M. J., Humbert, A., Huybrechts, P., Kleiner, T., Larour, E., Leguy, G., Lipscomb, W. H., Lowry, D., Mengel, M., Morlighem, M., Pattyn, F., Payne, A. J., Pollard, D., Price, S. F., Quiquet, A., Reerink, T. J., Reese, R., Rodehacke, C. B., Schlegel, N.-J., Shepherd, A., Sun, S., Sutter, J., Van Breedam, J., van de Wal, R. S. W., Winkelmann, R., and Zhang, T.: initMIP-Antarctica: an ice sheet model initialization experiment of ISMIP6, The Cryosphere, 13, 1441–1471, https://doi.org/10.5194/tc-13-1441-2019, 2019.

Ln 298 – 315: related to that, it would be good state in this section what happens with the basal melt rate during the initialization procedure of Gladstone and Wang (2022)

and during your 1 year relaxation (I assume they are kept constant). In that way you can state explicitely (related to the previous comment): ‘during our initialization we used basal melt rates from … /shown in supplementary material, and when starting a continuation run we immediately switch to … . Keeping the melt rates and the basal friction constant and running forward to 2100 did not introduce model drift, as shown in Fig (S) …’
Reply: The initialization procedure includes 1) steady state simulation with a basal sliding coefficient from the output of a whole Antarctic ice sheet inversion for the year 2015 (Gladstone et al., 2019); 2) 1 year relaxation; 3) Inversion for basal drag; 4) Inversion for viscosity enhancement factor.

We do not need to prescribe basal melt rate during the steady state simulation or inversions, because they are diagnostic simulations for a fixed ice sheet geometry. We set the basal melt rate equals 0 during the 1 year relaxation because we do not want to change the ice shelf geometry and 1 year is too short to have a large effect.

The basal melt rate we used for surface relaxation is different from that used in Gladstone and Wang (2022). In the surface relaxation simulation of Gladstone and Wang (2022), the basal mass balance under the ice shelf uses the Ice Sheet Model Intercomparison Project (ISMIP6) “local quadratic melting parameterisation”. To avoid confusion, we remove Gladstone and Wang (2022) from the reference in the text about initialization. In the revision, We change to “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 without any sub-shelf melt rate to reduce the non-physical spikes in the initial surface geometry”.

Ln 336: Please avoid starting sentences with parameter symbols
Reply: We add words in front of symbols.

Ln 337: ‘show a rib like pattern’
Reply: Done.

Ln 337: ‘regions’
Reply: Done.

Ln 338: ‘it does not directly give us the cause’.
Reply: Done.

Ln 339: The dot after the references appears as red to me. Is this a track-change left in the document?
Reply: Yes, we change it to black.

Ln 340: I would replace ‘unity’ with just ‘one/1’
Reply: Okay. We change ‘unity value’ to ‘1’.

Ln 342: Xi varies from 0.3 – 0.5 (what units?) and then increases to 0.3 – 0.5? This repetition of values seems like an error to me
Reply: Xi has no unit. What we wrote is: Xi increases to 0.3-5 rather than 0.3-0.5. So it is not repetition of values.

Ln 393: You could also argue that the rib-like pattern is caused by other errors in the ice sheet modelling process and/or by the use of non-coherent datasets, as described by (Berends et al., 2023)
Reply: Thanks for your suggestion. We add the discussion about the rib-like pattern of basal friction in Section 5 of the revision: The rib-like pattern may also arise as compensating errors to other missing processes or uncertainties in datasets (Berends et al., 2023).

Ln 407: ‘might be because’
Reply: Done.

Ln 495: Did you also run with the same 1-year relaxation period?
Reply: The 1-year relaxation was done before the inversion. We ran the Blatter-Pattyn model, restarting from the steady state result (i.e., inversion result) of the reference run using the full-Stokes model. We did not need do relaxation again using the Blatter-Pattyn model.

Ln 510: Cluster specific details are not necessary here in my opinion. Stating that Blatter Pattyn is 15 times faster would be sufficient.
Reply: Okay. We got your point. But it is 5 times faster, not 15 times faster. We change it to: Restarting from the steady state result using full-Stokes model in the reference run, the steady state simulation using Blatter-Pattyn model is 5 times faster than one timestep forward iteration using full-Stokes model with linear Weertman sliding parameterization.

521: Or, the structures arise as compensating errors to other missing processes or uncertainties in datasets, see my comment on Ln 393
Reply: We add the discussion about the rib-like pattern of basal friction in Section 5 of the revision: The rib-like pattern may arise as compensating errors to other missing processes or uncertainties in datasets (Berends et al., 2023).

Ln 571: This is not true, the rewriting was done first by (Brondex et al., 2017)
Reply: We remove the words “give a novel way to”.

References

Berends, C. J., Van De Wal, R. S., Van Den Akker, T., & Lipscomb, W. H. (2023). Compensating errors in inversions for subglacial bed roughness: same steady state, different dynamic response. *The Cryosphere*, *17*(4), 1585-1600.
Brondex, J., Gagliardini, O., Gillet-Chaulet, F., & Durand, G. (2017). Sensitivity of grounding line dynamics to the choice of the friction law. *Journal of Glaciology*, *63*(241), 854-866.
Schoof, C. (2012). Marine ice sheet stability. *Journal of Fluid Mechanics*, *698*, 62-72

Referee 2

I appreciate the additional work that the authors have carried out to address my review comments and to the further comments on those points from the editor.

I am satisfied that my concerns with the interpretation of the model results have been verified and then addressed in the updated manuscript. I think that the figures are improved from the first draft, and the points that needed additional discussion have been developed further.

When published, the differences in the modelled mass loss that the paper demonstrates due to changes in approximation scheme, sliding laws and melt rate forcings will be a valuable contribution to the ongoing discussions and developments in the field.

Reply: Thank you so much for your encouragement and helpful suggestions!