**Anonymous Referee #3**

*Summary: This paper describes a new version of the GRISLI ice sheet model, including new model development since an earlier version many years ago (Ritz et al., 2001). While there are no major scientific advances relative to the state-of-the-art manifested with this update, documenting the current state of the model and the individual progress that has been made is well appreciated. The paper is generally well written and clear. Nevertheless, I believe there could be more detail given in some of the descriptions to make it a better reference and make the paper more accessible for other modellers. I consequently recommend publication in GMD with some corrections as detailed below.*

Thanks for your encouraging comment. We provide a point by point answer to your concern in the following.

*General comments:*

*I believe it is a good practice to (regularly) publish model description papers like the present one, to document the applied models, increase transparency and allow for other modellers to learn, improve upon and critically evaluate the applied techniques. One point I find regrettable with the present paper is that the authors seem to not plan to publish the model code alongside with the manuscript. I know that this may not be common practice in our community, but I believe it would be an important step forward. I would applaud if the authors would think about how to make the code publicly available, possibly with certain restrictions.*

We agree with the reviewer on this, however in practice it is not that trivial to make GRISLI code published publicly. The public distribution needs to be done with a specific license (e.g. GNU public licence). Such a license is currently lacking in GRISLI. The model has been developed from the 90ies and has benefited from the additions from numerous contributors. To put a license to the current model, we have to get the permissions to all the contributors from the past 20 years. To date, we do not have this permission from all the contributors. However, as stated in the manuscript, any potential users are encouraged to get in touch with C. Dumas, A. Quiquet or C. Ritz, to start a collaboration.

*While the applied modelling techniques are mostly well described in words and equations (textbook style), the numerical implementation is often not possible to determine. I invite the authors to make an additional effort to increase the precision. This is even more important when the model code cannot be consulted. The ultimate goal should be that someone who does not know the model would be able to implement a specific feature from the given information. See also specific points below.*

In the revised version of the manuscript, we have expanded on the different subjects: 1) the description of the polynomial flow law and the link between temperature and viscosity; 2) the computation of the flux at the grounding line; 3) the structure of the grids. In addition, we have given more information on the numerical resolution of the equations in the “numerical features” section (Sec. 2.3).

*Reference to textbooks and earlier works is mostly in order. However, in some cases it would be useful for the reader to have some additional (’meta’) information for the specific descriptions. E.g. who else is using the same technique, or does the applied technique represent a notable difference/novelty compared to other models used in the community. If other approaches exist, do you have reasons to choose this approach compared to another and why (simplicity, better results,* *tried other approach but didn't work ...). Again, the motivation should be to make the paper a useful reference and interesting resource for another modeller trying to implement a specific feature.*

Because the novelties were not presented in a sufficiently clear way in the original manuscript, in the revised version, we have systematically referred to the similarities and differences from the previous model description (Ritz et al., 2001). We also have referred to the similarities to other models (SSA velocity as a sliding law, temperature computation, particle tracking scheme, etc.).

*Specific comments:*

*P1 L14 Which time scales or time scale range, be more specific.*

From diurnal to multi-millenial. Added in the text.

*P1 L15 "surface albedo" is not a feedback, nor "freshwater flux". Please clarify.*

Changed for: “[...] through multiple feedbacks such as temperature - surface albedo, gravity waves and oceanic circulation changes related to freshwater flux release.”

*P1 L18 Not sure about the connection between the two sentences implied here.*

Changed for:
“If the two major ice sheets have been mostly stable for at least the last 1000 years, their contribution to global sea level rise in the future is largely uncertain. Conversely, in the past, there is evidence of sea level rise as fast as four metres per century (e.g. Fairbanks, 1989; Hanebuth et al., 2000; Deschamps et al., 2012).”

*P1 L21 I would suggest rewording to avoid drastic terms "rapid" and "destabilisation".*

We agree with the reviewer that such terms are often used deliberately in inappropriate context. However, our previous sentence directly refers to the melt water pulse 1A event, which leads to abrupt sea level rises as high as 4 m within a century. This event can certainly be qualified as “rapid” and is induced by an ice sheet destabilisation.

*P1 L22 "The surface mass balance-height feedback has ..."*

Thanks, we have followed your suggestion.

*P2 L1-3 Not all bedrock in the Antarctic shows a retrograde slope. More precision needed.*

We have added “[…] is related to the fact that large parts of the bedrock presents a retrograde slope [...]”

*P2 L4 MISI driven retreat does not have to be very fast. Suggest removing "fast and"*

Done.

*P2 L7 Add "ice" before "cliff"*

Done.

*P2 L7 Buttressing already decreases when the ice shelve is removed and a reason for ice cliffs to fail. Reformulate.*

True, we have reformulate as follow: “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)”

*P2 L10 What is the range of temporal and spatial scales, specify.*

Changed to:
“Because they include processes operating on variety in temporal, from diurnal to multi-millenial, and spatial scales, from a few metres to thousand of kilometres, [...]”

*P2 l10 "temporal and spatial scales" of what exactly?*

Now specified, please see our response to your previous comment.

*P2 L14 There are also state of the art models that are not FS, clarify.*

In this sentence, the term “state of the art” has been replaced by “the most comprehensive”.

*P2 L19 Remove "Conversely".*

Done.

*P2 L21 add "e.g." before Hindmarsh*

Done.

*P2 L23 Reformulate "temperature and surface mass balance perturbations diffusion"*

We removed the word “diffusion”.

*P2 L26 New sentence with "GRISLI was in the late nineties ..."*

We have done so, thanks for the suggestion.

*P3 L4 Not only MISI, but also GL movement in general. Reformulate.*

We agree and simply replaced MISI by grounding line migration.

*P3 L15 "2.1 Ice thermo-mechanics". I didn’t find the "thermo" aspect in this section. Reword?*

Section 2.1.6 deals with the temperature computation in GRISLI and the viscosity presented in Sec. 2.1.1 is temperature dependent. The expression of B_AT (and its temperature dependency) is now presented.

*P3 L21 Add equation number after "mass conservation equation".*

Added.

*P3 L23 "the vertically integrated velocities in x and y-direction u_x and u_y".*

Done.

*P4 L1 "where sigma ... and tau ... are the longitudinal and shearing stress tensor terms, respectively".*

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"

*P4 15 Add an equation, explanation or reference how B_AT is calculated.*

We acknowledge that this information was missing in the original paper. We have added its equation.

*P4 L27-28 SIA and SSA are not yet defined.*

This part has been re-written.

*P5 L5 "horizontal derivatives" and "vertical derivatives" of what? Clarify.*

Of velocity, information added.

*P6 L16 How is the water pressure defined? Add equation or reference.*

The hydrology model presentation has been largely expanded, including water pressure definition.

*P6 L25 So "beta = C*N" for temperate ice. What is assumed for the rest? Clarify.*

As mentioned in P5L26 of the original manuscript, for cold-based points we imposed a large basal drag that ensures no-slip condition. In addition, sliding velocities are set to 0 in this case. We have added this last precision in the manuscript:
"For cold-based grounded ice we impose a large enough basal drag (typically $10^5$ Pa) to ensure virtually no-slip conditions on the bedrock and the basal velocity is set to zero in this case."

*P6 L25 I expected to get some information on how C_f is calibrated in this paragraph, maybe just a list of options that could be used, then you get back to that later.*

At the end of the paragraph we have added:
"The Cf parameter will be part of the calibrated parameters in our large ensemble."

*P6 L29 Avoid use of "flux correction" as it has a specific meaning in coupling climate models. Use e.g. "flux calculation" as on top of the next page.*

Done.

*P7 L18-19 Here it would be good to already know how the grid is laid out. Where is the velocity defined compared to the ice thickness nodes. You later state that you are using a "staggered Arakawa C-grid", but it may not be clear to all what that implies. Could you add a clear description or figure where the different quantities are defined? This would also help in the following to explain the numerical implementation of certain schemes.*

We have added a figure presenting the horizontal staggered grids (Fig. 2), with a description of where the variables are calculated.

*P7 L19. So the flux is imposed on two grid points in both x and y direction and applied there simultaneously? More precision is needed to make clear how to implement this.*

We acknowledge that the description of the flux computation at the grounding line was somehow incomplete in its description. Relying on the new Fig. 2, we have explained better how the flux computed at the sub-grid position is affecting the actual velocity nodes. This now reads:
“In GRISLI, from the last grounded point in the direction of the flow, we compute the subgrid position of the grounding line in the x and y directions linearly interpolating the floatation criterion (dark green dots in Fig. 2). From this position, the flux at the grounding line is calculated using Eq. 17 or Eq. 18 (red arrows in Fig. 2). Because the flux at the sub-grid position is perpendicular to the local grounding line, ideally we should project this flux onto the x and y-axis. However, in the model, we assume that the grounding line is always perpendicular to either the x or y-axis (dashed brown line in Fig. 2). Similarly to Fürst (2013), the value of the flux qgl is linearly interpolated to the two closest downstream and upstream velocity grid points (dark blue arrows in Fig. 2) using the two bounding velocity points (light blue arrows in Fig. 2).”

*P7 L22 Wouldn’t this give back force at the places where velocities are calculated, not at the GL where it is needed?*

Yes, the reviewer is perfectly right here. Unfortunately it is not obvious to infer the back force at the grounding line. For example, Pollard and DeConto (2012) and Pattyn (2017) evaluate the back force using the longitudinal stress at the first downstream point.

*P7 L27 Replace "Ice front" by "Iceberg" in front of "calving".*

Done.

*P8 L1 The section header states "Temperature coupling". There is no description of coupling in this section, only how the temperature is calculated. Maybe change title to "Thermodynamics" or "Ice temperature calculation".*

You are right. We have reformulated to “Ice temperature calculation” as suggested.

*P8 L2 Could give a sentence of introduction to state that this is the classic way to solve thermodynamics and similar to many other models (references). Or is there anything special here that I have overlooked?*

The referee is right here, this equation of advection-diffusion is common to most ice sheet models. The introductory sentence reads:

“Similarly to most large-scale ice sheet models (Winkelmann et al., 2011; Pollard and DeConto, 2012; Pattyn 2017), the temperature in GRISLI is computed by solving the general advection-diffusion equation of temperature: [...]”

*P8 L2- Consider to give some indication on how all of this is solved numerically. How are the differential equations discretised? Which numerical schemes are used for advection and diffusion (upwind, second order, Lax)?*

We have added this information in Sec. 2.3:
“For the temperature equation (Eq. 19), we solved a 1D advection-diffusion equation for each model grid point. The resolution is performed with an upwind semi-implicit scheme (vertical velocity and heat production at the previous time step is used). The ice thermal conductivity is computed as the geometric mean of the two neighbouring conductivity (Patankar, 1980). Because the horizontal diffusion is neglected, the only horizontal terms concern horizontal advection and are computed with an upwind explicit scheme. The heat production is computed at the velocity (staggered) grid points and is then summed up to the temperature (centred) grid points.”

*P10 L12 Hardly any information is shared on how the given equations are solved numerically. I believe it would make the paper a much more interesting reference for other modellers and even people in your own group if some details would be added on the practical side of the modelling.*

In the revised version, we now explicitly show the staggered grid in Fig. 2. We have also added two additional figures in the supplementary material showing: i) the boundary conditions when extending the front to the edges of the geographical domain and ii) the matrix used to solve the elliptic equation. We have also considerably expanded Sec. 2.3, adding more information on the numerical resolution of the equations:

“The mass balance equation is solved as an advection-only equation with an upwind scheme in space and a semi-implicit scheme in time (velocities at the previous time step are used). The numerical resolution is performed with a point-relaxation method with a variable time step. The value of this time step is chosen to ensure that the matrix becomes strongly diagonal dominant to achieve convergence of the point-relaxation method. The criteria is thus a threshold that is inversely proportional to the fastest velocity on the whole grid. Note that this smaller time step is solely used for the mass conservation equation and subsequent variables (e.g. surface slopes, SIA velocity) while the rest of the model uses a main time step, typically ranging from 0.5 to 5 years depending on the horizontal resolution.

To solve it, the ice shelves/ice streams equation (Eq. 14) is linearised. The viscosity is computed using an iterative method starting from the viscosity calculated from strain rates from the previous time step. As this equation is the most expensive part of the model, the iteration mode is not always used depending on the type of experiment (for instance not crucial when the objective is to reach the steady state). In this case the viscosity of the previous time step is used. The linear system is solved with a direct method (Gaussian elimination, sgbsv in the Lapack library (www.netlib.org/lapack)).”

*P10 L13 More precision is needed to understand what variables are defined where on which (staggered) grid, also in the vertical. How is the vertical grid laid out? Is the order up-down or down-up? Is the first vertical grid point from the top where T is solved assumed at the boundary or representing the middle of a first layer? How is that at the base?*

We have added a figure that presents a schematic representation of the horizontal staggered grids used in the model (Fig. 2). The direction of the vertical axis z is shown in Fig. 1 (pointing upward). In addition, we have added this information in the section on the numerical features:
“The model uses finite differences computed on a staggered Arakawa C-grid in the horizontal plane (Fig. 2). In the vertical, the model defines σ-reduced coordinates, $\varsigma=(S-z)/H$, for 21 evenly spaced vertical layers, with the z vertical axis pointing upward and $\varsigma$ downward (0 at the surface and 1 at the bottom). The coordinate triplet $(i,j,k)$ (in x, y and $\varsigma$ direction) is representative of the centre of the grid cell.”
The first vertical layer ($\varsigma=0$) has its temperature in equilibrium with the annual mean surface air temperature (Dirichlet boundary condition). In turn, the last vertical layer ($\varsigma=1$) is either at the melting point (Dirichlet boundary condition) or receives heat from the bedrock below (Neumann boundary condition at the bottom of the grid cell).

*P10 L17 "the resolution is *reduced* to ...".*

Right, corrected.

*P10 L19 Replace "computes" by "uses" or "computes with". Add "which is dynamically calculated" after "(Eq.2)" or similar.*

We have followed your suggestions.

*P10 L24-29 Could this be visualised for clarity?*

A schematic representation of the matrix is shown in Fig. S1 in the supplementary material.

*P10 L28 How small is "small" in this context? Clarify*

We have a threshold on the integrated viscosity. Points tagged as “ghost” (see Sec. 3.2) have an integrated viscosity lower than 1500 Pa s. This paragraph has been reformulated.

*P11 L1 Add a reference for OpenMP.*

We provide an url redirecting to the OpenMP website.

*P11 L15 Add a short overview what is coming next before going into details.*

We have added at the very beginning of this section:
“In the following, we present a simple calibration methodology for the Antarctic ice sheet based on a large ensemble of model simulations.”

*P11 L20 Add reference for ISMIP6 initMIP-Antarctica (ISMIP6 paper, website).*

Added reference to Nowicki et al., 2016.

*P12 L6 Is K0 changing the basal drag, or the basal drag coefficient? Clarify.*

The basal drag coefficient. We now refer more explicitly to variable names and equation numbers presented in Sec. 2.

*P12 L10 How does the BMB field keep the ice shelves stable? Clarify.*

For ISMIP6 initMIP-Antarctica, we use a data assimilation technique in which the basal drag coefficient is tuned to simulate an ice thickness as close as possible to observations assuming a fixed grounding line position. For ice shelves, we also inverse the basal melting rates under floating ice shelves so that the local (Eulerian) ice thickness derivative is minimal. The inferred basal melting rates are then averaged over the 18 ice shelf domains provided by the InitMIP project. With only a small addition, the sentence has been moved when first showing the present-day sub-shelf basal melting rates:
"Their values are based on the sectoral average of sub-shelf melt rates that ensured stable ice shelves (minimal Eulerian ice thickness derivative) in the recent intercomparison exercise InitMIP-Antarctica (Nowicki et al., 2016), with slight modifications due to change in resolution. They are in line with observations-based estimates (Rignot et al, 2013)."

*P12 L15 In my mind, the basic idea of LHS is to *sample* the hypercube and not perform all possible experiments. Maybe reword to avoid "the whole cube" if this is correct.*

It has been replaced to:
"We perform two times the 300 member ensemble with the flux at the grounding line of [...]".

*P12 L19 How is the low resolution data set produced from the original Bedmap2 data? Direct subsampling or smooth interpolation? This is a crucial part of preparing the input data and should be treated with detail and precision.*

We simply use a spatial bi-linear interpolation to generate the 40 km input data from the original high-resolution Bedmap2 data. This information has been added in the manuscript. We acknowledge the fact that this can alter the quality of the original data. In particular the shape (direction and slope) of fine scale structures such as narrow valleys and fjords might not be preserved. As stated in the discussion (second paragraph), we aim at introducing some sub-grid information in the model but this has yet to be done.

*P12 L20 Reword "discarded from the ensemble" to "not explored in this ensemble"*

Thanks for the suggestion, we have followed your suggestion.

*P13 L12 If differences are below 500, why does the scale go to 1000 in the figures?*

As stated in the manuscript, the differences are *generally* below 500 m although locally (e.g. Ronnie-Filchner area) we could have much larger differences.

*P13 L18 Do all these models use the same data, the same processing to get to the final input data and have similar resolution? Do you know if this is an error in the data or a problem of coarse resolution? What is different in models that do not show these features, if there are any?*

Pollard and DeConto (2009) also use a 40-km grid, while Martin et al. (2011) use their model at about 20 km resolution. However, input data, notably bedrock elevation / ice thickness and climate forcing, differ and can not explain why different models present systematic biases at the same locations.

The Ronnie-Filchner area presents a complex bedrock topography with pinning points stabilising the grounding line position. The fact that large scale ice sheet models do not explicitly account for sub-grid pinning points can explain consistent biases amongst the models. Similarly, the Transantarctic mountains present narrow ice streams draining part of the East Antarctic ice sheet and are generally poorly represented in large-scale ice sheet models.

More recent publications generally use an inverse method so that ice thickness mismatch with observations is greatly reduced and cannot be directly compared to the results presented in our manuscript.

*P13 L20 If the last point is resolved, maybe "... suggesting a common source of error related to the coarse model resolution". Or similar to add some interpretation to this comparison.*

We have added the following:
"Consistent model biases amongst these models, which use different input data, suggest a common source of error related to the coarse model resolution (20 to 40 km) or uncertainties in the bedrock dataset, particularly large in East Antarctica (Fretwell et al., 2013)."

*P16 L3 How can you be sure that the parameter range is sufficient/optimal for the transient experiments? Please add a short discussion on that.*

For the transient experiments, we used the twelve ensemble members that show the lowest RMSE when forced by perpetual present-day SMB. This shows the simulated variability through glacial-interglacial cycles for models calibrated on present-day ice sheet geometry. This is of course a too small subset to properly address the inter-model differences but we believe it is sufficient to discuss the general variability because the models produce similar evolutions. Ideally, we would have run all the ensemble members with the transient forcing and only kept the members showing the lowest RMSE at 0 kaBP. However, uncertainties in the climate forcing will also have biased the model responses.

We have nonetheless moderated the sentences:
"The uncertainty related to the choice of the internal parameters within our subset leads generally to up to [...]"

*P16 L10 Maybe "post-LGM retreat"?*

Thanks for the suggestion. Added.

*P17 L4 "relative to observations ... in this case, where ..." and briefly remind us what is different in the present experiments.*

This now reads:
"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."

*P17 L5 Where is the northern part of East Antarctica? polewards = south!, north=towards the margin?*

We have reformulated as:
“In particular there are some persisting model biases in ice thickness. In East Antarctica, the ice thickness is underestimated towards the pole and the Transantarctic mountains while it is overestimated towards the margins, from Queen Maud land to Wilkes land. In West Antarctica, there is an underestimation of ice thickness in the Ronnie-Filchner basin and an overestimation in the Ross basin.”

*P17 L15 Inversion of what? Specify.*

Changed to:
“However, by design, the fit with observations is systematically poorer compared to model results that make use of an inverse basal drag coefficient.”

*P17 L23 This comes a bit unexpected. Has vertical temperature been worked on in this paper? Why only the vertical?*

For sake of clarity, we have been more specific in the introductory sentence:
“Although widely used for ice sheet model spin-up or calibration, long-term integrations under present-day forcing induce a warm bias in the vertical temperature profile because they discard the diffusion of glacial-interglacial changes in surface temperature.”

In our paper, the calibration step has been done assuming a perpetual present-day climate forcing, which necessarily bias our simulated temperature field towards too high value. We believe that this point deserves discussion in this section and we have kept it in the revised manuscript.

*P17 L33 "lead to a more dynamic grounding line position"*

We have followed your suggestion.

*P17 L34 Has sensitivity to SL changes really been discussed in the paper?*

We have not tested explicitly the sensitivity to sea level change in this paper but it has been accounted for when performing glacial-interglacial simulations.

*P17 L34 Add sensitivity to sub-shelf melt rates?*

We now mention the sub-shelf melt rates. However, our point here was about changes in sea level and its local variations as this has been relatively unexplored within large-scale ice sheet model while sub-shelf melt rates is known to be a driver for glacial-interglacial Antarctic variability.

*P18 L11 Basal hydrology and semi-lagrangian tracking are described in "2.2 Additional features" next to other aspects that are not mentioned here in the conclusions. It may be useful to make it clearer already in the main text which of the described features are new developments in GRISLI.*

The “additional features” section contains material that is not directly related to the ice model itself (ice sheet thermo-mechanics). In this section, there is indeed old and new model features.

The basal hydrology has only been documented in a Phd dissertation written in French (Peyaud, 2006). The semi-lagrangian tracking has been presented by Lhomme et al. (2005) but for an other ice sheet model. It has been re-implemented in GRISLI for Quiquet et al. (2013) but has not been described there.

In the revised version of the manuscript we have listed better the difference with Ritz et al. (2001). Following is a list of the additions in the revised manuscript:

Introduction
"We also provide details on some components (sub-glacial hydrology and tracking particle scheme embedded in GRISLI) which are currently not documented in international scientific journals."

Section 2.1.1
"Ice deformation and mass conservation in GRISLI version 2.0 is mostly treated as in Ritz et al. (2001) with the notable exception of the use of a polynomial flow law with the introduction of a linear, Newtonian, viscosity."

Section 2.1.2
"Differing from Ritz et al. (2001), the velocity in GRISLI is now computed for the entire domain as the superposition of the shallow ice approximation (SIA) and the shallow shelf approximation (SSA) components, without using a sliding law to estimate basal velocities."

Section 2.1.6
"The ice temperature calculation has remained identical to Ritz et al. (2001)."

Section 2.2.1
"In the following we provide a complete description of the hydrology model because it has only been described in a French Phd dissertation (Peyaud, 2006) but currently lacks a description in an international scientific journal."

Section 2.2.2
"As in Ritz et al. (2001), GRISLI computes the bedrock response to ice load with an [...]".

Section 2.2.3
"GRISLI includes a passive tracer model that allows for the computation of vertical ice stratigraphy, i.e. time and location of ice deposition for the vertical model grid points. The model is the one of Lhomme et al. (2005) re-implemented in GRISLI by Quiquet et al. (2013)."

*P18 L26 Why does validating a model for the Antarctic give confidence to also use it for the NH. A bit more information is needed here to bridge that gap.*

If we consider today ice sheets that are still present now, the Antarctic ice sheet is a good case study because of its interaction with the ocean which could have been more important through glacial-interglacial cycles compared to the Greenland ice sheet. The fact that we manage to reproduce the Antarctic ice sheet variability gives some confidence in the ability of the model to simulate large grounding line migration that could have happened for palaeo ice sheets such as for example the Kara-Barents ice sheet. However, sub-glacial conditions (e.g. till distribution and rheology) are probably largely different and this has not been explored in the present manuscript. To avoid overstated statement we have decided to remove this sentence from the revised manuscript.

*P18 L30 Replace "and" by "or", unless all three have to be contacted to get the model code.*

Changed to “or”, no need to contact all three.

*Tables and figures*

*P26 in the middle. Replace "N.m" by "N m"?*

Yes, changed.

*P27 caption Table 2. Write out LHS.*

Done.

*P27 Figure 2. Left panel cold have additional contours to delineate the regions and a colour bar. Why does the grounding line have different melting rates than the shelf?*

The left panel simply displays the different sectors as defined in ISMIP6 InitMIP-Antarctica (Nowicki et al., 2016). The numbering is irrelevant as we never refer to the sector number in the text. We have nonetheless changed the color coding so as to show better the extent of the different sectors.

We have added this in the figure caption:
“The melting rates are different for the shelf and the associated grounding line to mimic the higher values observed close to the grounding line Rignot et al. (2013).”

*P29 Figure 5. Use a colour for the GL contour that does not appear in the colour map, e.g. black or dark gray.*

Thanks for the suggestion, we have used dark grey instead of red.

*P30 Figure 6. Same as for figure 5.*

Changed here as well.

*P32 Figure 8. Suggest to move the parameter values to a table or the main text.*

We have moved this information to a table.

*P34 Figure 12. "materialised" –> "shown" or "indicated" Why is the GL so patchy here compared to the steady state case?*

We have chosen the word “indicated”.

Fig. 5 and Fig. 6 in the original manuscript shows only the present-day observed grounding line. But in fact the grounding line is also relatively patchy for the steady state as soon as the model simulates a significant departure from present-day position. You will find below the surface elevation for the two ensemble members that show the lowest RMSE in Sec. 3 (100-kyr simulation under perpetual climate) in which the grounding line is indicated by a thick red line.

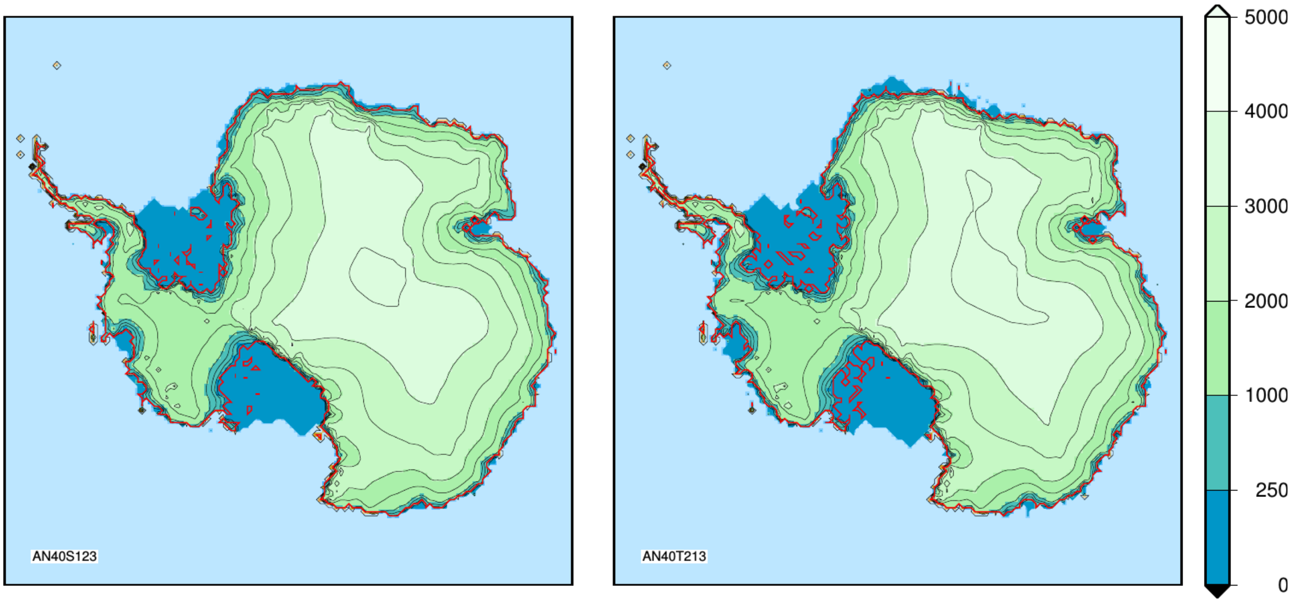


**Figure R1:** Simulated surface elevation at the end of the 100 kyrs simulations forced by perpetual present-day climate (Sec. 3) for the two ensemble members that show the lowest RMSE with respect to the observations (AN40S123 and AN40T213). The grounding line is indicated by the thick red line.

*P35 Figure 13. Same as for figure 5.*

Changed here as well.