Dear editor, dear reviewer,

We thank the reviewer for their 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 after the changes made to the original submission of the manuscript.

Finally, we would like to draw the reviewer's attention to the correct reference to the first author's last name, as it is "Mas e Braga" and not "Braga".

General Comments

1. *"I find inconclusive the set of experiments to determine whether or not the duration of the interglacial is responsible for AIS retreat rather than a warm peak as for MIS5. This is because the index derived from ice core records mainly impact on the oceanic forcing of the simulation which generates a tipping point. Once the tipping point is crossed, then the duration of the interglacial does not matter at all to explain the amplitude of ice sheet retreat in the simulations. In simulations using Vostok-GI, ice volume is lower but because the GI does not yield too warm temperature."*

Our response: The reviewer has a very good point, which we missed in our original submission. We have made sufficient changes to our analyses to address the possible tipping point mentioned by the reviewer. First, we have added a figure (Fig. 1a,b in this letter) that shows the oceanic forcing under the main ice shelves as requested in comments 53 and 54, compared to the Summer atmospheric temperatures. Based on what this figure shows, there is indeed a tipping point where the ocean starts to rapidly warm up at around 412 ka, reaching temperatures up to 2 ºC warmer than the ice shelf base, as expressed by the thermal forcing (i.e., the difference between ocean temperatures and ice shelf temperatures at the ice/ocean interface). To investigate whether this is a tipping point caused by the increase in T_{forc}, we performed four new experiments. Two are based on the EDC ice core, one where we keep the climate constant before and after the suspected tipping point (at 416 and 410 ka respectively). The other two are based on the Vostok ice core, one where we keep the climate constant at its peak GI value, and one where we rapidly move the climate from its peak back to constant pre-peak conditions (although still warmer than the 412 ka conditions). These are shown below in Fig. 2. We will reformulate our discussion in light of these new results, which essentially show that the duration of warming was key for instigating strong WAIS retreat, while warming intensity (peak) allowed the retreat to be accelerated or delayed. There is indeed a threshold of 1.5 ºC which must be crossed for the WAIS to collapse.

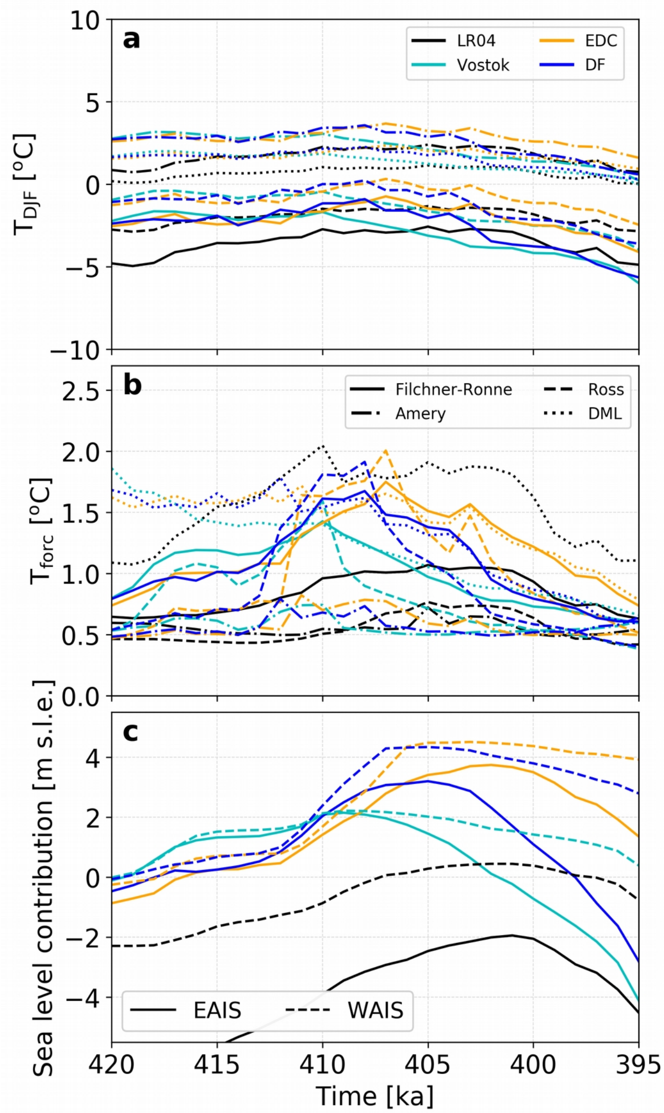

Figure 1: Evolution throughout MIS11 for each CFEN member averaged over specified Antarctic ice shelves for (a) Summer surface air temperature [ºC]; (b) thermal forcing under the ice shelves (i.e., ocean temperature minus ice base temperature, in ºC); and (c) sea level contribution by EAIS and WAIS. 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.

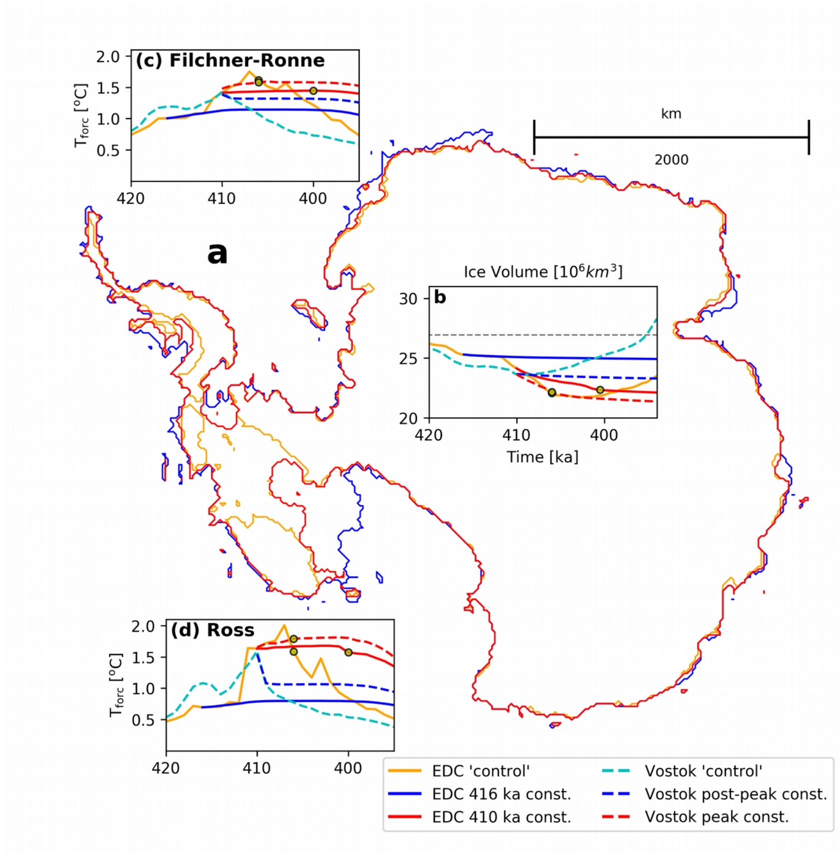

Figure 2: Thresholds for WAIS collapse. (a) grounding lines at 405 ka for three EDC-based experiments (solid lines, see below for explanation); (b) ice volume (10^6 km^3), (c,d) thermal forcing, i.e., the difference between ocean temperature and ice temperature at the ice/ocean interface (ºC), for the Filchner-Ronne and Ross ice shelves, respectively. Time series shown cover the period between 420 and 395 ka for both EDC (solid lines) and Vostok-based (dashed lines) experiments. Orange line shows the EDC control run, while cyan line shows the Vostok control run. Blue lines show EDC and Vostok simulations where climate was kept constant and the WAIS did not collapse, while red lines show EDC and Vostok simulations where climate was kept constant and the WAIS collapsed. Yellow circles show the moment when the WAIS breaks down and an open-water connection between the Ross, Weddell and Amundsen seas is established. This is Fig. 10 after the revisions to the manuscript.

2. *Actually, in question 2, ocean forcing is not mentioned at all as a potential driver of the ice sheet retreat. Same for ocean forcing: I would like to see a Figure in the supplementary of the oceanic forcing derived with the GI for the main ice shelves*

Our response: This is a good follow-up to the previous point. We now mention oceanic forcing in question 2, and Figs 1 and 2 in this response letter (which will be added to the revised manuscript) now show the ocean forcing for the main ice shelves. These figures underpin a discussion of our results regarding oceanic forcing, as suggested by the reviewer here and in subsequent comments, and will form the main basis for our discussion in the manuscript.

3. *You should discuss the impact to force WAIS with EAIS ice core records on the amplitude and timing of this retreat. For example, comparing them with WAIS divide ice core record.*

Our response: The WAIS divide record only spans the last 68 kyr, making a comparison between it and the used EAIS ice cores impossible for MIS11. What we suspect the reviewer is suggesting is that we add to our discussion that the WAIS could have responded sooner to changes in climate than the

EAIS, as the WAIS Divide ice core record shows a more than 2 kyr lead over the EAIS records (WAIS Divide Project members, 2013). We will include this in our discussion.

4. *In those simulations, all forcing co-vary: your surface climate forcing and your oceanic forcing are modulated with the same index. It is likely not the case as atmosphere cools or warms faster than ocean does. This is not accounted for here. You could perform some interesting tests that would provide a nice discussion about the interaction between ocean and ice sheet. A plot showing air and ocean temperature forcing versus WAIS ice volume evolution; same for EAIS for all simulations is really necessary to support or explain better some aspect of this manuscript and provide answers to questions 1 and 2.*

Our response: The reviewer offers another good point. To address this issue, we have added a lag to the ocean index (Fig. 3 below). We also performed three sensitivity experiments, one where its forcing is dampened by 50%, and two where the ocean is overall colder by 0.5 and 1.0 ºC (Figs. 4 and 5). Figure 1 presented above also shows how air temperatures and the thermal forcing vary throughout the study period, and how they relate to changes in WAIS and EAIS ice volume. These results and the other suggestions provided will also be incorporated in the discussion. Finally, because we added a lag of 300 years to the ocean response in all our experiments (which was the most probable response time of the ocean for this latitude; Yang & Zhu, 2011) we re-ran all the simulations shown in the manuscript.

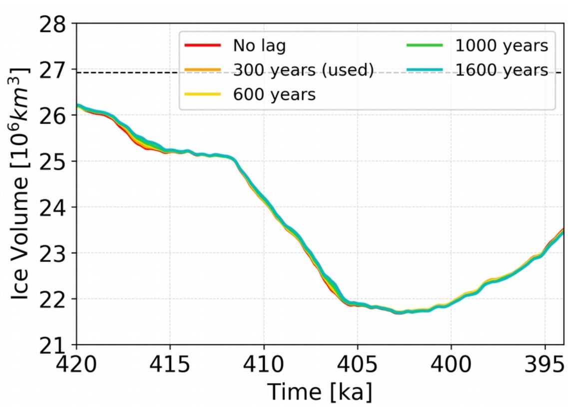

Figure 3: Sensitivity of the AIS response expressed in total ice volume [10^6 km^3] to different lags in the GI applied to the ocean between 420 and 394 ka. This is Fig. S9 after the revisions to the supplement.

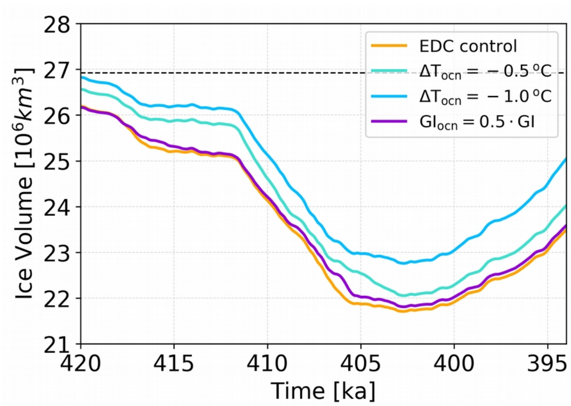

Figure 4: Sensitivity of the AIS response expressed in total ice volume [10^6 km^3] to three simulations where we test for the ocean sensitivity for a collapse. In two runs we apply a ΔT of -0.5 and -1.0 ºC, and in a third a reduction of the ocean GI amplitude by 50%. This is Fig. S6 after the revisions to the supplement.

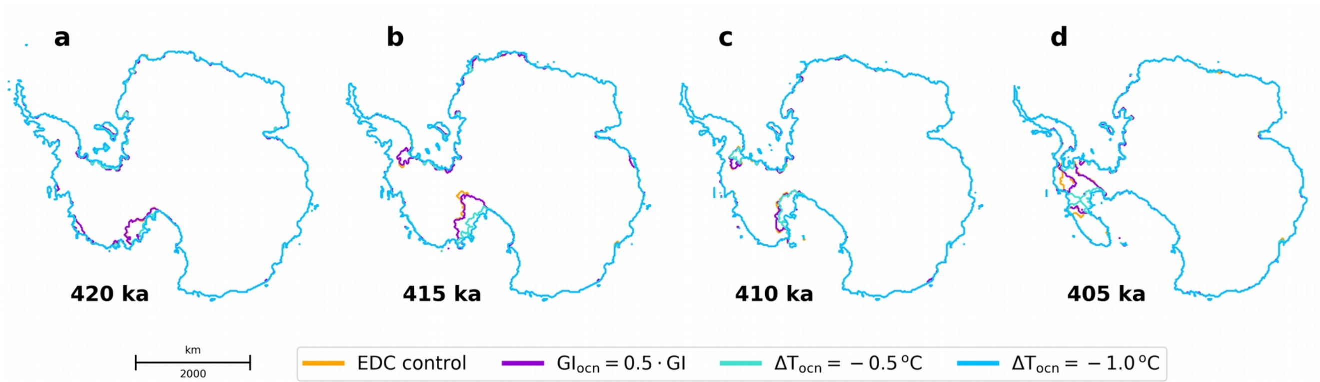

Figure 5: Grounding lines of the experiments presented in Fig. 4 at times of interest throughout the simulation. This is Fig. S7 after the revisions to the supplement.

5. *I would like to see a comparison between present-day simulated climate forcing and ocean forcing and observation from ERA5, and not between simulated PI and present-day ERA5."*
Our response: We appreciate the reviewer's concern here. We are unable to compare our model using present-day (PD) climate because we use an in-house simulation which does not have PD time slices. However, the difference between PI and PD in climate models is an order of magnitude smaller than the difference between either PI or PD and observational/reanalysis data (Otto-Bliesner et al., 2006, Fig. 2). Thus, we expect that not much would be gained if we were to compare the PD state as opposed to PI. We have, however, changed the comparison to CCSM3 from ERA5 to RACMO2 (see Fig. 15 in our response to comment 42), because the latter is a more accurate product for Antarctica (van Wessem et al., 2018). This change is also motivated by the changes based on the responses to comments 28 and 42.

Comments

6. *Line 47: please cite cite Tzedakis, P. C., et al. "Interglacial diversity." Nature Geoscience 2.11 (2009): 751-755.*
Our response: This paragraph was removed from our manuscript in response to comment 12 by Reviewer 1.

7. *Line 74: please cite De Boer et al. (2015) PLISMIP-ANT paper on which Dolan et al (2018) is largely based.*
Our response: We struggled to see how this citation fits in line 74 because we address ice-core reconstructions for MIS11c, while de Boer et al.'s paper is about model reconstructions for the Pliocene. We decided to add the reference to the previous sentence, which makes more sense.

8. *Line 74: please correct with "agree with how ANTARCTIC surface air temperature evolved"*
Our response: We have made the correction as requested.

9. *Lines 79-82: I strongly disagree with this paragraph. Lost of long-term transient simulations have been performed, including MIS11, and you cite all those contributions in your introduction. I think what you mean is that no study really tried to improve the current simulations of MIS11-AIS, neither with climate forcing or ice sheet modeling, in absence of geological constraints on both climate and ice* *dynamics. Please reformulate this way, this much more honest. State that your aim is to improve by exploring aspects on which nobody really focused on so far (e.g. the two questions you pose at the end of this paragraph).*

Our response: Thank you for this suggestion for better phrasing/framing. We rewrote the last two paragraphs of the introduction, also based on comments 15 and 16 from Reviewer 1.

"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 evolution and focus on aspects that remain un-addressed 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."

10. *Line 89: correct as follows "of uncertainties in sea level reconstruction, and of uncertainties of the geometry. . ."*

Our response: Corrected.

11. *Figure 1: If the starting AIS topography is present-day (BEDMAP2 or other) please state it in this caption as well.*

Our response: We mention it is based on Bedmap2, and refer to the section in the Methods where we explain our approach to creating it.

12. *Figure 1: Are you sure that the glacial tongue in the Wilkes Land corresponds to Ninnis Glacier and not to Mertz Glacier instead?*

Our response: Thank you for making us re-examine the precise features of Wilkes Land that were most affected, and their location in Figure 1: after more careful analysis, we conclude that these areas corresponded to neither Mertz nor Ninnis. The mainly affected glaciers in Wilkes Land are Totten, Dibble, and Cook. We have updated the text, figure, and caption.

13. *Lines 101-102: please invert the order of the two sentences (put together everything about ocean forcing and then put the rest).*

Our response: We removed the mention of salinity from the Methods section, moving it to the Supplementary Material. Please consult our response to comments 17-20, where the changes to the Methods section are detailed.

14. *Line 105: "i.e., apply a transient surface temperature signal from the EDC ice core (Jouzel et al., 2007)". But Jouzel et al. only provide a temperature anomaly, what is your baseline climate forcing here for this thermal spin-up tase and then for the 5,000 kyrs geometry adjustment afterward?*

Our response: We had initially applied the EDC core GI forcing to all simulations during the relaxation stage, so that they all had the same geometry at 420 ka. After this feedback (and from Reviewer 1, comment 20), we now apply the same GI forcing during the relaxation stage as the forcing during the main experiments (i.e., DF GI for the DF-forced runs, EDC GI for the EDC-forced runs, and so on). We have updated the text to reflect these changes and further clarify the point raised by the reviewer. It now reads:

"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. Figure 1 shows the thermally spun-up ice sheet configuration at 425 ka, from which the simulations start."

15. *Line 107: geometry is that of present-day, please specify which one and cite the reference (BEDMAP2, ALBMAP. . .).*
Our response: We now specify using Bedmap2, as shown above for comment 14.

16. *Line 107: "We then let the AIS freely adjust for 5 kyr, between 425 and 420 ka": what is the ocean forcing for this 5,000 years free run? It seems to me that the topography shown in Fig1 is really present-day. Is this really the AIS topography that you obtain after those 5,000 years of geometry evolution?*
Our response: We believe we have addressed this concern in the rewritten text shown above for comment 14. We have also updated Fig. 1 in the manuscript to show the post spin-up configuration of the ice sheet.

We group comments 17-20 because these are all addressed in the updated model description in the Methods section.

17. *Line105-107: Please detail ALL the forcing, boundary conditions (geothermal heat fluxes, etc..) used for the entire spin-up procedure this 5000 years (even in the supplementary if you prefer). All experiments presented here, including the spin-up, must be reproductible.*

18. *Table 1: Do you really use only one enhancement factor (the same for both SIA and SSA)? If yes please indicate it within the Table.*

19. *Table 1: what about calving? How is this done?*

20. *Table 1: Why is the relaxation time set at 1 ayr while characteristic time is 3 kyr? Please provide a detail description in the supplementary about the choice of your parameters. Also provide a description of the sliding law, surface mass balance in the Supplementary.*

Our response: We thank the reviewer for highlighting the need to further clarify our setup. We present an updated methods section including all information requested by both Reviewers (see also comments 1, 26, and 38 from Reviewer 1). We added information to Table 1, highlighting the use of the same enhancement factor, with a justification for this in the Methods section. For calving, we use a thickness threshold of 50 m, where ice at the calving front that is thinner than the threshold is instantly calved. We also refer Konrad et al. (2014), from which we obtain our ELRA parameters. Konrad et al., (2014) found them to yield the closest results to a fully-coupled ice-sheet-self-gravitating viscoelastic model. The expanded model description is shown 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."

21. *Table 1: Units for salinity is "PSU", please fill the missing units.*

Our response: We understand the reviewer's concern and confusion regarding salinity units, which is often very tricky. In the Practical Salinity Scale, introduced in 1978 (PS1978) salinity is a unitless quantity, since it follows a scale. The Practical Salinity Unit (PSU) was unofficially introduced and is invalid despite being accepted in some academic journals. Practical Salinity is calculated by its conductivity compared to Standard Seawater. Standard Seawater, in turn, is a reference manufactured by Ocean Scientific International Limited (OSIL). A brief review on salinity units, along with a list of technical papers on the matter can be found in OSIL's website, in the following link: https://osil.com/category/seawater-technical-papers/.

22. *Table 1: Please explain in the Supplementary how you choose the value for the thermal mixing coefficient (it varies quite a lot and this one of the main important parameter of oceanic parameterisation)*

Our response: We use the value of 10^{-4} ms^{-1} as it is the one presented in the original work of Beckman & Goosse (2003) and used in the ice-sheet model implementation of this parameterisation in Martin et al. (2011). This is clarified in the Supplementary Material.

23. *Table 2: Please substitute "Age scale" with "Age model".*

Our response: Done.

24. *Table 2: please provide a more detailed caption for this Table. What does "Age (ka)" corresponds to?*

Our response: We have updated the table caption, which now reads:

"Ice and sediment cores reference values used in Eq. (1), together with the age (in thousand years before present; ka) from which the reference values were obtained. The respective age models of each core, and their references, are listed."

25. *Table 2: Add a column to state what is the nature of the record (either dO18 or dD and it record is glaciological or marine).*

Our response: We added a new column to Table 2 where we show the core type (ice/sediment) and isotope ($\delta^{18}O$/δD).

26. *Subsection 2.2: In this paper your focus is on MIS11. Can you explain why you chose to scale the ice cores isotopic records to the difference between LGM and PI? Thus because of this, how much do your glacial index scaled surface temperature differs from the temperature form ice core records at DF, EDC and Vostok? I would like to see a Figure showing the derived surface air temperature for each GI and in comparison with each temperature reconstructions from dD for each ice cores used in this study.*

Our response: We chose LGM and PI for the scaling because they are the two best constrained periods available, which is especially important when combining the records with climate model forcings. The comparison between ice-core-inferred and GI-climate-model reconstructed temperatures was already given in Fig. S6. We have, however, improved the figure by removing the curves that were not related to this comparison (see Fig. 6 below). We will update the discussion to bring attention to this figure.

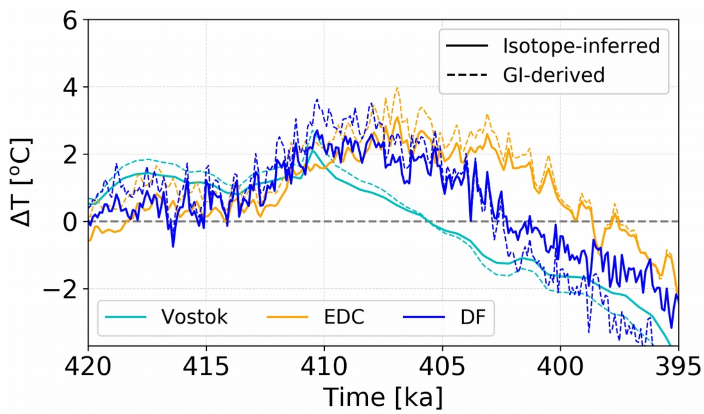

Figure 6: Comparison between surface atmospheric temperature anomalies (ΔT) obtained by the ice cores GI scaling and those inferred from their respective δD values. This is Fig. S12 after the revisions to the supplement.

27. *Lines 123-124: I don't understand the choice of CCSM3 since many other runs form CCSM4, even earlier versions of CESM, were released by Otto-Bliesner's group for contribution to PMIP3 on*

CMIP5 platform for both PI and LGM, run by NCAR, on the same computer. CCSM4 presents strong improvements relative to CCSM3. I would like to see a discussion about this and related literature for both version CCSM3 and CCSM4 in the Supplementary.

Our response: During early stages of this study we carried out both CCSM3- and CESM1.2-driven simulations, forced by the EDC-derived GI. In the end, we decided to use CCSM3 rather than CESM1.2 for two important reasons, which we also include in our supplementary material:

1. With CESM1.2 forcing (in-house simulations, see Bakker et al., 2020), the ice-sheet model failed to match the geological constraints for MIS11c by Raymo & Mitrovica (2012), i.e., not showing a volume loss that would cause the expected contribution from the AIS to sea level rise for this period (Fig.7, “CESM”). In order to understand what exactly caused this difference in performance between the two versions, we ran a series of sensitivity experiments where we replaced one forcing field (air temperature, ocean temperature, or precipitation; Fig.7) or two forcing fields (e.g. air temperature + ocean temperature; not shown) from CCSM3 by their CESM1.2 equivalent. We found that the evolution of the ice sheet throughout MIS11c was most sensitive to the differences in precipitation. The differences in precipitation fields show that CESM1.2 precipitation rates are, in general, lower than those of CCSM3 during MIS11c, especially in key areas of the WAIS, such as East of Siple Coast. As a consequence, the calibration of the ice sheet model to CESM1.2 forcing fields resulted in the need for a higher basal friction (relative to CCSM3) to compensate for the reduced precipitation in order to match the modern reference observational data sets. In turn, the combination of this higher basal friction and the higher precipitation rates during MIS11c compared to PI results in a much reduced sensitivity of the ice sheet to MIS11c warming, thus not capturing the AIS contribution to sea level rise shown by the geological record (Raymo & Mitrovica, 2012).

2. Several studies have shown that CCSM3 does a reasonably good job in simulating Southern Ocean conditions during glacials and intergalcials, which is important for the simulation of the AIS. It has been shown that CCSM3 correctly simulates characteristics of water masses produced in the Southern Ocean (AABW, AAIW) for the LGM (Otto-Bliesner & Brady, 2008; Ronge et al., 2015) and the transition into the Holocene (Ronge et al., 2020). Moreover, Marzocchi & Jansen (2017) have shown that CCSM3 has a better skill in simulating glacial Antarctic sea ice and deep-ocean circulation than CCSM4.

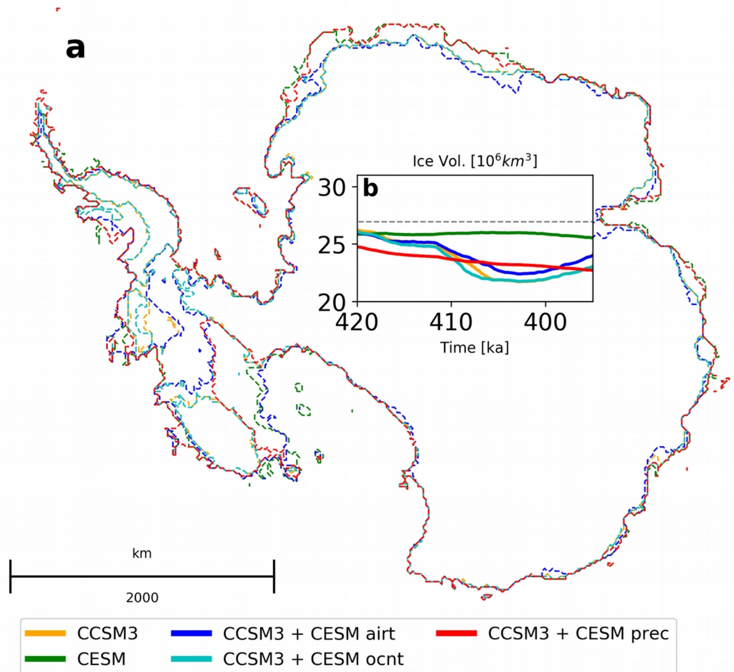

Figure 7: (a) Grounding lines (dashed for an easier comparison) at 405 ka (i.e., the MIS11c sea level highstand), and (b) ice volume [10^6 km^3] throughout MIS11c for a series of CESM1.2 and CCSM3 runs. 'airt','ocnt' and 'prec' denote atmospheric surface temperature, ocean temperatures, and precipitation rates respectively, and these refer to the runs where CCSM3 forcing variables were replaced by equivalent CESM1.2 variables (e.g., 'CCSM3 + CESM airt' means that all variables were from CCSM3, except for atmospheric surface temperatures, which were from CESM1.2). This is Fig. S3 after the revisions to the supplement.

28. *Lines 126-127: On the contrary, I would like to see a few panels about simulated Antarctic climate and associated biases since it is also highly important to your study. Thus I am expecting you to also provide a bias correction to your forcing field (assuming the bias correction propagates linearity back in time). This is something that you did not do, but it needs to be done. I also expect to see a figure of surface air temperature over Antarctica and comparison with all available ice core records for LGM (not only the few that you consider here), to have a comprehensive view of the performance of your climate forcing.*

Our response: As requested, we provide a map of the mean annual surface air temperature difference between LGM and PI, as simulated by CCSM3 (Fig. 8) along with temperature differences derived from ice cores (Werner et al., 2018). The CCSM3 results suggest a stronger cooling than the proxy data, which is likely related to a too thick Antarctic Ice Sheet prescribed in the LGM (PMIP2) simulations, and hence does not substantially affect our ice-sheet forcing due to lapse rate correction.

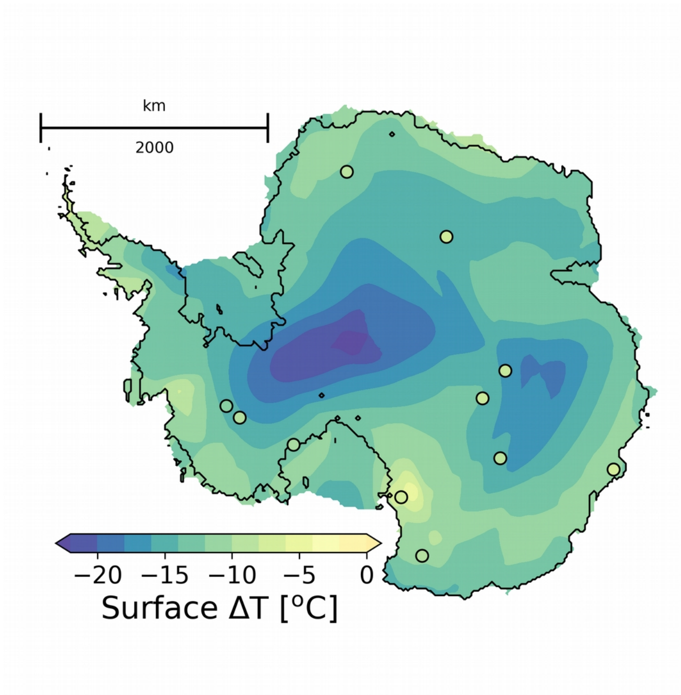

Figure 8: Surface atmospheric temperature difference (ΔT in °C) between CCSM3 LGM and PI time slices. Circles show the ΔT values derived from isotopes presented by Werner et al. (2018). This is Fig. S4 after the revisions to the supplement.

To assess how our forcing's biases impact our results, we have now performed an EDC-GI-forced simulation using RACMO2 as our modern reference fields, with CCSM3 providing the anomalies and ocean forcing. We also include a similar experiment using RACMO2 modern fields and CESM1.2 anomalies and ocean forcing (for the sake of completeness to the assessment presented in comment 27). In Fig. 9 we show how they differ from the simulations fully forced with CCSM3 (which we used in the manuscript) and CESM1.2 (which we presented in Fig. 7). Differences do exist between RACMO2 and CCSM3, but are relatively small and most evident during the time of sea level highstand at 405 ka. This difference in ice volume at 405 ka means that the RACMO2 runs contribute 1.1 m less to global mean sea level. The position of the grounding line shows that this difference seems to relate mainly to the fringes of the EAIS (particularly in Dronning Maud Land) and to the WAIS sector just south of the Peninsula. The runs in which CESM1.2 anomalies were applied yield a rather insensitive ice sheet. As already discussed in our response to comment 27, and considering that basal sliding conditions in these simulations were calibrated to RACMO2 climate, the 'RACMO+CESM' experiment further confirms that its precipitation anomalies are what makes it not capture the sea level contribution constraints for MIS11c in Raymo & Mitrovica (2012). Overall, the RACMO2-forced simulations do not decisively change our results, except for final sea level contribution calculations. This analysis is also added to the Supplementary Material.

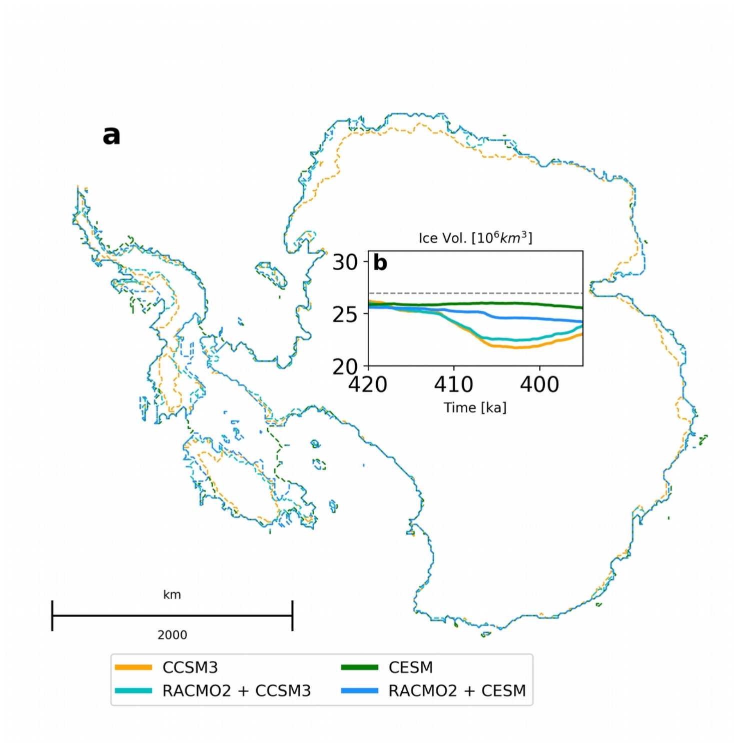

Figure 9: (a) grounding lines (dashed for an easier comparison) at 405 ka (i.e., the MIS11c sea level highstand), and (b) ice volume [10^6 km^3] throughout MIS11c for the bias assessment runs for CCSM3 and CESM1.2, where we test for the impact of using RACMO2 atmospheric fields as our modern reference, while keeping the ocean field and all anomalies from CCSM3 and CESM1.2. This is Fig. S5 after the revisions to the supplement.

29. *Line 134: Please substitute "age scale" with "age model".*
Our response: We have made the requested change.

30. *Line 140-141: I don't understand how you can compare dO18 from marine sediments and dD from ice cores and deduce that Holocene temperature history is inconsistent between those two. First of all, it is not straightforward to compare marine and glacial records togethers. To me this figure 2a does not make any sense, remove it.*
Our response: We changed Fig. 2 in the manuscript (based on comment 42 from Reviewer 1) by (i) zooming-in on the MIS11c period and (ii) by adding a panel with the different derived GI curves (see response to comment 3 by Reviewer 1). The idea is to provide a comparison between the isotope curves and their respective rescaled GIs (amplitude and structure), not a comparison between different isotopes. We removed comparisons between isotopes, such as referenced by the reviewer, from the manuscript, made changes to the text, and also removed the GI plots from Fig. 1 to avoid duplicating information.

31. *Subsection 2.3.2: Please refer to Figure 7 to show the filtered GI.*
Our response: We refer to Fig. 2b instead, as this is the panel where the filtered GI is shown after the revisions mentioned in comment 30.

32. *Subsection 2.3.3: I find the choice of your sea level curve a bit awkward. Why not considering also Waelbroeck et al (2002) which also encompasses MIS11 and which is one of the best curve we have with Bintanja et al. (2008).? Actually, many other new isotopic reconstructions have been done (e.g. Sutter et al., 2019), which is performed with more recent versions of models that Bintanja. Please redo some simulations also considering at least Waelbroeck et al (2002) in your ensemble.*

Our response: We added the Waelbroeck et al. (2002) record to our ensemble (SLEN, Fig. 10). Our results were not impacted by introducing this new simulation (which is hardly surprising, given that this is what this ensemble showed in the first place).

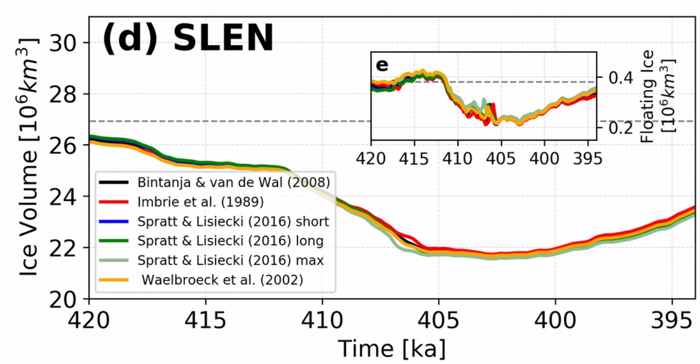

Figure 10: Sensitivity of AIS response (in total ice volume, 10^6 km^3) between 420 ka and 394 ka to the SLEN sea level reconstructions forced by EDC GI. These are Figs. 4d,e after the revisions to the manuscript.

33. *Subsection 2.3.4: The methodology to provide intermediate geometries is definitely highly science-fiction. One can provide geometries, even though idealised, but with a more appropriate approach. For example: you could have done an equilibrium simulation with LGM conditions scaled with your GI for a few tens of thousands of year, and then transiently vary your climate forcing as in your control experiment until beginning of MIS11. This is a much better alternative than what you propose here. Or, alternatively, you can start one glacial cycle ahead and transiently vary your climate forcing with your GI. Then you could have used your the various GI generated with your scaling ensemble to vary the slope of transition from glacial to MIS 11. I strongly suggest you to try this way since, at least, you can justify much better your geometry ensemble than how you defined it currently.*

Our response: In light of the reviewer's suggestions, we have changed to a more conservative approach regarding the creation of our different initial geometries, which is close to the first approach proposed by the reviewer. We now use constant LGM conditions and no ice shelf basal melt to grow the ice sheet towards an intermediate extent between PI and LGM in 5 kyr. We then place this intermediate-sized ice sheet at 420 ka (as was our old 'gmt1' ensemble member), at 425 ka, and at 430 ka, and let them transiently evolve since then. We have updated the figures accordingly, but overall, this change did not impact our results (see Figs. 11 and 12 below, which are updated version of Figs. 3 and 10 in the original submission). We have made the changes to Table 3, recalculated all the sea level contributions, and changed the text accordingly. The description of how we create our spread in initial geometry now reads:

"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 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 ice sheet 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).”

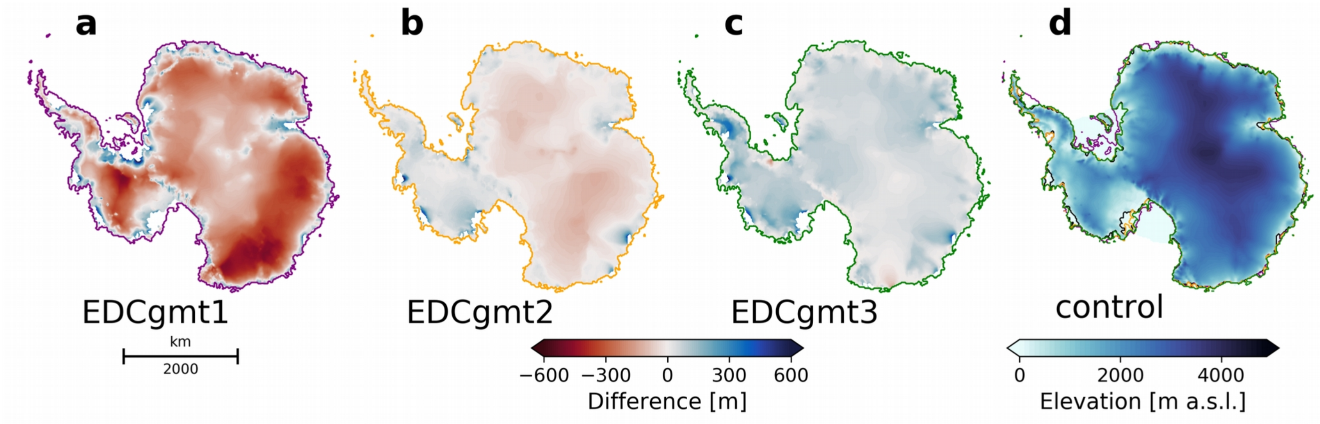

Figure 11: (a-c) Three different starting ice sheet geometries at 420 ka for the EDC CFEN member (gmt1-3). Color scheme shows differences in surface elevation between each geometry and the control for 420 ka (d). Differences are only shown where the ice is grounded in both geometries, and coloured lines show the respective grounding lines in gmt1-3, also overlain in (d). This is Fig. 3 after the revisions to the manuscript.

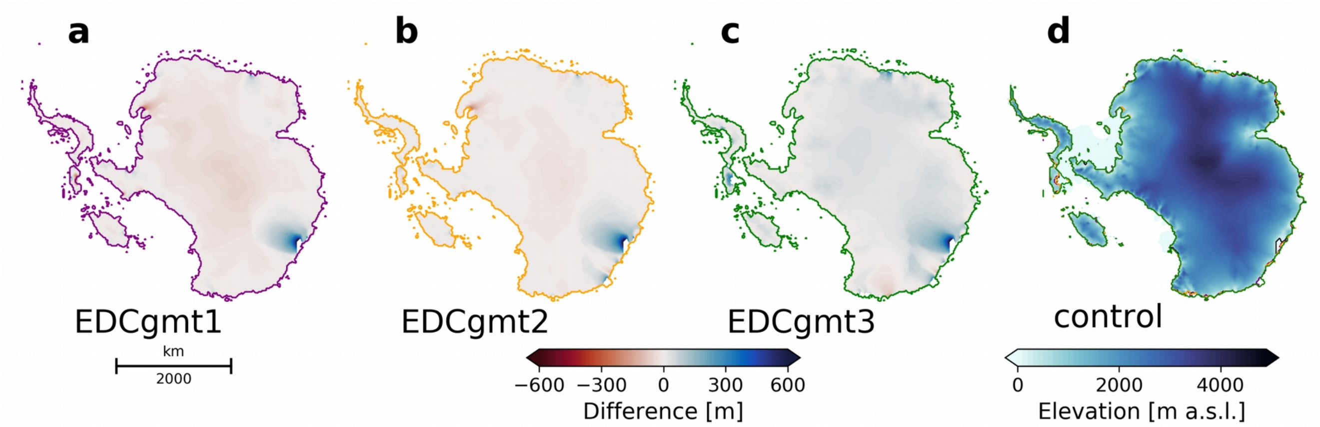

Figure 12: (a-c) ice sheet geometries at 405 ka for the EDC CFEN member using three different starting geometries at 420 ka (Fig. 11). Color scheme shows differences in surface elevation between each geometry and the control for 405 ka (d). Differences are only shown where the ice is grounded in both 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.

34. *Line 194: I think that the Figure number is wrong, it should not be Fig. 6.*
Our response: We thank the reviewer for spotting this typo.

35. *Line 191-204: I am not sure in which Figure I can see the corresponding GI. Figure 4? If yes, I don’t understand why you say that LR04-GI does not warm above PI temperature. PI temperature in Fig4a is given by 0 (the dashed horizontal line) right? To me LR04-GI goes beyond, even if not a lot.*
Our response: The reviewer is right. We have updated the corresponding section:

“Considering the four adopted isotope curves (Fig. 2a,b), although similar at first sight, the GI reconstructions are different from one another, and therefore offer a range of modelled ice-sheet responses. The LR04 GI reconstruction shows conditions warmer than PI only for the warmest period of MIS11c (i.e., between ca. 410 ka and 400 ka), and colder-than-PI before and after.”

36. *Figure 4: Please put a horizontal dashed line corresponding to present-day AIS volume (26.9 x10ˆ6km3).*
Our response: We have added a dashed line, and reference Bedmap2, which we believe is the source of the number suggested by the reviewer (and which we use as our model's initial topography).

37. *Lines 205-213: Actually, the amplitude of T ◦ increase between al curve is broadly the same, as shown on your Figure 4a. The difference resides in the fact that LR04-GI starts with colder conditions that the others. However the ice volume evolution also decreases for a long time event with LR04-PI, however, because initially it the AIS grows, then it can not retreat beyond present-day extent during the peak of MIS11c. Vostok-GI yields a decrease in ice volume of the same order than LR04-GI, about 2x10ˆ6 km3.*
Our response: As the reviewer highlighted, the main problem behind LR04 is that it is consistently colder than the other ensemble members during MIS11c. The relatively colder values we obtain are consistent with a bias towards Northern Hemisphere temperatures, which were found to be colder than PI during MIS11c. In line with the reviewer, and based on our results, it seems possible that an LR04-forced simulation would yield a WAIS collapse if its GI was somehow shifted towards the warmer conditions captured by the ice cores. However, at present we have no justification for adding such a shift. We reinforce this in our discussion by adding the following:

"The fact that MIS11c marine records show oxygen isotopic values similar to the Holocene (Lisiecki and Raymo, 2005) despite geological evidence showing that there was a contribution to higher-than-Holocene sea levels from both Greenland and Antarctica (Scherer et al., 1998; Raymo and Mitrovica, 2012) implies that, if true, the ocean must have been colder. Indeed, paleoceanographic records from the Nordic Seas, for example, indicate that they were colder than present during MIS11 (Bauch et al., 2000; Kandiano et al., 2016; Doherty and Thibodeau, 2018). Southern Ocean records remain equivocal about a warming of MIS11 relative to the Holocene (e.g., Droxler et al., 2003). The inclusion of many Northern Hemisphere records in the LR04 stack could explain why it does not capture an Antarctic warming event during MIS11c capable of driving ice sheet retreat. Thus, it is not surprising that the different criteria attempted for its scaling also had little effect (Fig. 4b), since it shows globally-integrated (overall colder) conditions for this period. A possible way of circumventing this problem could be by using a similar scaling approach to Sutter et al. (2019), who combined the LR04 stack and EDC ice-core temperature records, which led to a WAIS collapse during MIS11c."

38. *Line 218: What I see on Fig 4b is that there is a tipping point, a threshold from which the AIS retreats very fast. Thus, instead of warming rates, I see that when temperature reaches a certain threshold, the ice sheet reacts fast. For example, the Vostok curve is initially the warmest and thus the initial crease in volume is the strongest. Then the GI stabilises compared to the other and the volume decreases slow down. I would thus reformulate the analysis more in terms of tipping points and thresholds.*
Our response: The reviewer is absolutely right, and we are thankful that we were directed towards this important observation. We have updated our figures (Figs. 1 and 2 in this response letter), and will update the discussion so the analyses are more focused on the tipping points and thresholds observed in the oceanic forcing.

39. *Line 224: What about surface melt? Do you have any in your simulations? What method is used to calculate surface melt? Please provide detail about it in the Supplementary.*
Our response: We used a PDD model as described in Calov & Greve (2005). This was added to the model description, which was included as a response to comment 20. Based on Figs. 1 and 13 of this response letter, we show that some areas do experience surface melt, and that it is most relevant along the Dronning Maud Land margin. This will be mentioned it in the discussion, also comparing this region to the main ice shelves.

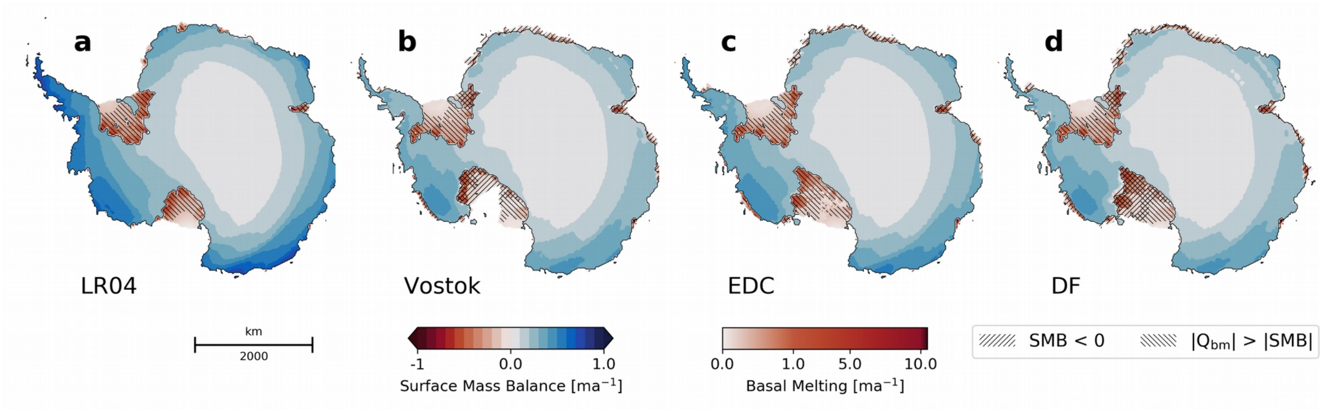

Figure 13: Surface Mass Balance (SMB) for the grounded ice and basal melting (Q_{bm}) for the ice shelves for the CFEN simulations at 415 ka. Hatched areas show where basal melting dominates over surface mass balance and where surface mass balance is negative (i.e., where surface ablation occurs) Everywhere where $|Q_{bm}| > |SMB|$ ice shelves are thinning. This is Fig. 8 after the revisions to the manuscript.

40. *Line 223-228: Could you provide a figure.*
Our response: We have addressed this with the changes made to Fig. 5 of the original submission, shown above as Fig. 13.

41. *Figure 5: I would be nice to have a contour for SMB= 0m/yr, so to understand which area are subject to surface melt.*
Our response: We agree with the Reviewer, but have added hatching where SMB<0 (see Fig. 13 above), which makes it easier to see these regions.

42. *Figure S2: you can't comparison between PI climate and ERA5 fields. . .this makes no sense. Please modify this figure and show a comparison between present-day CCSM3 fields and ERA5 instead. Same for basal melting comparison: you can't use PI fields and compare with present-day inferred basal melt rates from Rignot et al. By the way, Which reference did you used in c) for basal melt rates?*
Our response: The first part of this reviewer's comment was addressed in comment 5. We have added the reference to the present-day basal melt rates as requested, which were from Rignot et al. (2013), and now compare our fields to RACMO2 instead of ERA5, which is a more accurate representation of the Antarctic climate (Fig. 14). Including RACMO2 suits its use in other additions to the supplementary material following comment 28.

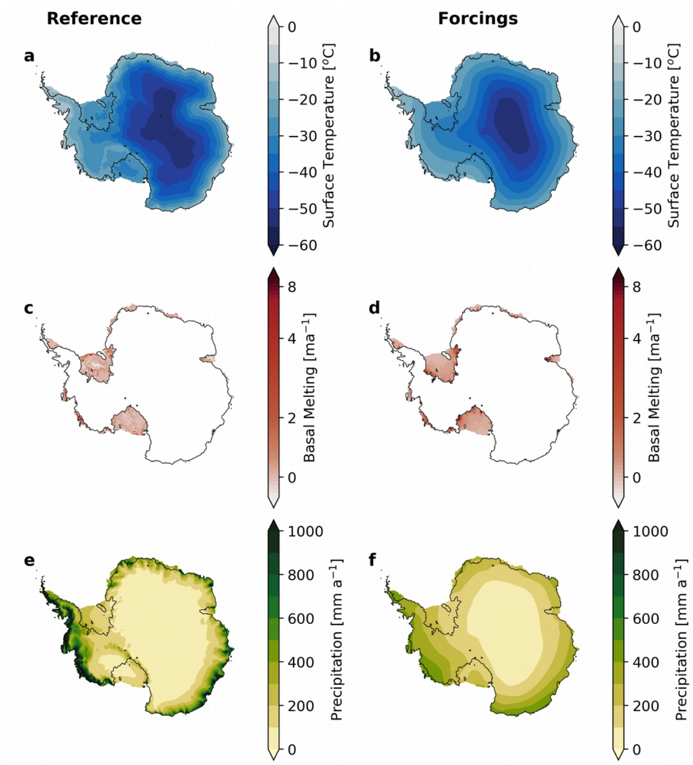

Figure 14: Comparison of CCSM3 forcings (right) to reference data (left) from RACMO2 (a,e; van Wessem et al., 2014) and of the calibrated basal melting to those of Rignot et al. (2013). (a,b) surface temperature [ºC]; (c,d) basal melting [ma^{-1}]; (e,f) annual precipitation [mm a^{-1}]. This is Fig. S2 after the revisions to the supplement.

43. *Line 241-244: Thus why did you use an average over the last 10 kyrs. . .this does not make sense, because orbitals are varying so much. Please remove the corresponding results from the manuscript.*
Our response: We have removed these results.

44. *Line 249: I disagree. Trajectories are the same, they are only delayed, please reformulate.*
Our response: We have removed this sentence because of the changes in our ensemble and for reasons explained in the comment below.

45. *Line 254: "This effect seems to be non-physical, and a result of the delay introduced by the low-pass filter. " âĂŤ-> The effect is physical, this is the result of your delayed curve. Please remove this sentence. Because it is not the point here.*
Our response: This was pointed out by the Editor during the first screening process. We considered that the best way to address this was to use an alternative method for low-pass filtering, and re-run this ensemble of simulations using a box-filter, which does not yield the delay seen. Figure 15 shows the new results, which do not impact our inferences regarding the impact of high-frequency variability. Sect. 3.3 now reads:

"The trajectories of each ensemble member in RSEN agree very well with one another (Fig. 4c), showing slightly increased delays in retreat due to the filtering process. Also, although it is possible to see slight differences in ice sheet volume between each ensemble member (the volume is larger the more filtered the forcing is), it is negligible compared to the overall changes in volume experienced by the entire ensemble."

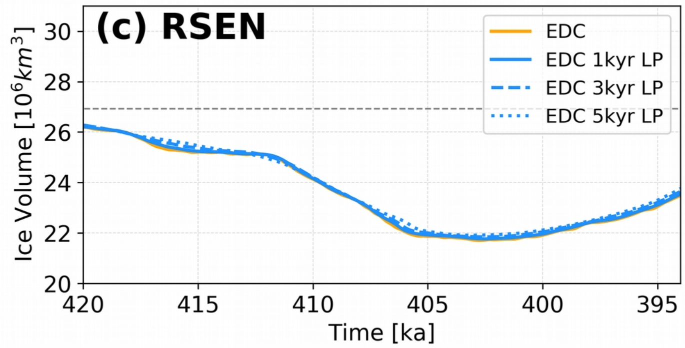

Figure 15: Sensitivity of AIS response (in total ice volume, 10^6 km^3) between 420 ka and 394 ka to the RSEN low-pass filtered GI reconstructions. This is Fig. 4c after the revisions to the manuscript.

46. *Line 256-259: "The 1 kyr low-pass GI is the only one that still preserves some higher-frequency variability ". I don't this on the Figure, I disagree. None of the filtered curve preserve the high frequency visible on the original EDC record.*

Our response: The reviewer is right, and this has been addressed with the changes shown above.

47. *Subsection 3.5: the only difference visible is before the threshold at 412k for EDC and DF index. This is because there is this threshold that initial geometry does not impact on your results. Basically, ocean forcing is driving all your scenarios. To see the difference in initial ice sheet geometry, you should turn-off the ocean forcing. But this wouldn't make sense. So the conclusion here is that ocean forcing is driving the initial trajectories until 412k, the tipping point. Thus is it not surprising that initial geometry does not matter too much. There is one thing you have not tested though here, is the variation in ocean forcing. Those tests makes also a lot of sens because ocean forcing has this tremendous effect on your simulations. Thus I would like to see a couple of other transient simulations with lower ocean forcing. And thus, try again your geometry scenarios with the difference ocean forcing rather than with EDC-GI or DF-GI.*

Our response: We appreciate the highlighted importance of ocean forcing, and the lack of sensitivity experiments. Hence, we address this concern by performing an ensemble forced by the EDC GI with three simulations: Two where we lower the ocean temperature by 0.5 and 1.0 °C, and one where we reduce the ocean GI amplitude by 50%. We include the original ensemble member for comparison. We show the resulting ice volume (Fig. 4) and the grounding lines (Fig. 5) at different times of interest (see our response to comment 4). In short, the tipping point at ca. 412 ka still persists despite the fact that the thermal forcing is substantially colder, but a total collapse of the WAIS is not achieved using a forcing with reduced temperatures. In light of these results, we believe that the ocean is driving our simulations *after* 412 ka (as evidenced by Figs. 1 and 16), which is why the different ice-sheet configurations converge to a similar geometry.

In our tests for different oceanic forcings, there is no connection between the Ross and Weddell seas at 405 ka for the ΔT_{ocn} = -0.5 °C run (Fig. 5d), but a narrow passage is established between the Ross and Amundsen seas. A comparison of the thermal forcing below the Ross and Filchner-Ronne ice shelves (Fig. 16) shows the stronger effect in the Ross sector and further strengthens a 1.5 °C warming below the ice shelves as the threshold for which WAIS collapse is possible, as we postulate in the original manuscript and in our response to comment 1. This discussion is added to the supplementary material.

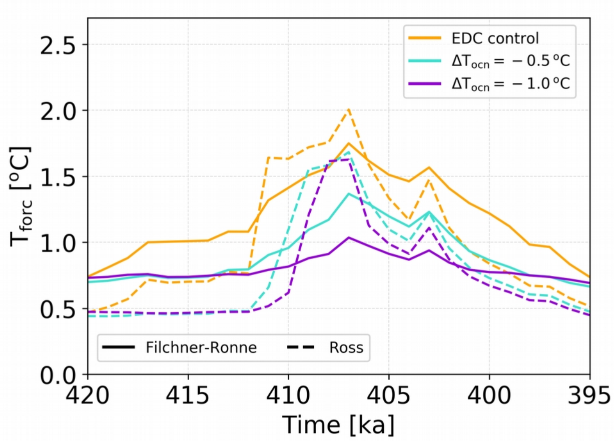

Figure 16: Thermal forcing (i.e., the difference between the ocean temperature and the ice shelf base temperature) averaged over the Ross and Filchner-Ronne ice shelves for the ΔT experiments presented in Figs. 4 and 5. This is Fig. S8 after the revisions to the supplement.

48. *I also would like to see a figure in the supplementary showing the Tforcing for each GI.*

Our response: We have added the requested figure to the supplement, and below as Fig. 17. Also, Fig. 1 of this response letter shows how T_{forc} evolves under the main ice shelves, and the overall response to the mentioned tipping point.

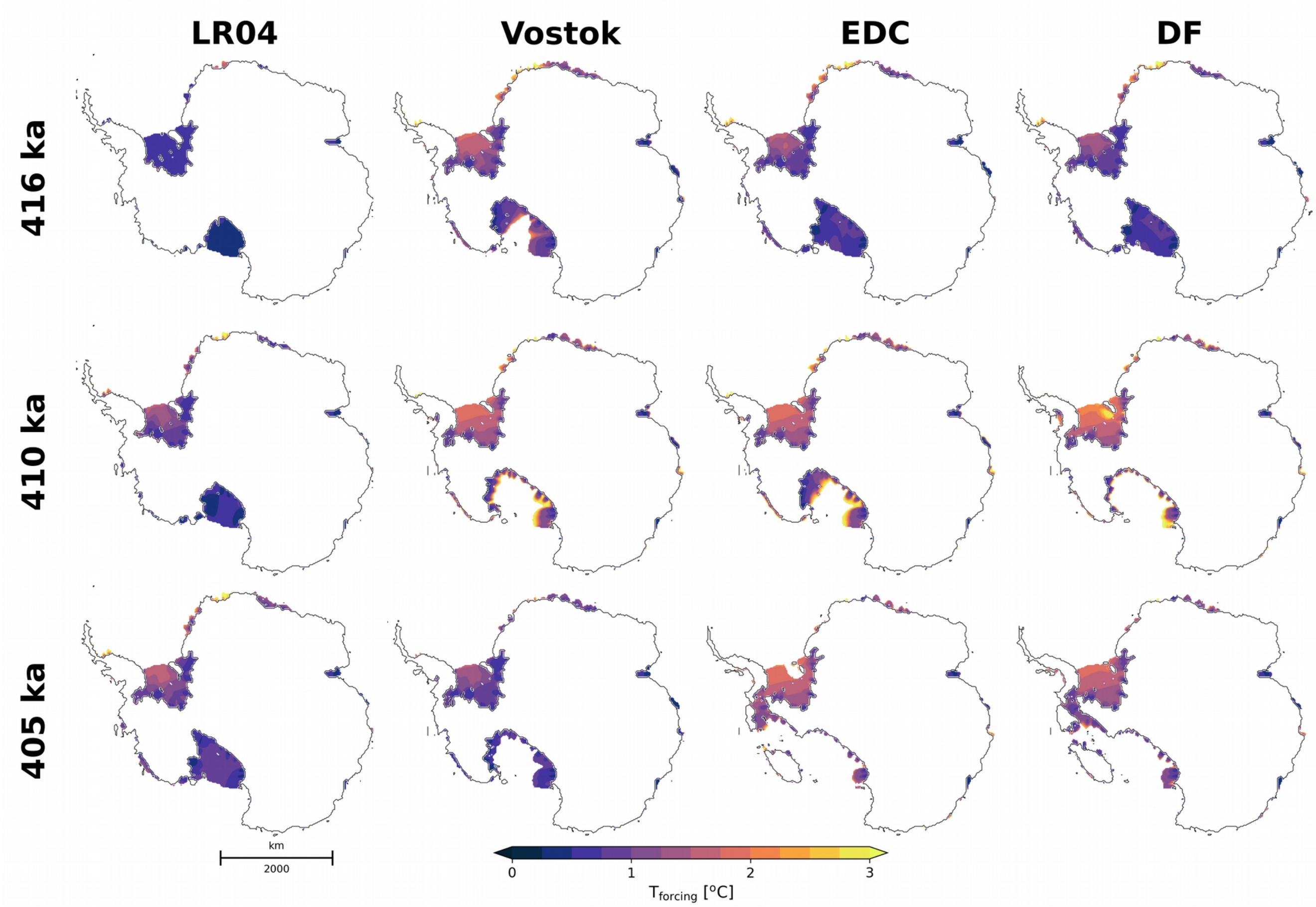

Figure 17: Thermal forcing (i.e., the difference between the ocean temperature and the ice shelf base temperature) at time steps of interest for each of the CFEN members. This is Fig. S13 after the revisions to the supplement.

49. *Figure 12: Please also add total AIS sea level contribution on the figure for each geometry.*

Our response: We have added the total AIS sea level contribution for each geometry to the figure (see Fig. 18 below).

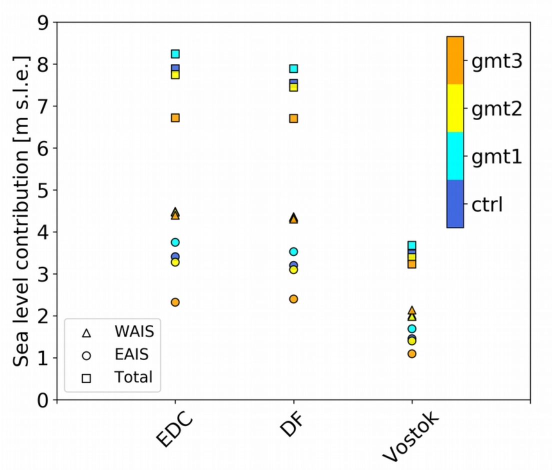

Figure 18: Sea level contribution (in m s.l.e.) of each SGSEN member during the global sea level highstand at 405 ka. This is Fig. 11 after the revisions to the manuscript.

50. *Line 321-323: Please show some calving fluxes against oceanic warmth because you never really discuss calving, neither describe the calving method used here. Put this in the supplementary.*

Our response: Figure 19 shows the calving fluxes for the main ice shelves versus T_{forc}. The mention of how calving is treated in our model is presented in the response to comments 17-20. We have added the requested calving plot to the Supplementary Material, and reference it when discussing the main forcings of ice loss in our experiments:

Given the simplistic treatment of calving in our model, ice-shelf basal melting is the main source of ice loss in our experiments. The resulting strong thermal forcing at the ice-shelf fringes (Fig. S13) is what drives the thinning necessary for calving to take place, making it essentially reflect the behaviour of the ocean forcing (Fig. S14).

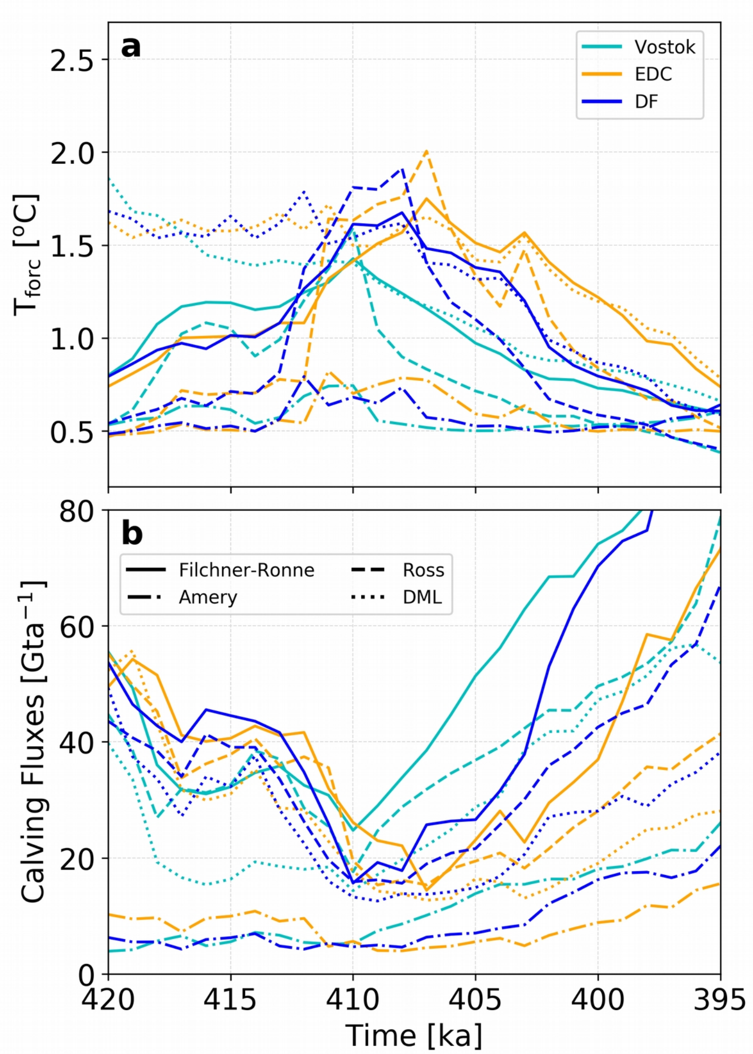

Figure 19: (a) Thermal forcing (i.e., the difference between the ocean temperature and the ice shelf base temperature) averaged over the main ice shelves for the CFEN experiments presented in the manuscript. (b) Calving fluxes (in Gt/a) integrated along the main ice shelves' calving fronts. DML encompasses all ice shelves in Dronning Maud Land. This is Fig. S14 after the revisions to the supplement.

51. *Line 340-353: I really would like to see a specific figure in the Supplementary of temperature forcing derived from GI for each ice core records and compared with the temperature reconstructed from dD of those ice cores.*

Our response: This has been addressed in comment 26.

52. *Line 349-350: I completely disagree with this statement. On your Figure 1, you can definitely see that this is because EDC-GI and DF-GI yield temperature warmer than those I Vostok and thus it is a matter of tipping point rather than duration. . .*

Our response: The reviewer offers a very good point. After a renewed assessment (see response to comment 1), we show that a collapse is possible for temperatures above the mentioned threshold if they are sustained for long enough. In short, there is indeed a threshold that must be crossed (which happens after the tipping point at 412 ka mentioned by the reviewer in comment 47), but it needs to be sustained for long enough to trigger the collapse.

53. *Line 391-392: "WAIS collapse was caused by the duration rather than the intensity of warming ". I don't see how you can conclude this here. I find the entire set of simulations rather inconclusive for this aspect. There is a tipping point in all the simulations shown in Figure 4. However, the amplitude of contribution to sea level is determined then by the magnitude of the warmth during the peak rather than the duration of the peak itself. Actually the ice sheet retreat in a very comparable way when using EDC and DF, which have a different peak duration. . .IN fact there is almost no significant difference between them in Figure 12 as well.*

Our response: The reviewer is right about the existence of a tipping point. As we showed in our responses to comments 1, 4, and 47, this marks the point when the ocean forcing becomes the main driver of retreat. However, as shown in our response to comment 1, the length of warming is still a decisive factor for the WAIS collapse, but both EDC and DF fulfill this criterion. EDC has a longer warmth period, and thus contributes slightly more to sea level. We will include the importance of tipping points and the relationship between length and sea level contribution in our discussion.

54. *Line 395-400: Instead of just stating it, show it. Plot air and ocean temperature forcing versus WAIS ice volume evolution; same for EAIS*

Our response: We have provided this information in Fig. 1 in this response letter, which also helps restructure our discussion around possible tipping points and thresholds.

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