We thank Jorge Alvarez-Solas for his thorough review on our paper and for his constructive comments and remarks. We hope that we are able to address them in a satisfactory manner and discuss the modifications we introduced motivated by his remarks below. Please find all changes and the new or modified figures in the attached manuscript tracked in red. We also included the main changes below each comment in this document in red and our response to the reviewer's comments in blue.

1) Main goal of the study:

I think this study nicely focuses on providing new information on where to look for the oldest Antarctic ice within the framework of continentally wide ice-sheet modeling. And it is also appreciable to subscribe such an approach within the context of transient simulations covering the MPT.

Nevertheless, several parts of the abstract and the introduction can be misleading because they are somehow suggesting that an important part of the paper will be devoted to analyze the effects of parametric uncertainty on the Antarctic ice dynamics during the MPT.

For example, the abstract reads: "We discuss the effects of changing climate conditions, sea level and geothermal heat flux boundary conditions on the mass balance and ice dynamics of the Antarctic Ice Sheet." This is not strictly true, particularly so with respect to ice dynamics (there is not a single panel or comment devoted to ice velocities or changes in the ice flow dynamics as a result of the changing climate). Changes in the grounding line position (also very superficially tackled), basal temperature and thickness evolution are related to changes in ice velocities, but this relationship is not addressed in the paper.

Similar misleading sentences to that of the abstract mentioned above can be found throughout the introduction (e.g. page 2, lines 27-29) and methods (page 7, lines 5-7).

I suggest two different ways of mitigating this issue:

a) Lower the reader's expectations concerning ice dynamics.
b) Expand the analysis on the effects that different parameters controlling the ice flow have on the conclusions of the study.

We concur with the reviewer, that the use of the term "dynamics" can potentially misguide the reader's expectations. While we do discuss e.g. grounding line migration in the Last Interglacial we do not dive into the intricacies of e.g. ice flow changes and individual rerouting of outlet glaciers. We rather put the focus on ice thickness changes around Oldest Ice candidate sites and analyse ice volume throughout the last 2 million years as an integrative measure of configurational changes of the Antarctic Ice Sheet. We appreciate the reviewer's suggestion to rephrase certain parts of the manuscript to lower the readers expectations with regard to the term "dynamics". While rewriting the manuscript with a detailed focus on ice dynamical changes during the last 2 Million years would be certainly worthwhile, we think that this would be better suited for a separate manuscript and would divert too much attention from the main focus of this study: to identify potential Oldest Ice sites for the IPICS endeavour.

We rephrased all potentially misleading sentences containing the term "dynamics" in the manuscript.

2) Reproducibility and description of the methods:

The manuscript contains a large amount of small inaccuracies, typos and erratum, particularly so in the methods section. These, taken individually, do not constitute a major problem, but taken together have a double negative effect: i) do not allow for the suitable reproducibility of this study, and ii) make the reading of the manuscript frustrating.

SPECIFIC COMMENTS

1) Ice-sheet model and ensembles

Page 4, line 29 reads: "[...] we linearly scale the computed present day melt rates in the Amundsen and Bellinghausen Sea by a factor of 10 and underneath the Filchner Ice Shelf by a factor of 1.5. Shelf melt rates adjacent to Wilkes, Terre Adelie and George V Land in East Antarctica are also scaled by factor of 10."
What do these scaling factors do?
Are they simply multiplying (or dividing) the observed values? Please define.

This is indeed not self explanatory, we therefore add a short sentence clarifying, that the scaling factors multiply the computed melt rates.

P5, l8 -

To better match present day observed sub ice-shelf melt rates (Rignot et al., 2013; Depoorter et al., 2013), we had to multiply the computed present day melt rates in the Amundsen and Bellinghausen Sea by a factor of m_b=10, around the Antarctic Peninsula by 5, and underneath the Filchner Ice Shelf by a factor of 1.5. Shelf melt rates adjacent to Wilkes, Terre Adelie and George V Land in East Antarctica are also scaled by factor of 10 in a subset of the simulations.

Furthermore, the above quoted sentence seems incongruent with having a parameter between 1 and 10 as a part of the ensemble controlling the basal melt values (gamma_EAIS [1;10]).
Caption of table 2 refers to George V and Wilkes Lands, while the above scaling factors refer to other basins as well.
Thus, are the values of the shelf melt being changed as a part of the ensemble? If yes, both basins simultaneously?
On the other hand, if these scaling factors reflect the uncertainty associated to the processes that determine shelf basal melt in time, why not exploring the factors of the rest of the basins as well? Please define and clarify.

We agree with the reviewer that this is unclear in the manuscript. Table 2 is now revised according to the suggestions of both reviewers and provides the two main parameter sets discussed in the manuscript, while additionally providing further parameter combinations used in the ensemble but not discussed in the results. We now address specifically which ensemble members are mainly analysed in the study (p10, l4-11) and changed table 2 (p10) accordingly. Table 2 now provides the two main parameter sets used for all sea level and geothermal heat flux combinations in ensemble B1 and for all geothermal heat flux combinations in B2. We further provide additional parameter combinations simulated in the ensemble but not discussed in the manuscript in the third line of table 2. We hope this clarifies the main scope of the ensemble.

P10, l4-11

The constituting forcing set for the ensemble consists of four different geothermal heat flux and three sea level data sets, i.e. twelve individual experimental settings. We further explore two main parameter sets (P1 and P2) highlighted in table 2. While we do take into account all sea level variations for ensemble B1 (48 individual experiments), we only look at the sea level forcing derived from Lisiecki and Raymo (2005) (LR05) in ensemble B2. We also experimented with other parameter choices based on table 2 (VP) but not covering all individual forcing sets, thus these are not discussed in this study. The ensemble members discussed in this manuscript consist of 8 experiments for each ensemble B1 and B2 with sea level forcing from LR05.

Table 2. Selected ISM parameters for the model ensemble. First and second line show the main parameter sets used in the ensemble (P1 and P2). The third line lists additional parameters tested but not further explored (VP). cH stands for thickness calving limit (in meter), cE is a parameter in the Eigencalving equation. sia_e and ssa_e stand for the so called sia and ssa "enhancement factors", $till_{min}$ and $till_{max}$ modify basal friction in the sliding law. γ_{EAIS} is a dimensionless scaling factor for basal shelf melt for selected East Antarctic ice shelf regions (George V Land, Wilkes Land).

Parameter	sia_e	ssa_e	cH (m)	cE	$till_{min}$	$till_{max}$	γ_{EAIS}
P1	1.0	0.55	**75**	$1 \cdot 10^{17}$	5	30	**10**
P2	1.0	0.55	**150**	$1 \cdot 10^{17}$	5	30	**1**
VP	1.6 ; 1.7 ; 2.0	1.0	100	$1 \cdot 10^{18}$	10	40	5-20

How is the shelf basal melting evolution in time achieved in the model? Is it dependent on the temperatures of the ocean given by equation 2?
If yes, how is it done? Perhaps an anomaly method with respect to the scaled PD basal melt defined in page 4, line 29?
Please define.

Basal melt rates are calculated according to the formulation in Beckmann and Goosse (2003) with a square dependency on the temperature difference between the pressure dependent freezing point and the ambient ocean temperature as used in e.g. Pollard and DeConto (2012). We added the melt rate equation on p5. The ocean temperature over time is derived as shown in equation 4.

$$M = m_b 0.005 \frac{\rho_w c_{pw}}{L_i \rho_i \gamma_T} |(T^{3D}_{ocean} - T_f)|(T^{3D}_{ocean} - T_f) \quad (1)$$

where M is the melt rate in m/s, m_b is a scaling factor, ρ_w and ρ_i are ocean water and ice shelf density, respectively, c_{pw} is ocean water heat capacitiy, γ_T heat transfer coefficient, L_i latent heat, T_f freezing point at depth of ice and T^{3D}_{ocean} ambient ocean temperature. The ambient ocean temperature is derived from simple extrapolation of the 3D ocean temperature into the

P4, L32, calving parameterisations:
Are these two parameterisations (threshold and eigen) exclusive to each other?
If yes, then specify in the ensembles description that you explore the use of two different calving "laws", the threshold one with two values of its parameter and the eigen one with only one value.
If no, then please remove the eigen parameter value to the table summarizing the explored values of the parameters for the ensemble.

We rephrased the description of calving, hopefully clarifying that both parameterizations act simultaneously (p5, l12-17).

P5, l12-17

These scaling factors are kept constant throughout the paleo simulations. Ice shelf calving and therefore the dynamic calving front is derived via two heuristic calving parameterisations: 1. thickness calving (cH) sets a minimum spatially uniform ice thickness (75 m or 150 m) at the calving front, if the ice thickness drops below this threshold, ice in the respective grid node is purged; 2. independently of 1. we additionally employ Eigencalving (cE), which calculates a calving rate from the ice shelf strain rates (Albrecht and Levermann, 2014). Both calving parameterizations are active simultaneously throughout the simulations.

Table 2 shows the explored values of two parameters that are not described nor mentioned at all in the text: "sia" and "ssa". By looking at the values, I assume these refer to the enhancement factors of the SIA and the SSA parts of the simulated ice sheet. They need to be defined and properly named (sia and ssa are not parameters per se but approximations).

See changes applied to table 2 and caption table 2.

Table 2 shows the parameters till_min and till_max. These are not defined nor described in the text. The reader can only speculate about the possibility that these parameters have something to do with the description of page 3, last line: "[...] the yield stress (tau_c) is determined by the pore water content and the strength of the sediment which is set by a linear piecewise function dependent on the ice-bedrock interface depth relative to sea level". If the reader keeps digging and tries to identify these parameters in the references of the model given here, will still not succeed because no mention to "till_max" and "till_min" can be found in Bueler and Brown 2009, nor in Winkelmann et al, 2011. I had to go to Martien et al, 2011 (The Cryosphere) and assume that "till_min" and "till_max" correspond to the upper and lower numbers given by their equation 10. Is this correct?

Please define, clarify and cite accordingly.

The role of till_min and till_max was indeed not evident from the methods section, we added a short reference to the respective equation in Winkelmann et al 2011.

P4, l8-10

The relevant parameter for this approach is introduced in PISM via the till-friction angle (see Winkelmann et al. (2011) eq. 12) which is scaled linearly between $till_{min}$ and $till_{max}$, depending on the bedrock elevation.

1) Glacial index description.

Having the glacial index in Figure 5 and knowing that you use 3 climate snapshots weighted in time by such an index, one can have an idea of how you are forcing the ice sheet model.
However, the description of the method (including equations 1 to 5) is a bit odd. Perhaps it would be simply solved by providing the values of GI_pd and GI_max (as far as I saw the value of these parameters is not given in the manuscript).

We agree, that the description of the climate forcing should be expanded. To this end we introduce a plot which illustrates how the glacial index is implemented (p.7 new Figure 2). We also correct several typos in equations 5-7 (p6).

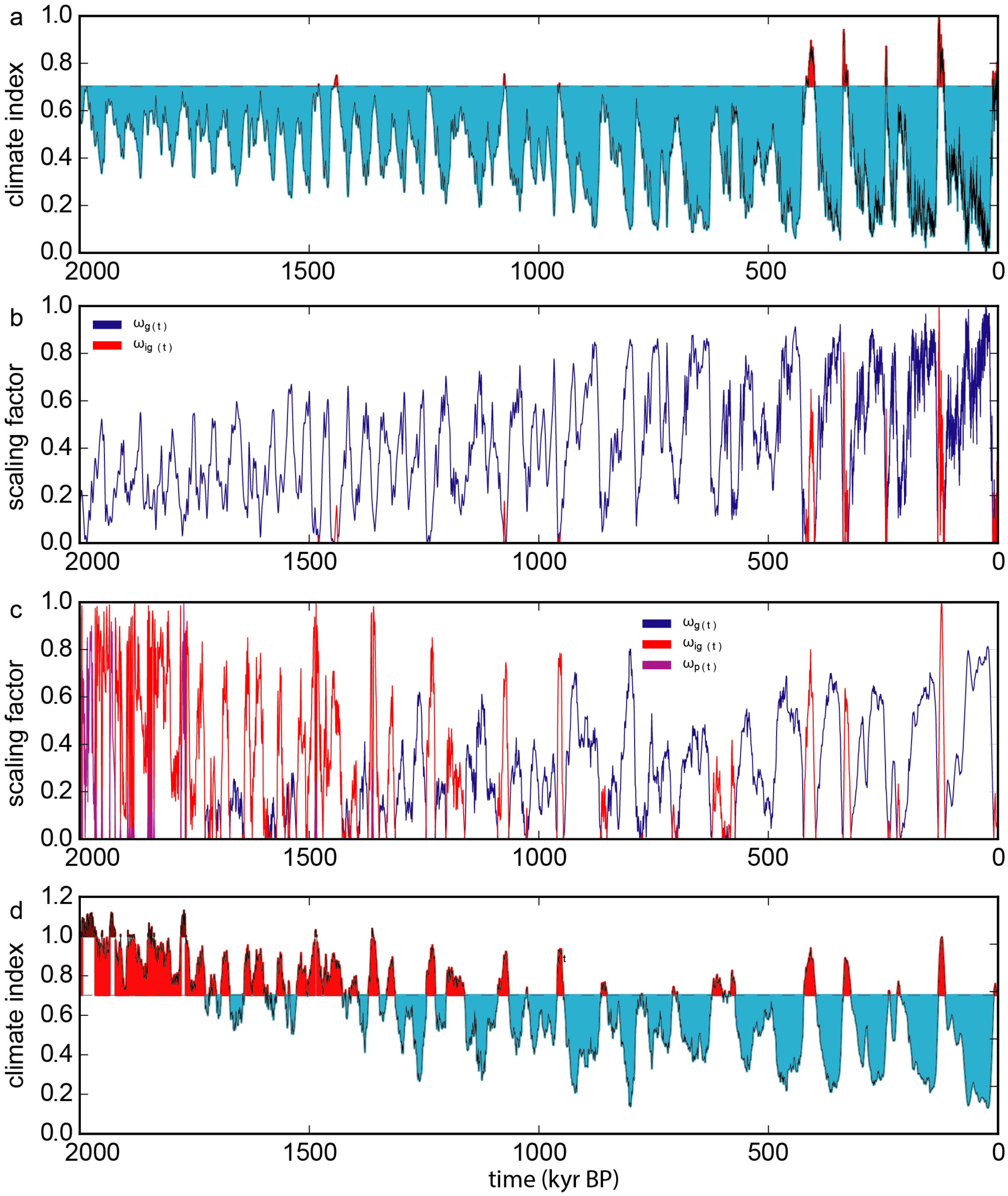

Figure 2. climate index derived from Dome C deuterium record a) and corresponding scaling factors ω_X in b). Times colder than present are shaded in cyan and times warmer than present in red. d) same as as a) but for climate index derived from the Snyder global surface temperature record and scaling factors ω_X in c) . Times warmer than the last Interglacial are shaded in dark red.

Otherwise the reader could wonder:

i) why is the value of the glacial index of ca. 0.7 during the Holocene? Does that mean you are taken a 30% of the LGM anomaly? (I guess not, and assume you put GI_pd to be approximately 0.7, so w_g in equation 3 goes to 0)

The climate index (called glacial index before, see ii)) is normalized with respect to the warmest climate period in the Dome C temperature record, therefore the LIG has index 1.0 in ensemble B1 and B2. The climate is linearly scaled between present day and LIG if the climate index surpasses the mean Holocene climate index which is ca. 0.7 (see inclusion starting on p6, l20).

P6, l20-

The respective values of the climate indices are CI_{lgm} = 0.0, CI_{pd} = 0.7022776, CI_{lig} = 1.0, CI_{p} = 1.130952. The climate index is normalized with respect to the warmest climate period in the Dome C temperature record, therefore the LIG has index 1.0 in ensemble B1 and B2. The climate is linearly scaled between present day and LIG if the climate index surpasses the mean present day climate index CI_{pd}, and between LIG and the Pliocene if the index is larger than 1.0.

ii) Is "glacial index" the right term for a curve that goes to 0 during glacial times? Would not be more appropriate to define it as (1-GI) or simply call it "climateindex"?

We agree that the term "glacial index" could lead to the impression that the index solely scales the climate between full glacial and present day conditions. We replace "glacial index" with "climate index" throughout the manuscript.

ii) What happens, for example when GI = 1.2 ? Do you take a linear interpolation of the Pliocene and the Last Interglacial climate fields? (I would assume so, because in equations 1 to 5 there is not any explicit differentiation of the time period, just a dependence on the values of the index, GI). If yes, can you justify it or elaborate?

Correct. Since the warmest climate period in the Snyder surface temperature record is not the LIG we interpolate between the LIG (climate index 1.0) and the warmest period in the Snyder climate index (ca. 1.2). Here, the climate is linearly scaled between peak LIG conditions (climate index 1.0) and peak climate conditions in the Pliocene (climate index ca 1.2).
The justification for the manner we implement the climate index is, that we assume, that qualitatively the climate system fluctuated throughout the Quaternary between interglacials and glacials as it did during the last glacial interglacial cycle. We are aware that this is a simplification, but we only had access to paleoclimate time slices from the LIG, LGM and Pliocene.

Furthermore, equation 6 must contain a typo or be wrongly formulated. In its current form, the more you cool from PD the more you increase the precipitation. Is that correct?

Typos / erratum on the glacial index description:
P5, L21: "T_opd is the surface temperature" (should be T_s)
P5, L22: commas missing after "LIG" and "LGM". I advise you to rephrase the whole sentence. P5, last line of equation 5: It reads "0.0 pdfor GI".

Corrected.

2) Other specific/technical comments:

P5, L2 reads. “To adequately capture continental ice sheet dynamics on long timescales (i.e. millennia and more), in principle, a coupled modelling approach is required to resolve climate-ice sheet interactions.”
I think I understand what you mean here, but I also believe a sentence that says “To capture A, a given approach is required to resolve B” does not make sense.

Agreed and changed.

P5, l 19-20

To adequately capture continental ice-sheet dynamics on multi-millenial timescales, in principle, a coupled modelling approach which resolves climate-ice-sheet interactions is required.

P9. L24: “The two clusters in the upper panel of Figure 5 show a present day ice sheet configuration (B1-branch) and a strong interglacial configuration in which the WAIS is collapsed (B2-branch)”.
I think this sentence needs some rephrasing.
By “.. show a present day and a strong interglacial” do you mean that their mean state is similar to the ones expected during present day and strong interglacial respectively?

Agreed and changed.

P12, l3-5

The two clusters in the upper panel of Figure 6 show an ice-sheet configuration similar to present day (B1-branch) and a strong interglacial configuration (B2-branch) in which the West Antarctic Ice Sheet (WAIS) has collapsed.

P9. L24: "...resembling the waxing and waning of the marine West Antarctic Ice Sheet" "resembling" or "due to" the waxing and waning...?

Changed accordingly.

Figure 4: hard to see. Because not said in the caption, I assume black (or dark grey) thin lines in the top panel correspond to the individual realisations of the B1 ensemble. But, because the Pollard 2009, deBoer2014 and Tichgelaar 2018 curves are also plotted in dark grey or black they are really not distinguishable. Why not plotting the individual members in white or light grey as for the B2 ensemble?

Changed accordingly (now Figure 5).

At the end of section 3.1 you state: "[...] all simulations with the GHF field from Purucker (2013) exhibit a collapse of the WAIS in the LIG with a much smaller percentage for both Martos et al. (2017) and Shapiro and Ritzwoller (2004)."
Why is this? I see no obvious explanation since the West Antarctica GHF values from Purucker seem lower than those from Shapiro and way lower than Martos's.

Agreed. This sentence needs further explanation. However, we think that the role of geothermal heat flux on the stability of the WAIS and glaciers in general is a research field in its own right and therefore a detailed discussion would be beyond the scope of this manuscript. Regardless, we would like to keep the sentence in order to point out that GHF can have a decisive role for ice sheet systems which are close to a nonlinearity (i.e. MISI). We added a sentence, providing one potential reason for the outstanding effect of a very cold GHF underneath WAIS in Interglacials.

P16, l3-

All simulations with the GHF field from Purucker (2013) exhibit a collapse of the WAIS in the LIG with a much smaller percentage of simulations for both Martos et al. (2017) and Shapiro and Ritzwoller (2004). A thorough analysis of this result is beyond the scope of this manuscript, but it could be caused by larger glacial ice cover caused by the cold Purucker (2013) basal conditions. This would lead to an over-deepened bedrock and larger surface gradients along the coast at the onset of interglacials and therefore favourable conditions for the marine ice sheet instability.

Figure 6 left panel:

i) Are the LGM (and LIG) values given in red and blue the mean of the three particular cases shown in figure 5?
Please specify in the caption.
ii) If yes, why is the LGM mean (-6.12 m) considerably higher than the red (-9.65 m) and the blue (-10.17 m) means?
Is it because the red and blue values are simply the mean of the three particular cases and not the mean of the whole B1 and B2 ensembles?
If yes, what makes these 3 particular cases to show a bigger ice sheet than their respective sub-ensembles during the LGM?

If yes, can we learn something from this related to the conclusions of the paper?

Figure 6 middle and right panels:

- To what particular realisations of the ensemble do the 2D plots correspond to?
- Following the PD grounding line position line, I recognize indeed its current grounding line position but also its current ice front. Is that correct? Are you plotting both? If so, I would change the legend or specify it in the caption
- Why is the LGM grounding line of the Ronne ice shelf, Pine Island and west of the Antarctic Peninsula much more retreated than the one suggested by Bentley?
- Is this a result of the particular model realization or proper to the ensemble?

Figure 6:
It would be nice to have a third 2D panel showing the simulated PD ice sheet for the same parameters as for the LIG and LGM plots.

Caption of figure 6 reads: "Middel and left panel illustrate simulated ice sheet configurations for the LGM and Last Interglacial".

Figure 6 is now split into two figures (Figure 7 on p14) and Figure 8 on p15. We hope Figure 8 provides an improved visual aid to the spread of the ensemble, with the main 8 ensemble members highlighted in colour for both B1 and B2.

We also add a discussion of the LGM grounding line position and the spread of results regarding LGM sea level contribution in Figure 8 (formerly Figure 6).

P14, l29 -

In general, the glacial extent of the AIS matches reconstructed LGM grounding margins rather well, with the notable exception of the Ammundsen and Bellinghausen sea sectors. In this region, the ocean forcing seems to be too warm to allow for an advance of the ice margin to the continental shelf edge in the model. LGM ice growth in the whole ensemble is strongly dependent on the SIA enhancement factor sia_e, with values larger than 1.5 leading to a underestimation of ice thickness, albeit not necessarily ice extent. In the Ross Sea, ice thickness calving exerts a strong influence on grounding line advance. A calving thickness of 75 generally leads to a good representation of LGM ice margin reconstructions (Bentley et al., 2014), while simulations with a thickness limit of 150m underestimate Ross Sea LGM grounding line advance. Furthermore, the parameterisation of ice shelf calving can play a preeminent role in interglacials, which underlines the dire need for a physical rather than heuristic representation of calving in ice-sheet models.

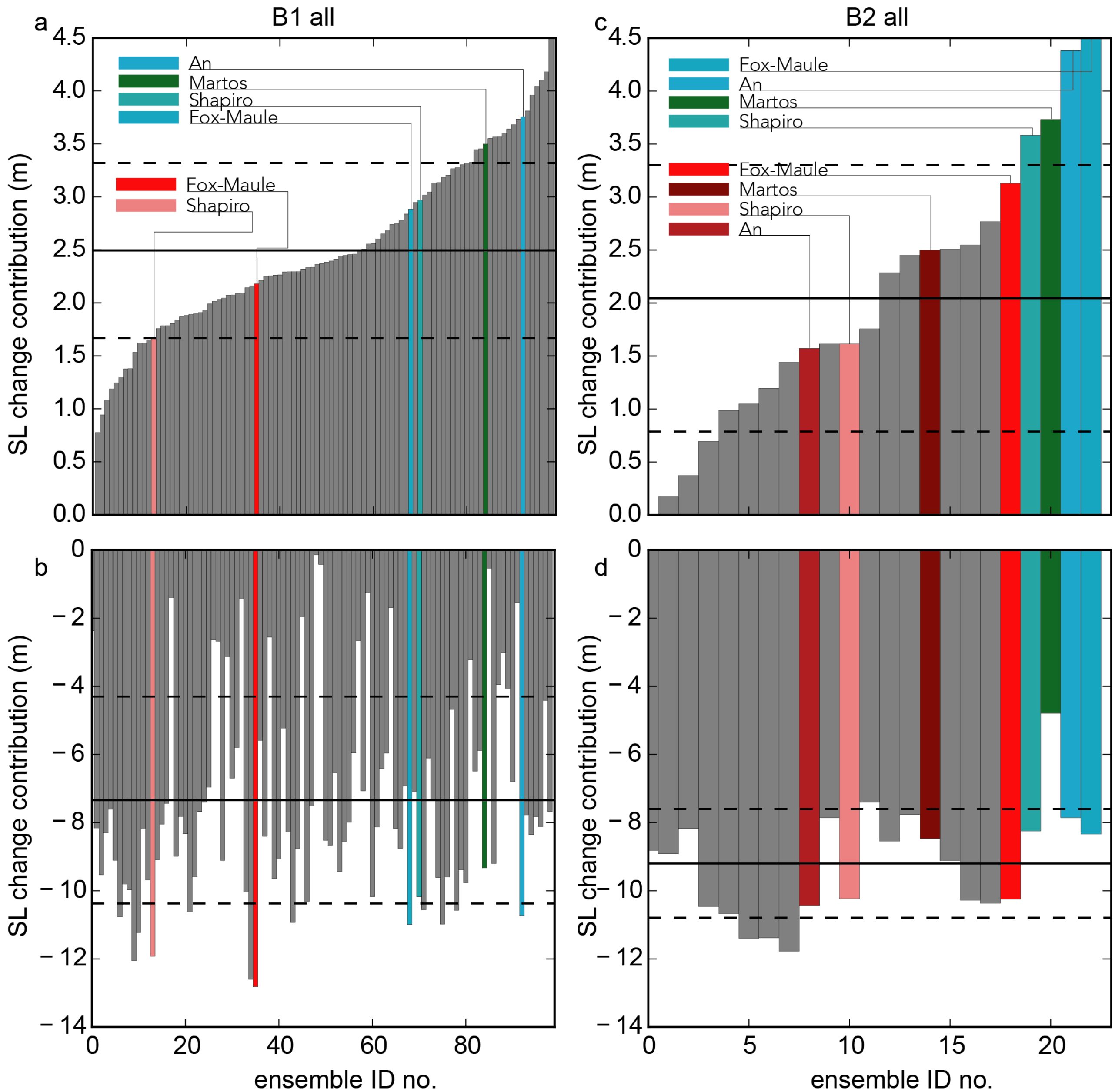

Figure 8. Sea level contribution in the LIG (a/b) and LGM (c/d) for the full ensemble forced with climate index B1 and climate index B2. The ensemble members focused on in this paper are highlighted with colours (P1 red colors, P2 blue/green colors). Horizontal black line depicts the full ensemble mean and the dashed line the standard deviation.

We expanded Figure 7, to illustrate the effects of the different forcing approaches on the spatial configuration of the AIS during the LIG, LGM and PD.

“Middel” is wronlgy spelled.
“Left” should be right. And invert the order of LGM and Last Interglacial.

Addressed (see new Figure 7).

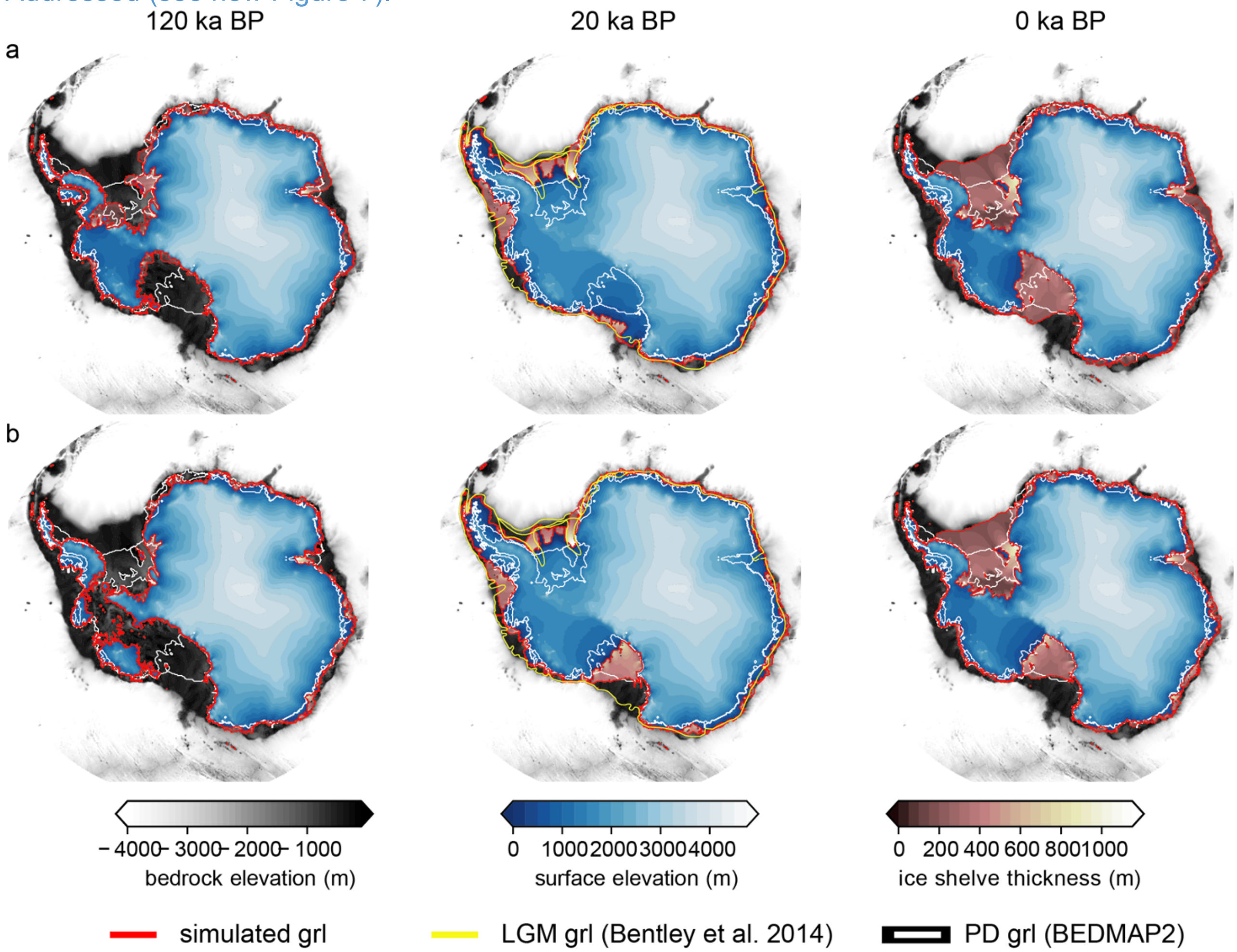

Figure 7. Panel a/b illustrate simulated ice-sheet configurations for the LIG, LGM and PD, respectively. Both simulations are carried out with forcing B1, using a different ice thickness calving limit (a: cH=75 m, b: cH=150 m). Reconstructed grounding line positions for the LGM (Bentley et al., 2014) are depicted in yellow, and both grounding line and ice shelf front from BEDMAP2 (Fretwell et al., 2013) are depicted in white.

P15, L6 reads: “Mean ice thickness variability for Dome Fuji and Dome C during the late Quaternary is 165 and 195 m, respectively (105 and 140 during pre-MPT).”
How is this “mean ice thickness variability” calculated? Is it the temporal standard deviation of the mean evolution of the ensemble, or has it something to do with the standard deviation of the ensemble itself?

Corrected. The increase is in mean ice thickness, not in variability. We added a sentence, pointing out the change in ice thickness variability (std) before and after the MPT.

Changes in the EAIS manifest in an increase in ice thickness by ca. 30% (mean ice thickness at ice core locations calculated for pre-MPT (1.8-1.2 Myr BP) and Quaternary (0.8-0.0 Myr BP)) time intervals alongside an increase in variability (standard deviation of ice thickness of individual ensemble members) of ca 50%.

P15, L7 reads: “Overall, the simulated present day ice cover after 2 million years at the highlighted ice core locations is in good agreement (within ≈ 5%) with the BEDMAP2 (Fretwell et al., 2013) data set.”

This sentence does not seem precise enough. Does “present day ice cover” mean ice thickness? Here it is important to be precise, because it is not the same saying that the the error of the simulated region of Antarctica covered by ice falls within 5% with respect to BEDMAP2 than saying that the simulated thicknesses are within 5%. If the latter is what you meant, how did you calculate it?

We agree that this sentences requires clarification. We now specifically address the discrepancy of ice thickness at selected East Antarctic ice core locations (see below)

Somehow related: Why is Talos systematically too thin?

We agree that the TALDICE region ice thickness is not as well captured as for the other deep ice cores discussed here. We now address this discrepancy explicitly in the manuscript.

P19, l7-16

Overall, the simulated present day ice thickness after 2 million years at the highlighted ice core locations in East Antarctica is in good agreement with the ice thickness derived from the BEDMAP2 (Fretwell et al., 2013) data set (within ≈ 5% discrepancy in ice thickness for the selected ensemble members in B1 and B2). A notable exception is the Talos Dome ice thickness, which is too thin in all discussed B1 and B2 ensemble members except for the runs simulated with the relatively cold (Purucker 2013) and intermediate (Shapiro and Ritzwoller, 2004) GHF data set. The applied model resolution of 16km is generally to coarse to to accurately reconstruct smaller outlet glaciers and therefore might overestimate advection away from coastal ice domes.

P15, L11 reads: “We apply the conditions for the existence of 1.5 Myr old ice derived in Fischer et al. (2013) to our simulations …”.
Please specify the conditions you refer to here. This would allow the reader to have an idea of your “oldest ice” results without having to look for such conditions in Fischer et al. 2013.

Agreed, we added the conditions for Oldest Ice from Fischer et al. 2013

P19, l6-9

We apply the conditions for the existence of 1.5 Myr old ice derived in Fischer et al. (2013) (ice thickness larger than 2000 m, basal melting zero and surface ice velocity slower than 1 m/a) […]

As a conclusion, I think this is a nice paper and that these comments represent only a minor revision, but I also fear that, in its current form, the reader would not appreciate the manuscript as much as they should. Therefore, I recommend the authors to correct all these minor issues and carefully go through the new manuscript in order to maximize reproducibility and a nice “flow” when reading it.

We thank Jorge Alvarez-Solas for his positive review of our manuscript and hope that the revised manuscript satisfactorily addresses all critical points.