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 Elmer/Ice 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)).

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).

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.

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.

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 often 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.

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.

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

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.

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?

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). I'm 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.

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).

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.

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.

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 ice-shelf 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.