Dear editor, dear Dr. Schanwell,

We thank the reviewer for his constructive, insightful and helpful evaluation which we feel helped to improve the manuscript. This instigated additional modeling that resulted in numerous refinements, and significant upgrades to the model description and discussion sections. Below, we provide a point-by-point response to each comment, which we numbered in red for easier reference. Our response is structured as follows: Referee comment (*in black italics*), author's response (in green), and proposed changes in the original manuscript text (*in blue italics*) where significant rewriting was done to include the suggested changes. We also add to the end of each figure caption (in blue) their proposed numbering in the revised version of the manuscript.

Main concerns

1. *"[…] I suggest to expand section 2.1 to add this required information. To be more specific, what type of stress balance does SICOPOLIS use? What kind of basal friction law do you apply? I know you list the parameters in Table 1, but without the corresponding equation, they are rather useless. Does your basal friction coefficient vary spatially and/or temporally? What are your boundary conditions for your enthalpy equation (e.g. do you specifiy a geothermal heat flux? Is it spatially constant?)? How do you treat calving in the model? There are a number of ways how to parameterise this. Since you talk about this in your results, it is essential to know how this is handled in your model.*
Also you should mention with what geometry your initialise your model. I believe it is with present-day geometry, but with which dataset (Bedmap2, Bedmachine)? Do you take the bedrock and ocean floor topography from the same dataset?"

Our response: We have expanded the model description, including the requested additional information, as can be seen below:

"For our experiments we employ the 3D thermomechanical polythermal ice-sheet model SICOPOLIS (Greve, 1997; Sato and Greve, 2012) with a 20 km horizontal grid resolution and 81 terrain-following layers in the vertical. It uses the one-layer enthalpy scheme introduced in Greve and Blatter (2016), which is able to correctly track the position of the cold-temperate transition in the thermal structure of a polythermal ice body. The model combines the Shallow Ice (SIA) and Shallow Shelf (SSA) approximations using

$$U=(1-w)\cdot u_{sia}+u_{ssa}$$

where U is the resulting hybrid velocity, u_{ssa} and u_{sia} are the SSA and SIA horizontal velocities, respectively, and w is a weight computed as

$$w=\frac{2}{\pi}\arctan\left(\frac{u_{ssa}^2}{u_{ref}^2}\right)$$

where the reference velocity, u_{ref}, is set to 30 ma^{-1}, which reduces the contribution from SIA velocities mostly in coastal areas of fast ice flow where this approximation becomes invalid. Basal sliding is implemented within the computation of SSA velocities as a Weertman-type law (cf. Bernales et al., 2017a, Eqs. 2–6). Sliding coefficients are adjusted during the equilibrium calibration run such that grounded ice thickness matches the present-day observations from the Bedmap2 data set (Fretwell et al., 2013) as close as possible. This adjustment process follows the iterative method of Pollard & DeConto (2012b), where the coefficients are allowed to vary spatially, but not temporally, outside of the adjustment phase. Sliding coefficients in sub-ice shelf and ocean areas are set to 10^5 m $yr^{-1}Pa^{-1}$, representing soft, deformable sediment, in case the grounded ice advances over this region. The initial bedrock, ice base, and ocean floor elevations are also taken from Bedmap2. Enhancement factors for both grounded and floating ice are set to 1, based on sensitivity tests in Bernales et al.

(2017b). This choice provides the best match between observed and modelled ice thickness, similar to the findings in Pollard and DeConto (2012a).

Surface mass balance is calculated as the difference between accumulation and surface melting. The latter is computed using a semi-analytical solution of the positive degree day (PDD) model as in Calov and Greve (2005). Near-surface air temperatures entering the PDD scheme are adjusted through a lapse rate correction of 8.0 °C km^{-1} to account for differences between the modelled ice sheet topography and that used in the climate model from which the air temperatures are taken. For the basal mass balance of ice shelves, we use a calibration scheme of basal melting rates developed by Bernales et al. (2017b) to optimise a parameterisation based on Beckmann and Goosse (2003) and Martin et al. (2011) that assumes a quadratic dependence on ocean thermal forcing (Holland et al., 2008; Pollard and DeConto, 2012a; Favier et al., 2019). This optimised parameterisation is able to respond to the variations in the Glacial Index (GI, Sect. 2.2). A more detailed description of this parameterisation is given in the supplementary material. In our experiments, we prescribe a temporal lag of 300 years for the ocean response to GI variations, which is considered the most likely lag in response time of the ocean compared to the atmosphere in this region (Yang and Zhu, 2011). At the grounding line, the basal mass balance of partially floating grid cells is computed as the average melting of the surrounding, fully floating cells, multiplied by a factor between 0 and 1 that depends on the fraction of the cell that is floating. This fraction is computed using an estimate of the sub-grid grounding line position based on an interpolation of the current, modelled bedrock and ice-shelf basal topographies. At the ice shelf fronts, calving events are parameterised through a simple thickness threshold, where ice thinner than 50 m is instantly calved out.

Glacial isostatic adjustment is implemented using a simple elastic lithosphere, relaxing asthenosphere (ELRA) model, with a time lag of 1 kyr and flexural rigidity of 2.0×10^{25} Nm, which was found by Konrad et al. (2014) to best reproduce the results of a fully-coupled ice sheet–self-gravitating viscoelastic solid Earth model. The geothermal heat flux applied at the base of the lithosphere is taken from Maule et al. (2005) and is kept constant throughout the simulations. All relevant parameters used in the modelling experiments are listed in Table 1."

*"**2.** Could you please motivate the ensembles or parameters changes that you are investigating a bit more? As it stands now, it seems like you picked a number of parameters, but there also could an argument be made for a bunch of other parameters to be varied."*

Our response: We picked these ensembles as they are inherent sources of uncertainty that were not addressed by any previous studies that included MIS11. We also performed additional tests to support our parameter choices (such as ocean temperature, lag in its response, and the choice of climate model), which we included in the supplementary material. We justify the choice of ensembles in the last paragraph of the introduction:

"For this purpose, we perform five ensembles of numerical simulations of the AIS and focus on aspects that remain unaddressed by previous studies. We evaluate the impact on resulting ice volume and extent of the choice of proxy records (including their differences in signal intensity and structure), of the choice of sea level reconstruction, and of uncertainties in assumptions regarding the geometry of the AIS at the start of MIS11c. Additionally, we assessed the robustness of our results to the uncertainty in external climate forcings, which we present in the supplementary material."

*"**3.** I find most of the figures (e.g. 4, 6,7,9) not very informative. Looking at integrated quantities is OK, but having five Figures like that is too much. I suggest to combine them into a Figure with several panels. I also find it hard to judge in these volume plots whether differences are small or large (Is 2000 km3 a lot?). Maybe better to plot it in percent normalised to your starting volume? Also just because your ice volume is similar does not mean you cannot have regional differences in grounding-line position or ice thickness. For example on P16L279 you state ". . . show similar retreat rates..." but I cannot find a Figure where this is actually shown. So I suggest to add some Figures, where we can also look at some spatial differences (a few suggestion in the technical corrections below). For example, you could plot some grounding-line positions from different simulations in 2D on top of each other to see the differences in retreat or lack thereof. I also encourage the authors to discuss their results more in depth. For example, they state in L276ff that different initial ice sheet configurations converge to the same geometry for the same climate forcings. This alone is quite surprising to me and at least warrants a discussion why potential feedback mechanisms (e.g. stabilising grounding-line on topographic height) are not triggered in these simulations?"*

Our response: We have added a compilation of all different GIs as a panel in Fig. 2, which allows us to remove them from Fig. 1 (see Figs. 1 and 2 below). Regarding figures 4, 6, 7, and 9, they were restructured, with their (b) panels being merged into a single figure, and added a line that represents present AIS volume, as suggested by Reviewer 2 (see Fig. 3 below). We have added a new figure (see Fig. 4 below) where we show the grounding lines for each of our core experiments at times of interest: 420, 415, 410, and 405 ka. Regarding the fact that different configurations converge to the same geometry, we have found a tipping point at 412 ka (as pointed out by Reviewer 2 in his comment 47), where the ocean forcing under the main ice shelves (cf. thermal forcing in Fig. 8) is strong enough to drive ice sheet retreat in all geometry scenarios. There are two grounding-line stabilising feedbacks not included in our current version of the model: (*i*) a local sea-level drop caused by a reduced gravitational attraction of a shrinking ice sheet (e.g. Mitrovica et al., 2009), and (*ii*) the observed faster rebound of the crust due to a lower mantle viscosity in some WAIS locations (Barletta et al., 2018). Even though our ELRA model is set up with a relatively fast response time of 1 kyr (compared to the standard 3 kyr), the resulting bedrock uplift is still not able to trigger a stabilizing effect that compensates for the strong ocean-driven retreat. These feedback mechanisms during MIS11c could be further investigated through the utilization of an Earth-ice coupled model, which is certainly an interesting topic for future research. These points will be incorporated in our Discussion section.

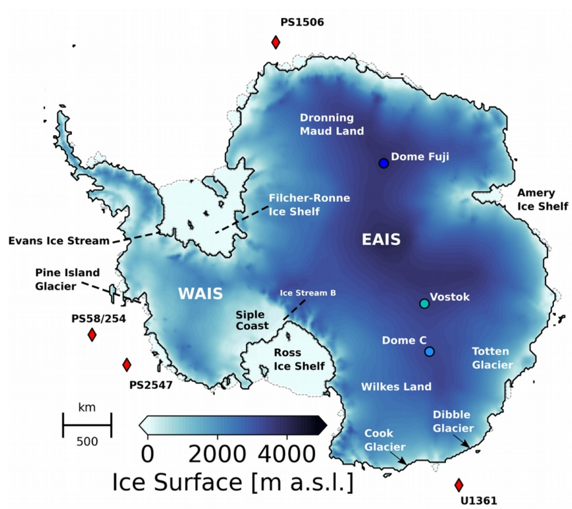

Figure 1: Surface topography of the AIS at the start of our core experiments (425 ka), based on a calibration against Bedmap2 (Fretwell et al., 2013; see Sect. 2.1). The locations mentioned in the text, including the drilling sites of the ice (circles) and sediment (red diamonds) cores on and around Antarctica, are showcased. This is Fig. 1 after the revisions to the manuscript.

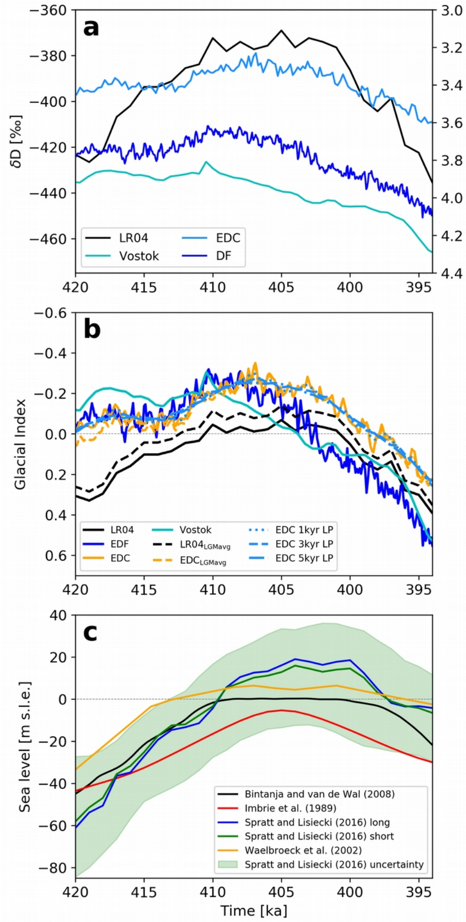

Figure 2: Reconstructions used in this study: (a) LR04 $\delta^{18}O$ (black) and ice-core δD [‰]; (b) resulting Glacial Indices from the reconstructions in (a) (cf. Sect. 2 and Table 2); (c) global mean sea level anomaly relative to PI (meter sea level equivalent, m s.l.e.). This is Fig. 2 after the revisions to the manuscript.

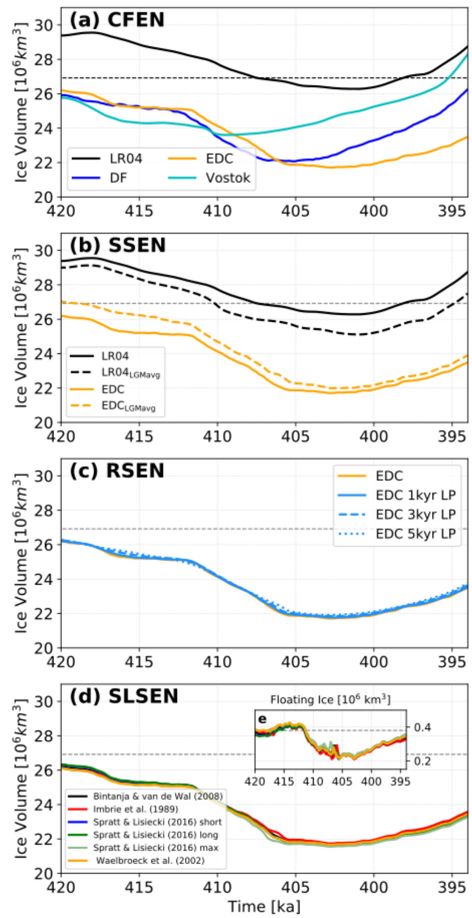

Figure 3: Sensitivity of AIS response (in total ice volume, 10^3 km^3) between 420 ka and 394 ka to (a) CFEN GI reconstructions; (b) SSEN rescaled GI reconstructions; (c) RSEN low-pass filtered GI reconstructions; (d) SLEN sea level reconstructions forced by EDC GI (cf. Table 4). This is Fig. 4 after the revisions to the manuscript.

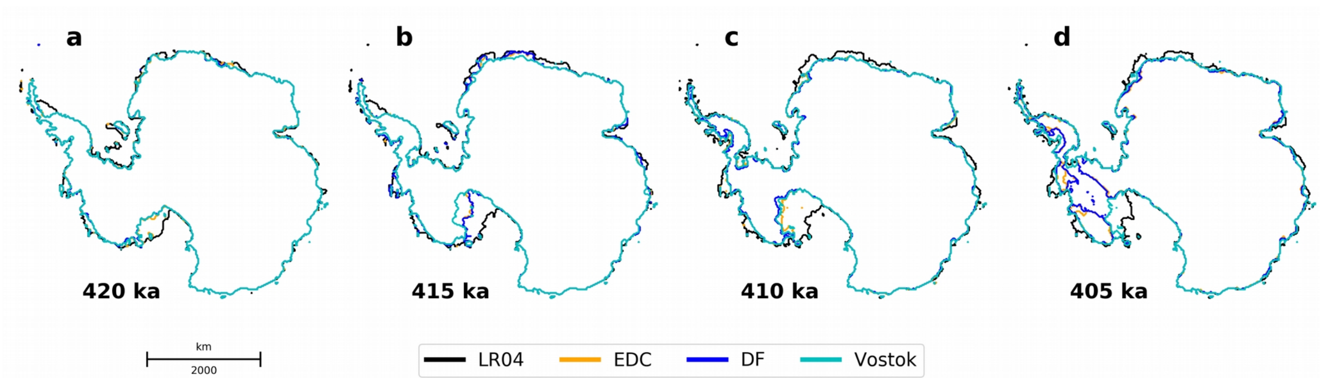

Figure 4: Grounding lines at 420, 415, 410, and 405 ka for the CFEN simulations. This is Fig. 5 after the revisions to the manuscript.

*“**4.** I think you should scratch your attempt to identify drivers for future change. You have it in your research questions, but other than in the conclusion section you never mention it again. And your statement in the conclusion statement is extremely vague (and we know this already) and to be honest not backed up by your simulation results.”*

Our response: We agree with the reviewer regarding the relative weakness of this section, and have removed the mention of drivers for future change from our research questions and the conclusions.

*“**5.** The abstract in its current form is much too long and too descriptive. Please shorten and make more concise.”*

Our response: we have shortened the abstract, and modified it to also account for the analyses suggested by Reviewer 2. It now reads:

“Studying the response of the Antarctic ice sheets to periods when climate conditions were similar to the present can provide important insights for understanding the observed changes and help identify natural drivers of ice sheet retreat. The Marine Isotope Substage 11c (MIS11c) interglacial is one of the best candidates for an in-depth analysis, given that during its later portion, orbital parameters were close to our current interglacial. Although Antarctic ice core data indicate that MIS11c CO_2 levels were close to Pre Industrial, they also show that warmer-than-present temperatures (of about 2 °C) lasted for much longer than during other interglacials. While substantial work has been conducted regarding the response of the Greenland Ice Sheet to MIS11c climate, the response of the Antarctic ice sheets and their contribution to sea level rise remain unclear. We improve the current constraints for this period using a numerical ice-sheet model forced by MIS11c climate conditions derived from climate model outputs scaled by three glaciological and one sedimentary proxy records of ice volume. Our results suggest that the East and West Antarctic ice sheets contributed with 6.7 to 8.2 m to the MIS11c sea level rise, independently of the choice of sea level reconstructions and multi-centennial climate variability. The main source of uncertainty arises from the sensitivity of the East Antarctic Ice Sheet response to the initial configuration. We have found that the regional climate signal of the MIS11c peak warming in Antarctica captured by the ice core records is necessary for the recorded sea level highstand to be reproduced, and that oceanic warming of only 1.5 °C under the Ross and Filchner-Ronne ice shelves, if sustained for ca. 4 thousand years, leads to a West Antarctic Ice Sheet collapse.”

*“**6.** This is more an optional point and maybe a matter of taste, but I think you could also add a model limitations section. There are a few places where you can shorten the main text (see below), so that this would not much increase the length of the manuscript. I always find it helpful in modelling papers to have a section in which limitations and potential future avenues for improvements are discussed. I must admit that as the paper stands now with very little information about the ice-sheet model, it is hard to examine what the benefit of your model setup is?”*

Our response: We expect that the changes made in the methods (included as a response to comment 1) help to partly clarify this issue. We have further opted to discuss the model limitations within the context where they were relevant in the discussions, as opposed to giving them their own section.

Technical corrections

7. *“L8 I do not think that the Greenland information is necessary in the abstract. Also the latter half of the sentence makes no sense to me “. . . , both configurations of the Antarctic ice sheets. . . ”? What configurations?”*

Our response: We have removed the mention of the Greenland Ice Sheet sea-level contribution from the abstract, and we expect that the reformulation presented above (comment 5) has clarified the text.

8. “L12 Does LR04 need to be introduced as an acronym? I did not know straight away what it is.”

Our response: We have removed the LR04 acronym from the abstract while making it more concise and less descriptive as requested (see response to comment 5).

9. *L17 Here and throughout, I find the term "ice-sheet contraction" unusual. I know what you mean, but I think more commonly it is referred to as "ice-sheet retreat". Please consider changing it.*
Our response: We had used "contraction" since the changes seen are both in extent and volume. Nevertheless, we reverted to the usual term as suggested.

10. *L29-34 This sentence is way too long and confusing. Please split up and make clearer.*
Our response: We recognise the sentence was indeed too long, and have rewritten the passage. It now reads:
"However, Dutton et al. (2015) point that climate modelling experiments with realistic orbital and greenhouse gas forcings fail to fully capture this MIS11c warming despite the fact that orbital parameters were almost identical to Present Day (PD) during its late stage (EPICA, 2004; Raynaud et al., 2005). Earlier studies (e.g., Milker et al., 2013; Kleinen et al., 2014) have also shown that climate models tend to underestimate climate variations during this interglacial, for which ice core reconstructions show the mean annual atmospheric temperatures over Antarctica to have been ca. 2 ºC warmer than Pre-Industrial (PI) temperatures."

11. *L43 What do you mean by "reduced stability"? And why would that trigger stronger glacial-interglacial cycles?*
Our response: Holden et al. (2011) show that a reduced stability of the WAIS (i.e., a higher susceptibility to collapse) through time is caused by an increased bedrock relief as a result of continuous erosion, while Holden et al. (2010) show that the positive feedback of a strong WAIS retreat could contribute to these stronger cycles. We have added the Holden et al. (2010) citation and rewrote this part of the introduction as shown below. We refrained from discussing the mechanisms for the mentioned feedback in detail, since they are not the focus of our work.
"The length of this unusual interglacial and the transition to stronger glacial-interglacial cycles seen in the recent geological record may have been triggered by a reduced stability of the West Antarctic Ice Sheet due to cumulative bed erosion (WAIS, Fig. 1; Holden et al., 2011), which in turn provided a positive climate feedback (Holden et al., 2010)."

12. *L49-55 I think this paragraph can be thrown out, as it is irrelevant to the Antarctic simulations in the paper. It suffices to say, I believe, that the ice-sheet history in Antarctica is more uncertain than for Greenland.*
Our response: We have removed the paragraph, and moved the appropriate references to the beginning of the next paragraph, which we start with
"The MIS11c history of Antarctica is less constrained than that of Greenland (e.g., Willerslev et al., 2007; Reyes et al, 2014; Dutton et al,. 2015; Robinson et al, 2017)."

13. *L56-58 The first half of the sentence is confusing. The way it is written, it makes it sound as if Raymo and Mitrovica estimated it to be 6-13 m above present-day? But why is there a reference to Dutton et al. then? Here and throughout, could you please try to keep sentences shorter. It makes it easier to follow for the reader.*
Our response: We have removed the Dutton et al. reference, as it was misplaced there. We tried to rewrite the sentences to be shorter and easier to follow where necessary.

14. *L61-64 Again a very long sentence which I do not understand. Please break up the sentence and clarify.*
Our response: We have rephrased the sentence:
"Counter-intuitively, the dating of moraines in the Dry Valleys to MIS11c is used to indirectly support regional ice sheet retreat (Swanger et al., 2017). Swanger et al. (2017) argue that ice sheet retreat in this region would result in nearby open-water conditions and thus a source of moisture and enhanced precipitation, fueling the local glacier growth that produced the moraines in the Dry Valleys."

15. *L65-80 Here, I would like to see what your study adds to studies like the one from Sutter et al. 2019. What is the advantage of your study/model setup ?*

Our response: Our study has different objectives than that of Sutter et al. (2019), and thus uses different approaches. For example, we focus on MIS11 exclusively, evaluating different transient climate signals and testing for a different set of factors that can influence ice sheet simulations. We made this clearer by rewriting the last two paragraphs of the introduction:

"As detailed, many modelling studies have investigated AIS responses over time periods that include MIS11. However, so far none has focused specifically on this period. Given a dearth of information for MIS11 and conflicting constraints on how Antarctica responded to this exceptionally long interglacial (Milker et al., 2013; Dutton et al., 2015), we here focus on MIS11c, the peak warming period between 420 and 394 ka. Our aim is to reduce the current uncertainties in the AIS behaviour during MIS11c, specifically addressing the following questions:

[...]

For this purpose, we perform five ensembles of numerical simulations of the AIS and focus on aspects that remain unaddressed by previous studies. We evaluate the impact on resulting ice volume and extent of the choice of proxy records (including their differences in signal intensity and structure), of the choice of sea level reconstruction, and of uncertainties in assumptions regarding the geometry of the AIS at the start of MIS11c. Additionally, we assessed the robustness of our results to the uncertainty in external climate forcings, which we present in the supplementary material."

16. *L81 I do not agree that you are presenting model reconstructions. What you present are sensitivity experiments. But as far as I can tell, you are not trying to match any geological constraints which is what I understand as model reconstruction.*

Our response: Geological constraints are very scarce for this period, and we discuss how our simulations match the available constraints throughout the manuscript (e.g., L313-318 and L368-375 in the original submission). Nevertheless, it is a good point that our experiments can be seen as sensitivity experiments. For this reason, we have refrained from using the term and rewrote this sentence also with input from Reviewer 2:

"Given a dearth of information for MIS11 and conflicting constraints on how Antarctica responded to this exceptionally long interglacial (Milker et al., 2013; Dutton et al., 2015), we here focus on MIS11c, the peak warming period between 420 and 394 ka. Our aim is to reduce the current uncertainties in the AIS behaviour during MIS11c, specifically addressing the following questions: [...]"

17. *L85 As said above, I do not think you really address the last question about future ice-sheet changes. Therefore, I recommend removing it from the manuscript altogether.*

Our response: We have removed this question.

18. *L106 In addition to the changes suggested above. How do you initialise for the different ice-sheet configurations? Do you use the same temperature spin-up and let it evolve afterwards? Or do you let it evolve to a different geometry and do the temperature spin-up then with a fixed geometry?*

Our response: In response to your comments and those of Reviewer 2 (comment 33) regarding our different geometries, we have changed our approach. We force the thermally spun up ice sheet with LGM conditions for 5 kyr so it grows to an intermediate stage between PI and LGM extent, and then place the resulting geometry at different points in time: 420, 425 and 430 ka. We then let these transiently evolve from then until 395 ka, and analyse the period between 420 and 395 ka, as in the original submission. We made changes to the text (see below) and to Table 3 to reflect this new approach:

"In order to create a representative range of initial geometries at 420 ka, we use a common starting geometry, but vary the relaxation time (0, 5, and 10 kyr). For this common starting geometry, we perturb the thermally spun up AIS with a constant LGM climate (i.e., temperature and precipitation) without sub ice-shelf melting, allowing it to grow to an intermediate extent between PI and LGM over a 5 kyr period. We assume this to be the starting AIS geometry at 420 (Fig. 3a), 425, and 430 ka, and let it transiently evolve from then. Table 3 summarises the procedure to create each initial ice sheet geometry (labelled gmt1 to gmt3; Fig. 3a-c). The gmt1 initial topography is generally more extensive and thinner

than the control. Its grounding line advanced at the southern margin of the Filcher-Ronne Ice Shelf and at Siple Coast, but the ice sheet interior is on average 200 m thinner than the control and indeed up to 500 m thinner across particular regions such as the dome areas of the WAIS and Wilkes Land (Dome C). It is, however, about 200 m thicker at its fringes, which results in a gentler surface gradient towards the ice sheet margins. The gmt2 initial topography is less than 100 m thinner than control over the EAIS interior, and ca. 100 m thicker over the WAIS interior and at the EAIS margins. Finally, the gmt3 initial topography is overall thicker than control, though not by more than 100 m except at the western side of the Antarctic Peninsula and the WAIS margins, where some regions are up to 300 m thicker (Fig. 3)."

19. *L106 From where do you get your surface temperature distribution? An ice core only provides you with temperature changes with respect to a certain baseline. Please add this to this section.*
Our response: Based on this comment and on comment 20, which led us to slightly alter our approach (as detailed in the answer to comment 20), we have rewritten the paragraph for increased clarity:
"All ensembles cover a period from 420 to 394 ka. To initialise the AIS, we first perform a thermal spin-up over a period of 195 kyr from 620 to 425 ka, i.e., apply a transient surface temperature signal from the EDC ice core (Jouzel et al., 2007) as an anomaly to our PI climate (described in the next section) while keeping the ice sheet geometry constant at our previously calibrated Bedmap2-based configuration. We then let the AIS freely evolve for 5 kyr, between 425 and 420 ka, applying transient GI forcing during the relaxation period. We chose 425 ka as the starting point for relaxation because it is when the oxygen isotope value in the EDC ice core is closest to PI during our study period. In summary, we ignore the first 5 kyr of our simulations to avoid a shock from suddenly letting the ice-sheet topography freely evolve at the start of our period of interest."

20. *L107-109 This means you just move this shock outside of your time period of interest? This is in general OK, but raises the following questions: What forcing do you apply for the 5 ka in which the ice geometry is allowed to freely evolve? And how far away do you get from your initial geometry? And I am also missing a plot where you show that your ice sheet is close to steady state. I would appreciate if you could add a plot for this.*
Our response: We had initially applied the same GI forcing based on the EDC core for all simulations during the relaxation stage, so that they all had the same geometry at 420 ka. This proved to be a problem for the LR04-forced simulation, since it significantly deviates in its isotope values from the others. Consequently, and in response to the review, we now apply the GI forcing during the relaxation stage that corresponds to the forcing during the main experiments (i.e., the 425-420 ka DF GI for the DF-forced runs, EDC GI for the EDC-forced runs, and so on). A figure showing the spread in initial geometries during this period is now provided in the supplement (see Fig. 5 below). We do not provide a plot showing that the ice sheet is close to steady state because the point of the thermal spin-up is precisely to remove the effects of the initial steady state (attained during the calibration of the model) from our simulations, and offer a more realistic internal thermal structure for the AIS. All figures shown already contain the new simulations, and the corresponding part in the Methods section is also revised as shown in comment 19.

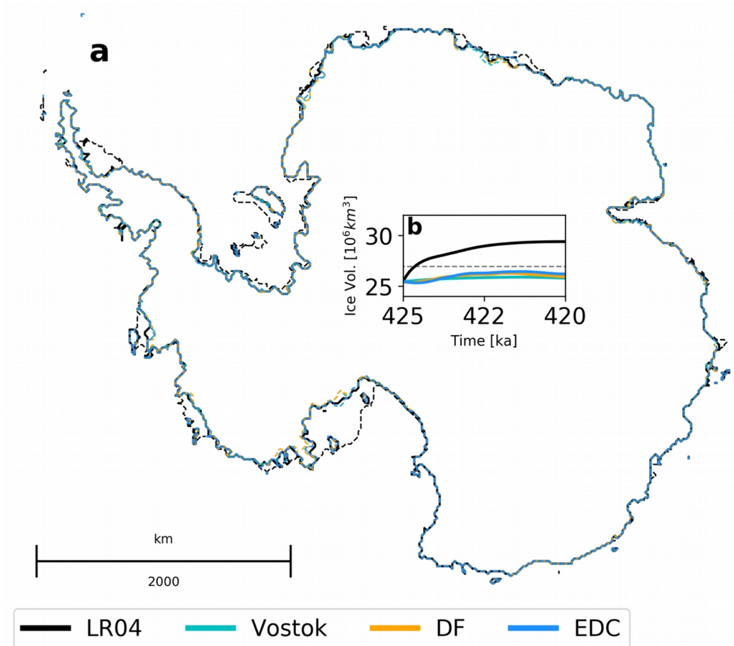

Figure 5: Relaxation period between 425 and 420 ka for all four CFEN members. (a) shows the grounding line at 420 ka (solid line) and at 425 ka for each member (dashed lines); (b) shows the evolution of total ice volume [10^6 km^3] during this 5 kyr period for each member. Dashed line shows the volume of the present-day AIS according to Bedmap2. This is Fig. S12 after the revisions to the supplement.

21. *L129, equation (2): From this equation I gather that you apply the same temperature differences to the ocean as you do to the atmosphere? And you also do not apply a time lag to the ocean warming/cooling? Is that really realistic giving the long response time of the ocean compared to the atmosphere? At the very least, this choice should be discussed somewhere in the text.*

Our response: We do not apply the same temperature differences to ocean and atmosphere, but modulate them with the same index. The differences are obtained by the ocean temperature and atmospheric temperature fields from the climate forcing. We appreciate this criticism regarding the ocean lag, also voiced by Reviewer 2 (in his comment 4), and have acted accordingly. We have introduced a lag to the ocean forcing of 300 years, as this is the timescale of response of the Southern Ocean (Yang & Zhu, 2011). We additionally present in the supplement an ensemble of sensitivity tests to different time lags in the ocean forcing (see Fig. 6 below), which shows their effect to be very small compared to the timescales of this study. We tried to clarify the concern about the differences applied by rewriting the last sentence before Eq. (2):

"The atmospheric and ocean temperature (T) fields at time t are reconstructed based on their respective PI and LGM reference fields (T_{PI} and T_{LGM} respectively) using: [...]"

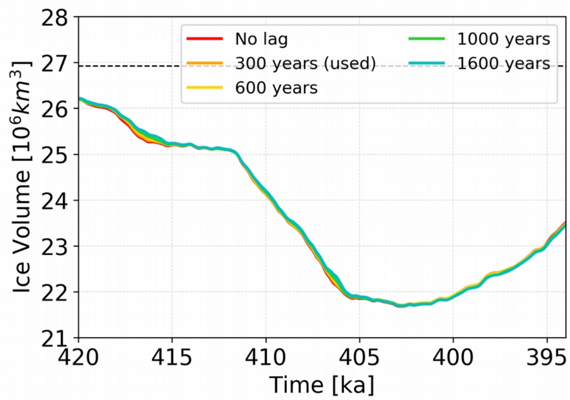

Figure 6: Sensitivity of the AIS response expressed in total ice volume [10^6 km^3] to a range of lags (0-1600 years) between the atmospheric forcing and the ocean forcing between 420 and 394 ka. This is Fig. S10 after the revisions to the supplement.

22. *L137 To me all headers in this section should rather read "Model sensitivity to XXX". Because this is ultimately what you do in this paper, rather than rigorously quantifying uncertainties.*

Our response: We have changed it accordingly. We thank for this good suggestion as it also makes it consistent with the headers in section 3 (results).

23. *L139-141 This sentence needs rewriting. I am not sure I understand what you are saying.*

Our response: We have rewritten this section, also based on an additional request from Reviewer 2 (comment 30):

"Because different approaches have been used to transform the isotope curves into a GI, we assess the sensitivity to the choice of the scaling procedure by performing an additional scaling using another reference value for δX_{LGM}. In the new scaling procedure, δX_{LGM} is the average (between 19 ka and 26.5 ka) rather than the peak value. We compare the effects of using these two procedures when applied to the EDC ice core δD and the LR04 stack $\delta^{18}O$ records. We call this ensemble the Scaling Sensitivity Ensemble (SSEN)."

24. *L154 should be "mean sea level"*

Our response: We have corrected as requested.

25. *L166 Here I believe you say that you also initialise with present-day conditions, but this needs to come much earlier and with more info as to what datasets you used for this.*

Our response: We expect that the changes made to the Methods section as described above (comment 1) have successfully addressed this issue. Hence, in this paragraph we merely provide a reference to section 2.1:

"Similar studies that assessed AIS changes over (one or more) glacial and interglacial cycles often adopt a PI or PD starting geometry (e.g., Sutter et al., 2019, Tigchelaar et al., 2019, Albrecht et al., 2020). We have followed the same approach in our CFEN experiments (see Sect. 2.1)"

26. *L221 you state: ". . . ice sheet contraction is associated with strong basal melting close to the grounding lines ...". First of all this comes a bit out of the blue. Secondly, you show little evidence that this is actually the case. In Fig. 5 you show that basal melting is dominating, but if you have different SMB rates, the basal melt rate could be either 1.5m/yr or 6 m/yr. Please also avoid relative terms like "strong" without giving any numbers. Do you mean 5, 50, or 500 m/yr when you say "strong" melting. Related to this, do you apply melting to partially grounded grid cells or only to fully floating? This makes a big difference how much your grounding line retreats for similar melt rates.*

Our response: Based on this and other comments from the reviewers, we have moved this paragraph to the Discussion section, where it is more fitting and does not "come out of the blue". We added the information about the basal melting to the Methods section (shown above in our response to comment 1). Also, we expect that changes made to Fig. 5 in the original manuscript (see Fig. 7 below) further help clarify the regions where SMB or ice-shelf basal melting dominates.

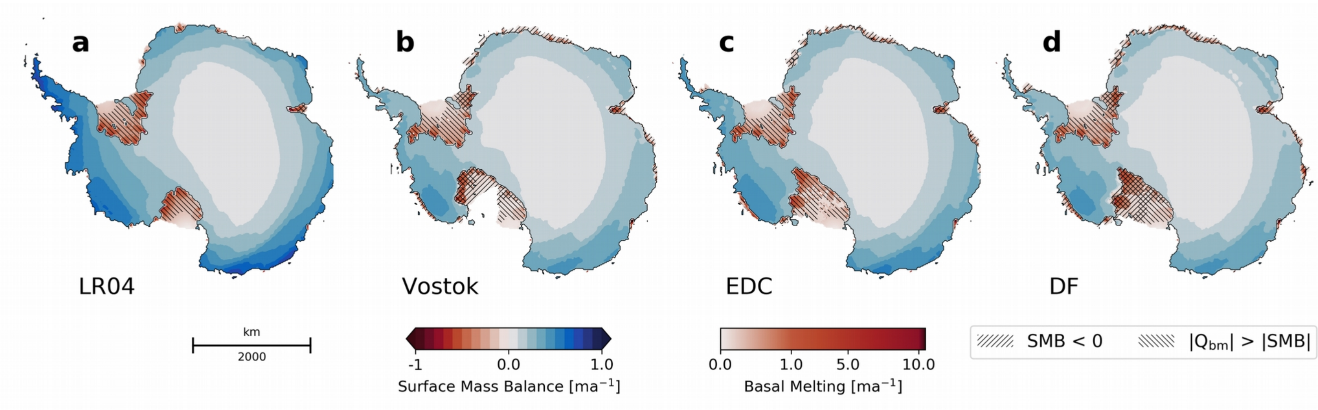

Figure 7: Surface mass balance (SMB, ma^{-1}) for the grounded ice and basal melting (Q_{bm}, ma^{-1}) for the ice shelves for the CFEN simulations at 415 ka. Hatched areas show where basal melting dominates over surface mass balance and where surface mass balance is positive (i.e., where surface ablation occurs). Everywhere where $|Q_{bm}| > |SMB|$, ice shelves are thinning. This is Fig. 8 after the revisions to the manuscript.

27. L222 should read ". . . Siple Coast, at the Ross Ice Shelf, and underneath ..."
Our response: We thank the reviewer for spotting the typo and have corrected it.

28. L223-224 & L227 Since your basal melt rate is a quadratic function of your ocean temperature, stating that it is a combination of warming of the upper ocean layer and high melt rates is saying the same thing. Please reformulate.
Our response: This sentence has been removed, and we focus our discussion on the thermal forcing under the ice shelves, as opposed to the distribution of SMB vs. ice-shelf basal melting (Figs. 7, and 8).

29. L228 Two things here. First, since you have a separate results and discussion section, I was expecting only a description of the results. However, here and in other places (e.g. L245, L256-259) in your results section you are interpreting and discussing your results already. So either you have a combined results and discussion section or you move this material to your discussion section. Secondly, I cannot confirm your statement that ice loss is dominated by surface ablation on Amery in Fig.5. First of all, the panels are too small, so I am not sure if Amery is hatched or not? I do not really understand the purpose of Fig. 5, but to me Amery looks pretty red which means to me that there is a lot of ablation in this area. So why would it not retreat there and why is ablation so high in this region compared to basal melting?
Our response: Thank you for highlighting these points. We have moved this part to the discussion as suggested. As described in comment 26, Fig. 7 in this response letter shows significant improvements related to Fig. 5 in the original manuscript, and now better highlights the regions affected by basal melting and surface ablation. In combination with a new figure provided (see Fig. 8 below), we were able to see that Amery is indeed, contrary to what we originally stated, dominated by basal melting. However, the difference between surface ablation and basal melting is not as pronounced as in the larger ice shelves, such as Ross and Filchner-Ronne. We will make the necessary adjustments to the text.

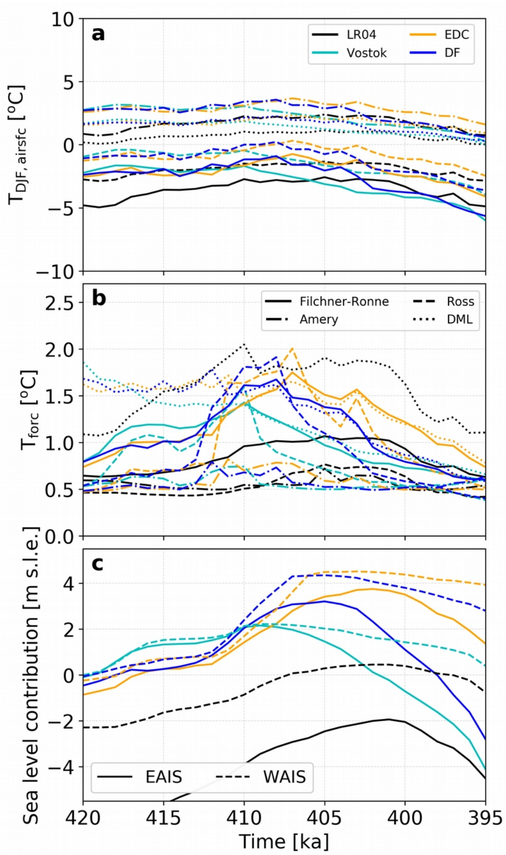

Figure 8: Evolution throughout MIS11 for each CFEN member averaged over specified Antarctic ice shelves for (a) Summer surface air temperature [ºC]; (b) thermal forcing under the ice shelves (i.e., ocean temperature minus ice base temperature, in ºC); and (c) sea level contribution by EAIS and WAIS. Colours denote the respective CFEN member, while line styles denote each ice shelf (panels a,b), and each ice sheet (panel c). DML refers to an average for ice shelves along the Dronning Maud Land between 27ºW and 30ºE. This is Fig. 9 after the revisions to the manuscript.

30. *L237 ". . . , the resulting ice sheet histories are quite similar." This is true for the integrated ice volume, but again I find this quite superficial and it could be different when we look at 2D fields.*
Our response: We have included a new figure (Fig. 4 above) to show the evolution of the grounding lines of each ensemble member at key times, and that further supports this statement that their histories are indeed fairly similar.

31. *L242 If it is problematic why did you include it?*
Our response: We have removed these from our study, as also requested by Reviewer 2.

32. *L256-259 This is discussion for me (see comment above).*
Our response: Indeed, we agree and have moved it to the discussion.

33. *L268-L274 This paragraph should rather be part of your experimental design section. By now there are so many simulations that you performed that I think it is really necessary to add a table where you list all the simulations with important forcing parameters in a table. It is really hard to keep track of the simulations.*
Our response: We agree with the reviewer; this paragraph felt out of place and was essentially recapping part of what we described in the methods. We have removed it. We appreciate the suggestion for a summary table, which we added to the end of the Methods section.

Table 4. Summary of performed experiments grouped by ensemble, listing their respective GI forcings, used sea level reconstruction and choice of initial geometry. LGMavg denotes that the GI was rescaled using the average LGM value as opposed to the peak value (cf. Sect. 2.3.1 and Table 5). The SGEN experiments were grouped for better visualisation, but each SGEN row corresponds to 3 experiments, one starting from each geometry (1 to 3).

Ensemble	Experiment	GI forcing	Sea level reconstruction	Initial Geometry
CFEN	lr04	LR04	Bintanja and van de Wal (2008)	control
CFEN	edc	EDC	Bintanja and van de Wal (2008)	control
CFEN	edf	DF	Bintanja and van de Wal (2008)	control
CFEN	vos	Vostok	Bintanja and van de Wal (2008)	control
SSEN	lr04lgmavg	$LR04_{LGMavg}$	Bintanja and van de Wal (2008)	control
SSEN	edclgmavg	EDC_{LGMavg}	Bintanja and van de Wal (2008)	control
RSEN	lp1bx	EDC (1 kyr low pass)	Bintanja and van de Wal (2008)	control
RSEN	lp3bx	EDC (3 kyr low pass)	Bintanja and van de Wal (2008)	control
SLEN	lp5bx	EDC (5 kyr low pass)	Bintanja and van de Wal (2008)	control
SLEN	s16l	EDC	Spratt and Lisiecki (2016) long	control
SLEN	s16s	EDC	Spratt and Lisiecki (2016) short	control
SLEN	s16u	EDC	Spratt and Lisiecki (2016) upper uncertainty	control
SLEN	spm	EDC	Imbrie et al. (1989)	control
SLEN	wae	EDC	Waelbroeck et al. (2002)	control
SGSEN	edcgmt[1-3]	EDC	Bintanja and van de Wal (2008)	gmt1-3
SGSEN	edfgmt[1-3]	DF	Bintanja and van de Wal (2008)	gmt1-3
SGSEN	vosgmt[1-3]	Vostok	Bintanja and van de Wal (2008)	gmt1-3

34. *L276 To me that is really surprising. From my experience, the initial geometry is quite important with regard to what your results look like at the end of the simulation. You glance over this, but this needs a discussion. Why do you think this is the case?*

Our response: The insensitivity to the choice of initial geometry of the WAIS seems to stem from the fact that the ocean is able to trigger its collapse regardless of its initial state. The EAIS, for example, showed a clear sensitivity to the initial geometry. We have included this in our discussion:

"The EAIS reacted sensitively to the choice of starting ice geometry, especially over Wilkes Land, where the spread in thickness and grounding line position is highest among its regions (Fig. 11). While drainage basins such as those of Totten and Dibble glaciers seemed to become more stable given a larger ice sheet, Cook glacier seems to thin regardless of the choice of initial geometry. Overall, the EAIS yields a contribution range of 2.4 to 3.7 m s.l.e. at 405 ka. Conversely, the WAIS was rather insensitive to the choice of geometry (yielding 4.3--4.5 m s.l.e. at 405 ka) due to the stronger role played by the ocean after 412 ka, which reduced its volume to a minimum in all simulations regardless of starting geometry."

35. *L279 ". . . also show similar rates of retreat ...". Again this is nowhere shown. I mean in Fig. 10 it looks like they actually have exactly the same grounding-line position. Is that true?*

Our response: We modified Figs. 3 and 10 of the original manuscript (Figs. 9 and 10 presented below), also based on comment 43. The grounding lines are indeed close to each other, but are not at the same position. Also, by "rates" we mean their pacing, and not the starting and final volumes, which can be seen in Fig. 9 in the original submission. We have changed the phrasing in the text to avoid misunderstanding.

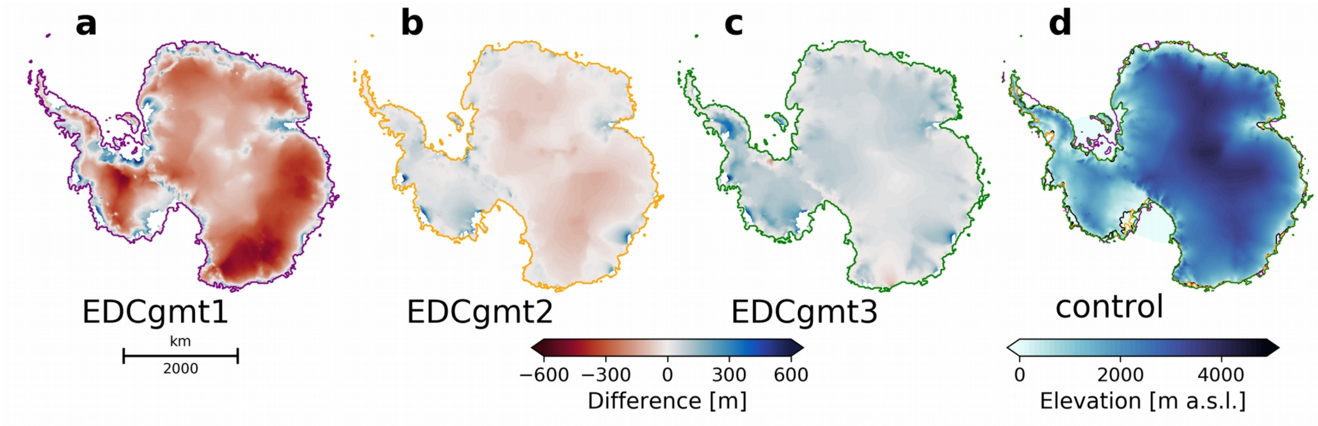

Figure 9: (a-c) Three different starting ice sheet geometries at 420 ka for the EDC CFEN member (gmt1-3). Color scheme shows differences in surface elevation between each geometry and the control for 420 ka (d). Differences are only shown where the ice is grounded in both geometries, and coloured lines show the respective grounding lines in gmt1-3, also overlain in (d). This is Fig. 3 after the revisions to the manuscript.

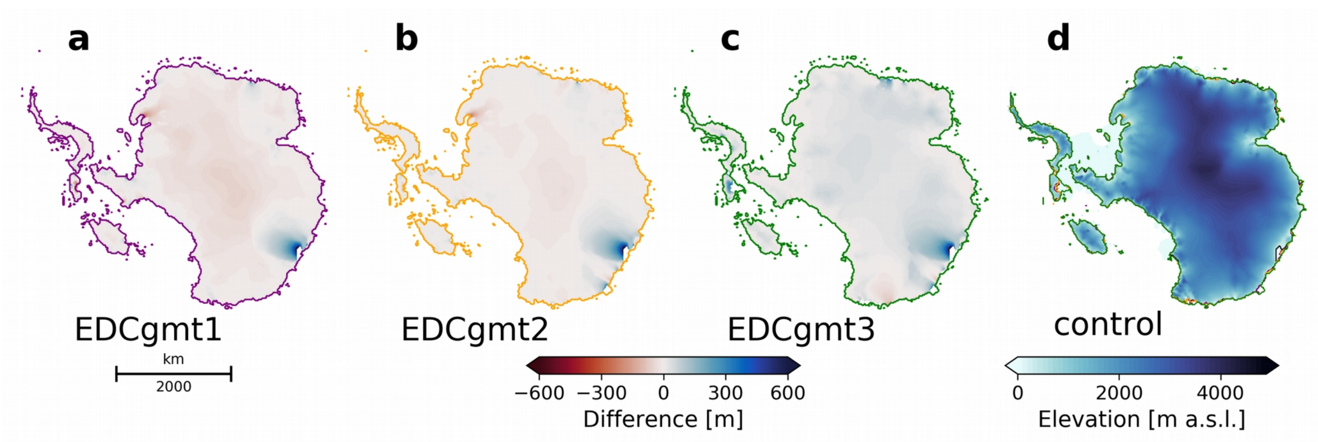

Figure 10: (a-c) ice sheet geometries at 405 ka for the EDC CFEN member using three different starting geometries at 420 ka (Fig. 12). Color scheme shows differences in surface elevation between each geometry and the control for 405 ka (d). Differences are only shown where the ice is grounded in both geometries, and coloured lines show the respective grounding lines in gmt1-3, also overlain in (d). This is Fig. 7 after the revisions to the manuscript.

36. *L282 Could you please add these locations to the respective Figure for better orientation.*
Our response: These locations were added to Fig. 1 (see our response to comment 3), so that it can be used as a reference for the locations cited in the manuscript, while the remaining figures can be less cluttered with text. Some glacier locations were also reviewed based on comment 12 from Reviewer 2, as we were originally pointing to adjacent glaciers instead.

37. *L289-301 This is a weak introduction to the discussion and repeats most of the material that you covered in the introduction. Consider removing it.*
Our response: We have removed it, while significantly reordering and rewriting most of the discussion, in light of the comments from both reviewers.

38. *L321 ". . . it seems that ice-shelf calving plays a role just as big". This again comes totally out of the blue and at the moment there is no way to check this statement as it is simply not described how calving is handled in the model. I also do not quite follow the explanation for this. Could the authors please elaborate on this?*

Our response: Calving in our model is done by a simple thickness threshold, where ice thinner than 50 m is calved out instantly. We have included this in our methods section (see our response to comment 1). Furthermore, in the rewriting of the discussion, this passage was removed. It no longer made sense to discuss calving there.

39. *L393 delete objective*
L406-407 Delete last sentence (see comment above).
Our response: We have removed the sentence and the word "objective".

40. *Comment hyphenation: I noticed that throughout the manuscript your use of hyphenation is inconsistent. You write ice-shelf calving, but then grounding line advance without hyphen. I am not sure what the TC policy is, but please make sure that you are at least consistent throughout the manuscript*
Our response: We thank the reviewer for noticing it, and have addressed the mistakes.

Figures

41. *Fig. 1: Please add a scale bar. Glacial index plots and their labels could be bigger.*
Our response: We have added a scale bar. The GI plots were removed from Fig 1 (see our response to comment 3).

42. *Fig. 2: Why do you show the time series until present-day? I think a zoom in into the period of interest would be better.*
Our response: Thank you for this suggestion. We have zoomed in to our period of interest, and added the GI plots from Fig. 1 in the original submission as Fig. 2b in this response letter.

43. *Fig. 3: It is really hard to see any differences in the upper panel (a-d) with the current colour scale. Also the grounding-line position should be made more prominent (thicker line or different color). In general there is too much white space and subplot labels (a-g) are too small. Please make each subplot bigger for better readability. Please also add a scale bar.*
Our response: The reviewer has a good point that it is hard to see differences in the upper panel, and a rescaling of the colorbar did not satisfactorily improve it. Thus, we have changed the figure to show only the control topography, and kept the difference plots to compare with the other geometries. We added thicker and colored lines for the grounding lines, which are plotted over their respective difference plot and over the control plot for an easier comparison. We have also added a scale bar as suggested. The same was applied to Fig. 10, which had the same style. Both figures were presented earlier in this letter as Figs. 9 and 10 (under our response to comment 35).

44. *Fig. 4: In the lower plot it looks like your model run for LR04 is not really in steady state or is your initial perturbation that large compared to your spin-up forcing? As mentioned above, I do not find the current y-axis units very intuitive for the lower panel. Labels (a,b) are too small.*
Our response: We never intended for it to be in steady state before this period, which is why we performed a thermal spin-up and gave it a relaxation period. Given the changes to the relaxation stage mentioned above, this figure has changed substantially, as shown in Fig. 3 of this response letter. A reference line indicating present-day Antarctic ice volume, suggested by Reviewer 2 (comment 36), helps put the presented numbers into perspective.

45. *Fig. 5: I do not really understand the point of this Figure as I do not get any information about the magnitudes of basal melting or the SMB. This Figure also needs a scale bar.*

Our response: We find that the changes incurred have improved the figure (see Fig. 7 in this letter, our responses to comments 26 and 29). We now show SMB for the grounded ice sheet, basal melting for the ice shelves, and added different hatching to where ablation occurs, and to where basal melting dominates over SMB at the ice shelves.

46. *Fig. 6: See Fig. 4*
47. *Fig. 7: Labels (a,b) are too small.*
48. *Fig. 8: Labels (a,b) are too small.*
Our response: We have increased the font size of all figures.

References cited in this letter that were not listed under the original manuscript submission

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