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.
- I am confused by your inversion method. You both metion 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.

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?
- 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.

- Ln 47: How much is 'significant retreat'? Under which climate warming scenarios?
- Ln 60: 'A nonlinear Weertman…' I do not understand this sentence, could you split it up or rephrase?
- 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 coulomb sliding more for faster outlet glaciers such as Pine Island and Thwaites. I believe this is featured in the ABUMIP paper by Sainan Sun.
- 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
- Ln 81: under which warming scenarios?
- 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'.
- Ln 91: what are 'the simplified models'?
- 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.
- Fig 1c: I would suggest to use a logarithmic scale for this plot.
- Fig 1e: Which reference did you use for these drainage basins?
- Fig 1: the purple GL is almost invisible. Also, I would suggest to show the GL for the whole area, not just for Totten.
- 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.
- Ln 137: which inverse method? And what target variable did you use?
- 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.
- 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.
- Ln 172: In Eq 8 the vector **u_b** is used twice. I think the first one should be the magnitude u_b.
- 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.
- Ln 190: In what quantifiable way does this alter your simulations?

- Ln 191: What is a linear Weertman sliding inversion? Are there different inversions per basal friction law?
- 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?
- Ln 220: what is shallow and what is deep water?
- 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.
- 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?
- 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.
- Fig 3: consider adding the observations to this figure as well.
- 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.

- 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.
- Ln 272: ‘the inverse method’. What inverse method? Please specify.
- 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?
- Ln 286: Why is regularized coulomb the most physically sound? Provide references.
- Ln 290: SMB has already been mentioned, consider removing it here.
- 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.
- 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.
- 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.
- 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.
- 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.
- 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.
- 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.
- 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 GL, preventing the thinning at the grounding line.
- 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.

- 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 GL retreat when using a different sliding law, contrasting your abstract.
- Ln 355: Why is this clearly controlled by the topography? I cannot see this in Figure 6.
- 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?
- Fig 6 and fig 7: I see much more grounding line retreat difference in Fig 6 compared to Fig 7, why is that?
- Fig 9: is there a seasonal cycle in your simulations? It shouldn't be because of the simplified melt parameterization.
- 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?
- 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)
- 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.
- 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?
- Ln 447: 'the maximal basal melt rates are'.
- Ln 449: this seems contradictory: is it consistent or are there large differences?
- Ln 452: 'Melt' has a typo
- 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?
- Ln 465: consider publishing your scripts to make the figures and datasets of your simulations as well.