

## Anonymous Referee #1

*Overall: This paper provides a description of the current GRISL1 ice sheet model, focusing on modifications and extensions from the previously described version in 2001. The model is designed for relatively coarse-resolution long-term paleo applications. Main change from 2001 include the specification of grounding-line fluxes (Schoof, 2007), and a basal hydrology model. A large ensemble with Latin Hypercube sampling is used to constrain and calibrate four uncertain model parameters. The paper is clear and provides useful supporting documentation for GRISL1 users and background for other papers on GRISL1 applications.*

Thank you for your time in reviewing our work. In the following we provide a point by point response to your comments. Referee comments are italicised and in orange.

### *Specific comments:*

*1. There is considerable scatter in Fig. 7, showing pair-wise parameter correlations with the results (here, rms error in modern ice thickness). This may be because the Latin Hypercube (LHC) sampling of the large ensemble (LE) may be too coarse to meaningfully detect pair-wise dependencies. The quasi-random distribution of red, blue and green stars in these panels is reminiscent of corresponding figures in Applegate et al. (2012, The Cryo., their Fig. 1). Chang et al. (GMD, 2014) subsequently found that the scatter in the Applegate study is due to inadequate sampling in the high-dimensional parameter space, and they used additional statistical analysis with Gaussian emulation to extract meaningful dependencies (their Fig. 4a vs. 4b). That study had a similar number of parameters (5) and ensemble members (100) as here (4 and 150). In a similarly sized Antarctic LE, Pollard et al. (GMDD, 2016) found that meaningful dependencies could only be found with "full-factorial" sampling, i.e., a run for every possible combination of parameter values, requiring  $5^4 = 625$  runs for 4 parameters and 5 values each. If that many runs could be performed here, it might yield much more meaningful pair-wise results than in Fig. 7. However, if that would be too computationally expensive, it could be left to future work, and the above caveats could just be noted.*

Encouraged by your concern we doubled the size of the ensemble. We now have 300 members for each formulation of the flux at the grounding line (600 members in total). Our ensemble is now considerably larger than the one of Applegate et al. (2012) considering that we have 4 parameters instead of 5. In addition, we have added the RMSE information in some figures (former Fig. 4 and Fig. 7) in order to facilitate the emergence of relationships. With this larger ensemble, the RMSE in the parametric space (new Fig. 7 and Fig. 8) mostly confirms what was suggested in the first version of the manuscript: there is generally no clear relationship between our parameters that can explain the RMSE with the major exception of the variable  $E\_SIA$  versus  $C\_f$  for simulations using Schoof et al. (2007) and  $E\_SIA$  only for simulations using Tsai et al. (2015). From this, we suspect that to obtain meaningful dependencies we should in fact probably expand drastically the ensemble, probably by doubling again its size (order of magnitude from  $10^2$  to  $10^3$ ). This is beyond the scope of the manuscript but we acknowledge the fact that emulators trained with very large ensemble are a very promising field of applications for large scale ice sheet model.

*2. The simulations here use uniformly prescribed basal drag coefficients, and do not use an inversion method to deduce a spatial map. There is discussion on the pros and cons (pg. 6, 15, 17), which makes good points for not using inverse methods. But it does not mention the primary motivation (I think) for using them: that without them, modern errors in ice thickness are much larger (as in Figs. 5,6), and can be made much smaller using an inverse procedure. Since these*

*errors are the primary metric here for evaluating the model, this could help to make the calibration of model parameters more meaningful. I think the whole issue depends on whether the inverse-produced map captures real bed variations at all, or if it just cancels with and obscures other physical errors in the model. I suggest mentioning this within the existing discussion. Also, the point made on pg. 17, line 16, on the desirability of making basal coefficients a property solely of internal model parameters (such as  $N$  here, in Eq. 14), is debatable: apart from basal temperatures and water amount of course, spatial variations in basal sliding can also depend importantly on geologic bed type, roughness, and the distribution of deformable till, which are outside the scope of the model.*

The simulations in this paper do not use uniformly prescribed basal drag coefficients as they are computed from the effective water pressure that varies both in space and time (Eq. 16). This is precisely the capability of this formulation to respond interactively to changes in geometry that motivated our choice over an inverse method.

Using an inverse method to infer the basal drag coefficients would not help in calibrating the model parameters because, by construction, the inverse method will correct any bias in both the forcings (climate/bedrock) and the model physics. For example, for different enhancement factor values we can, in principle, infer different basal drag coefficients maps that will result in close-to-observation geometry.

We have nonetheless added a sentence on the pros of inverse methods (Sec. 2.1.3):

“Inverse methods are particularly adapted to produce an ice sheet state (e.g. geometry and/or velocity) close to observations. However, such methods do not provide [...]”

About a basal drag computed from internal variables only, you are right mentioning the importance of bed properties such as geologic bed type and roughness. These are largely uncertain both in term of their values and in term of their impact on ice dynamics. We have slightly modified these lines:

“A step forward would be to use the basal drag computed from inversion in order to deduce a formulation based solely on internal parameters. Amongst these parameters, along with the basal effective pressure, the large scale bedrock curvature and/or sub-grid roughness could be used, similarly to Briggs et al. (2013). However, some key basal features, such as the geologic bed type and the deformable till distribution, remain today largely unknown below present-day ice sheets and will contribute to large uncertainties in the basal drag formulation.”

*3. In Eq. 14 on pg. 6, and section 2.2.1 (Eqs. 24-26, pg. 9), it is not clear how some variables for basal hydrology are determined:  $h_w$  or  $p_w$  (which are related, line 24), and effective pressure  $N$  needed for Eq. 14. Presumably there is a prognostic equation in the hydrology model for  $h_w$ , i.e.,  $d(h_w)/dt = \dots$ , that is not shown here. Perhaps it is the equation mentioned on pg. 9, line 23. Also  $N$  possibly depends on  $p_w$ . This information, and the equation for  $h_w$ , should be included. (Incidentally, if  $N$  depends on depth below sea level as in several other models, I would question how can it reasonably depend on that, at distances 10's or 100's km inland from the grounding line).*

We acknowledge that the original description of the hydrology was somehow incomplete and we have considerably rewritten this section with clarity in mind. The prognostic equation for the hydraulic head  $h_w$  is now presented. We also explicitly mention how we compute  $p_w$  and  $N$  from  $h_w$ .  $N$  does not depend explicitly on the depth below sea level as it is computed as the difference from the ice load pressure ( $\rho_i g H$ ) and the basal water pressure ( $\rho_w g h_w$ ). However, the

depth below sea level is a necessary boundary condition for the hydraulic head at the marine ice sheet margin ( $h_w = \text{sealevel} - B_{bed}$ ).

*4. The determination of buttressing factor  $\phi_{bf}$  in Eq. 15 (pg. 7) is an important part of the use of the Schoof flux equation, but the procedure is unclear to me from lines 21- 25 on that page. Perhaps the first solution provides the back-stress-free solution...does that solution use Eq. 15 with  $\phi_{bf} = 1$ ? Then what is the second solution, and where does its value of  $\phi_{bf}$  come from? These questions may not make sense, and just show my confusion. Hopefully this paragraph can be clarified, and perhaps expanded if that would help.*

We acknowledge that it was not clear in the first version of the manuscript. In fact, in our framework we compute the velocity equation three times with the two first iterations being used to compute  $\phi_{bf}$ . The first iteration is computed on the simulated geometry with no flux adjustment at the grounding line. The second iteration is computed the same way, except for the fact that the ice shelves are assigned to a very low viscosity so that they cannot exert any back force. The buttressing ratio  $\phi_{bf}$  is then computed as the velocity ratio between these two computed velocities. The flux adjustment at the grounding line is only applied for a third iteration which gives us the actual velocity field. We have made this clearer in the manuscript:

“To evaluate the back force coefficient  $\phi_{bf}$ , we solve the velocity equation twice. The first iteration is computed on the simulated geometry with no flux adjustment at the grounding line (i.e. not using Eq. 17 nor Eq. 18). The second iteration is computed the same way, except for the fact that the ice shelves are assigned to a very low viscosity so that they cannot exert any back force. The buttressing ratio  $\phi_{bf}$  is then computed as the velocity ratio between these two computed velocities. Once  $\phi_{bf}$  is estimated, we solve the velocity equation again, this time accounting for the flux adjustment at the grounding line using Eq. 17 or Eq. 18, in order to estimate the velocity used in the mass conservation for this time step.”

We acknowledge for the fact that the two first iterations produce unrealistic simulated velocities as they do not account for any specific treatment at the grounding line. However, we assume that the ratio in velocities is representative for the buttressing effect of ice shelves.

*5. pg. 16, lines 5 to 7: Perhaps, the timings of the deglacial retreat in AN40T vs. AN40S can be assessed vs. papers in the RAISED reconstruction volume (Bentley et al., 2014), or other data, in order to determine which one is more realistic. The paper seems to decide rather arbitrarily that the AN40T case is more realistic (pg. 18, line 25).*

The fact that the model is still drifting at +10 kyr in the future with AN40S is a clear indication of a too slow retreat in this case. However, it is true that our climatic forcing is relatively simple and that with an alternative climate forcing we could maybe have a faster retreat with AN40S. Keeping that in mind, we have moderated this sentence:

“This suggests that, in our model and under the climate forcing scenario we use, the Tsai et al. (2015) formulation produces a more realistic grounding line retreat rate. ”

There is unfortunately no archive that allows for an ice volume change reconstruction of the Antarctic ice sheet during the last deglaciation. While, the RAISED reconstructions do not quantify the change in ice volume, it is indeed nonetheless, at present, the most complete data compilation on the extent of the grounding line during the last deglaciation. However, the temporal resolution (5 kyr) together with the fact that the largest uncertainties remain in the Weddell and Ross seas, make

it difficult to compare with our model results. This is why although we discuss the RAISED reconstructions in the original version of the manuscript for the ice extent at the last glacial maximum, we did not use this as a constraint for the timing of the deglaciation.

*6. pg. 13, lines 9-10: The pairs of values "1.5 to 3" and "1.5 to 5" do not seem to relate to the bottom-right panels in Figs. 3 and 4, for basal-drag coefficients  $K_0$  (which are being discussed in that sentence). They seem to relate better (but still fuzzily) to the bottom-left panels for enhancement factors  $E_{sia}$ .*

The initial formulation was misleading as we were indeed referring to the enhancement factors. We reformulated as:

“As a consequence, for the AN40T ensemble, the enhancement factor requires values between 1.5 and 3 in order to reach a good agreement with observed ice thickness, whilst values within 1.5 to 4 are acceptable for AN40S.”

Technical comments:

*The English usage is generally good, but isolated words or phrases could be improved/corrected, some of which are noted below.*

*pg. 1, line 19: Change "are evidences" to "is evidence".*

Done.

*pg. 1, line 23: Change "An other" to "Another".*

Done.

*pg. 2, line 7: I think "prograde" should still be "retrograde", for MICI as well as for MISI.*

We acknowledge the fact that a retrograde slope will inevitably amplify both the MISI and MICI. However, as postulated by Pollard et al. (2015) and contrary to the MISI, the MICI can also occur on neutral and prograde slopes. We clarified this sentence:

“Additional instabilities may also occur on neutral/prograde bed slopes in relation with the structural instabilities of tall ice cliffs (marine ice cliff instability, MICI, Pollard et al., 2015).”

*pg. 2, line 23: The word "diffusion" should probably be removed (?).*

Removed.

*pg. 3, line 14, and several later places: "Tab. 1" should perhaps be "Table 1".*

The GMD manuscript preparation guidelines for authors suggest to use abbreviations (Sec., Fig., Eq., Tab.) when used in running text unless it comes at the beginning of a sentence. We have followed these guidelines consistently.

*pg. 4, line 1: The use of two "respectively"'s in the same sentence is confusing - perhaps divide into 2 separate statements for  $\sigma_i$  and  $\tau_{ij}$ .*

It has been changed to:

“[...] where  $\tau_{ij=x,y,z}$  are the shearing stress tensor terms and  $\sigma_{i=x,y,z}$  the longitudinal stress tensor terms, defined as ( $i=x,y,z$ ):  
 $\sigma_i = \tau_{ii}$ ”

*pg. 4, line 26: Change "Alike" to "Like".*

Done.

*pg. 4, lines 27-29: The word "reduces" in line 27 seems to contradict the word "favour" in line 29. Or perhaps "longitudinal" should be "shearing" in line 29 (?).*

Thanks for noticing, it was effectively a mistake. This part has been expanded and reformulated.

*pg. 5, line 24: What does "see also numerical feature" mean?*

Changed in the text to:

“[...] see also Sec. 2.3 on the numerical features”

*pg. 16: I would suggest emphasizing, as a positive note, that if the (interpolated) grounding line position is known, then all that is required to obtain ice thickness  $H_{gl}$  at the grounding line for Eq. 15 is (1) bedrock bathymetry interpolated to the grounding line position, and (2) sea level. (This is because of the floatation criterion at the grounding line of course).*

The sub-grid position of the grounding is known because we linearly interpolate the floatation criteria based on the knowledge of thickness, bathymetry and sea level on the centred GRISLI grid. For more clarity for the reader, we have added a schematic representation of the staggered grids (Fig. 2) and expanded the description of how we apply the grounding line flux on the velocity nodes.

*pg. 10, line 23-24: Explain the need for the artificial extension (to get an ice front parallel to x or y).*

With this extension, the front of the ice shelf is always parallel to either x or y which facilitates the application of boundary conditions. This is now explicitly stated in the manuscript. A schematic representation of the different cases for the elliptic equation is now shown in Fig. S1 of the supplementary material.

*pg. 11, line 20: Misspelled "projet".*

Corrected.

*pg. 12, line 15: "in 150" should be "of 150".*

Changed.

*pg. 12, line 20: Perhaps change "are discarded from" to "are not included in" ?*

It has been changed to “are not explored in”.

*pg. 14, line 6: Change "somehow" to "somewhat".*

Done.

*pg. 15, line 16: Does this mean that a 100-kyr long spinup is performed with perpetual modern climate for every ensemble member? If so, say that more clearly.*

All the ensemble members (300x2) in Sec. 3 are 100-kyr integrations of the model using perpetual modern climate (p.11 l.16-17 in the initial manuscript). The spun-up ice sheet used for the transient simulations is the final state obtained at the end of the 100-kyr integration in Sec. 3. For sake of clarity, we reformulate as:

“We used the 100-kyr integration under perpetual modern climate in Sec. 3 as a spin-up for the transient simulations.”

*pg. 15, line 24: The range of 10 to 20 m eustatic sea level drop here is actually a bit larger than several recent model studies. This might be due to larger basal drag coefficients used here on modern continental shelves, so when grounded ice expands onto them at LGM, the expanded ice is thicker there.*

The reviewer is perfectly right here. During glacial periods, the ice sheet expands onto part of the continental shelf that presents presumably different bedrock conditions. In particular, we can expect to find more deformable till relative to hard bed in these areas, facilitating the ice flow for large part of today ice-free regions. For these reasons, some authors choose to use a two-value basal sliding coefficient for hard bedrock (bedrock above sea level) and deformable sediments (bedrock below sea level) (e.g. Pollard and Deconto, 2012). Because geologic information below present-day Antarctica is poor, we have preferred to keep our approach as simple as possible with no additional tuning. However, for future work it is clear that sensitivity studies on the role of bedrock characteristics will have to be performed.

Following your comment, we have added the following in the manuscript:

“Our reconstructions are nonetheless at the higher hand of recent studies. This could be related to the fact that we do not account for different geologic bed types between today ice-free (with extensive amount of deformable till) and glaciated (mostly hard bed) continental shelf. To account for this, some authors have chosen a two-value basal drag for these different regions (e.g. Pollard and Deconto, 2012). Because of the large uncertainties related to the bed properties we have decided to ignore these differences, keeping in mind that this can bias our results towards thicker ice sheet when the ice expands over the continental shelf.”

*pg. 16, line 4: Change "In turns" to "In turn".*

Done.

*pg. 17, line 2: "using an inverse method" sounds like one is used here. Make it clear that one is not, and that phrase refers just to the references earlier in the sentence.*

This now reads:

“We have presented results from the updated version of the GRISLI model. Whilst the model is able to reproduce present-day Greenland (Le clec’h et al., 2017) and Antarctic (Ritz et al., 2015) ice sheets when using an inverse method to estimate the basal drag, our simulations with an interactive

basal drag computed from the effective pressure show some important disagreements relative to observations.”

*Table 1: Some of these variables do not seem to be used in the text, e.g., those under "Deformation". Others may have a different name, e.g., h\_till.*

We have now given more information on the computation of the viscosity in the model and we refer explicitly to the variables listed below “deformation”. We have checked the table carefully and corrected a few mistakes. If some of the variables here are effectively not used in the text, in particular the ones for the isostatic rebound model, we have nonetheless preferred to keep them here for documentation of this particular version of the model.

*Fig. 2: The relationship between the sector boundaries (left-hand panel) and the contour divisions for basal melt rates (right-hand panel) is confusing, not as one might expect. That is, there seems to be some divisions between the colors in the right-hand panel that are not present in the left-hand panel, and vice-versa.*

We imposed a specific (high) sub-shelf melting rate for the deep ocean (depth greater than 2500 m). This is why one region (deep ocean) of the right-hand panel does not appear on the left-hand panel. We now explain this in the text:

“Sub-shelf melting rate for the deep ocean (depth greater than 2500 m) are assigned a value of 5 m/yr.”

Also, if some sectors that appear on the left-hand panel do not appear on the right-hand panel this is because they have the same or similar sub-shelf melting rates and cannot be distinguished.

## References

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