Blue color: reviewer comment

Dark color: Author response

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Summary

The manuscript presents a study into 'tipping points' in the Antarctic ice-sheet system: climatic thresholds that, if crossed even briefly, lead to irreversible, large-scale retreat. Determining the existence and quantifying the thresholds of these tipping points can help understand the future evolution of the ice sheet. The authors study these tipping points from a palaeoclimate perspective. They have set up an ensemble of different realisations of the ice-sheet model Yelmo, and forced those realisations with output from a number of different climate models that have simulated the climate of the mid-Pliocene Warm Period. As that period represents the most recent time in Earth history that any significant retreat of the East Antarctic ice sheet might have occurred, it could help provide insight in the mechanics of these tipping points. The authors show that, depending on the amount of oceanic warming and the changes in precipitation predicted by the climate models, different sectors of the Antarctic ice sheet could have collapsed during this period. From this, they estimate the warming thresholds that represent the tipping points.

We thank the reviewer for the thoughtful comments and valuable suggestions, which will contribute to improving the quality of our manuscript. Below you can find our response to each comment.

General comments

In general, I think the manuscript does a good job of presenting the work. The introduction provides a good context, the methodology generally gives a good picture of exactly how the work was executed, and the results are presented clearly.

However, aside from a number of small, technical comments (outlined below), I have a few more significant concerns with the adopted methodology.

1) Initialisation. The ice-sheet model in this study is initialised by a steady-state spin-up, with no inversion procedure for basal friction, viscosity, or basal melt. The consensus that has emerged in the last few years is that the biases this introduces into the dynamic response of the model to a climate forcing, are no longer acceptable. The (currently stated) aim of this study is to quantify warming threshold beyond which certain sectors of the ice sheet will collapse. I believe the current initialization strategy introduces major errors in these numbers. Either the initialization strategy needs to be revised (which I think should become standard practice for all palaeoglaciological applications, just as it already is for future projections), or the aims of the manuscript need to be changed.

We understand the reviewer's concerns about our initialisation procedure.The reason we have chosen not to use this method here is that friction coefficients from past periods may not be the same as for the present-day (PD). Since the Antarctic Ice Sheet (AIS) has undergone several periods of advance and retreat since the mid-Piacenzian warm period (mPWP) to PD, we could expect that due to erosion, friction coefficients are different.

The friction law used in this study follows

$$\boldsymbol{\tau}_\mathrm{b} = c_\mathrm{b} \left(\frac{|\mathbf{u}_\mathrm{b}|}{|\mathbf{u}_\mathrm{b}| + u_0} \right)^q \frac{\mathbf{u}_\mathrm{b}}{|\mathbf{u}_\mathrm{b}|}$$

$$c_\mathrm{b} = c_\mathrm{f} \lambda N$$

$$\lambda = \begin{cases} 1 & \text{if } z_b \geq 0 \\ \max\left[\exp\left(-\frac{|z_b|}{400}\right), 10^{-4}\right] & \text{if } z_b < 0 \end{cases}$$

where c_f is a unitless coefficient (which in our study ranges from 0.1-1.0). Our simulations produce good results in terms of RMSE of ice thickness and surface velocities comparable to those of other groups in the context of ISMIP6 (Figure 1). Furthermore, there is no a priori reason to believe that optimized friction coefficients for PD would have been the same for the mPWP. Our approach has the benefit that basal friction adapts to changes in ice thickness and effective pressure as a result of the changes of the boundary condition of the mPWP with respect to the present day. Therefore, we believe that for the sake of our study, it is more beneficial to use a simple parameterization as in other paleo-studies (Quiquet et al., 2021), rather than optimized friction coefficients. Another future idea could be to include an active sediment mask to account for changes in erosion.

We will make it clearer in the revised manuscript.

2) Climate forcing. The method presented here, where the ice sheet is directly subjected to unchanging output from a GCM, is outdated. Even if setting up a coupled GCM-ISM is judged to be too expensive (which is probably the case for the multimillennial simulations presented here), there have been several papers in the last few years that describe more elaborate ways of forcing an ice-sheet model with pre-calculated GCM output. This way, the effects of the changing ice-sheet geometry on the climate, and the way these in turn feed back on the ice geometry, can be captured more accurately. Since the study involves major change in ice geometry (including the complete collapse of the West Antarctic ice sheet), these f+eedbacks should not be ignored.

Our lapse-rate factor includes changes in geometry. On one hand, PD climatologies are scaled with the surface elevation from RACMO2.3. On the other hand, we also take into account the surface elevation from the mPWP and pre-Industrial period provided by the PlioMIP2 protocol. This point will be made clearer in the next submission since it has raised questions about the methodology and the influence of topographic changes.

The other reviewer also had concerns about potential feedback mechanisms which may be neglected due to not coupling our ice-sheet model with an AOGCM. We have included the following discussion paragraph:

“Our forcing strategy based on an anomaly-snapshot method (i.e. one constant climatic snapshot from each AOGCM) ignores certain climate-interactions that could be relevant to the system. We take into account the surface melt-elevation feedback, by employing an atmospheric lapse-rate factor, and the albedo-melt feedback, which is considered within our ITM parameterisation. Nonetheless, probably one of the most important feedbacks not considered here is the effect of freshwater flux release from the AIS into the Southern Ocean. Results from Sadai et al. (2020) show that accounting for Antarctic ice discharges increases Southern Ocean temperatures, whereas in Bintanja et al. (2015) ice-shelf melt leads to a cooling of the Southern Ocean and an expansion of sea ice area. This points to the need for a more profound understanding of ice-ocean related processes within models.

A more sophisticated approach would include direct coupling between an AOGCM and our ice-sheet model. However, besides more computational resources, this would require constraints not only on our ice-sheet model parameters, but also on those of the AOGCM. The work of Berends et al. (2019) is a good example of an intermediate strategy, based on a matrix method. However, for this it is necessary to account for an AOGCM to produce several snapshots with varying CO_2 concentrations and ice-sheet coverage. In addition, running long transient experiments with this method still needs to be traded off with a lower ice-sheet resolution. In Berends et al., (2019), the AIS is simulated at a 40 km resolution. This is a potential explanation why they do not simulate a retreat in the East Antarctic region. Here we aim to obtain a more profound understanding of processes related to ice dynamics in part through a higher spatial resolution (16 km).”

3) Tipping points in dynamical systems. There have been a number of studies into tipping points from a dynamical systems point of view. These show, for example, that it matters for how long a threshold is crossed, or by how much, before a system tips. The steady-state approach adopted here excludes these temporal effects. For example, recently Stap et al. (2022, The Cryosphere) demonstrated that there are significant differences between the equilibrium response of the ice sheet (which is what is studied here), and the transient response (which is what occurs in reality). While they studied this in the even warmer Miocene, the conclusions apply to this study as well. The authors already mention that, in several of their simulations, the collapse of an ice-sheet sector can occur several thousand years after the onset of the climate forcing. Given the significant climate variability during the mid-Pliocene Warm Period, it is possible that this study significantly overestimates the ice-sheet retreat and the sea-level high stands during this period.

Indeed, we are applying a constant temperature for a long time period which may not mimic reality. Furthermore, we assume a constant steady-state whereas the transient behavior of the AIS may not respond equally. Thus, probably we could be overestimating the ice-sheet retreat and sea-level contribution since we forced with a warm period for a long model-time. Nonetheless, we applied a similar methodology as other ice-sheet modeling studies for the mPWP (deConto et al., 2021; Dolan et al., 2018; deConto and Pollard, 2017; Golledge et al., 2017; Yan et al., 2016; deBoer et al., 2015). We extended the discussion section as follows:

“In our study, the transient character of the climate system was neglected for the sake of simplicity and the poor knowledge on the transient forcing as well. Instead, we decided to force towards a steady mPWP state for an ensemble large enough to be statistically significant (more than 30 simulations) for 12 different mPWP conditions. This approach permits us to assess Antarctic tipping points starting from PD conditions as well as the impact of the uncertainty associated with state-of-the-artc equilibrium mPWMP climatic conditions. This experimental setup goes in line with other studies, allowing for a similar comparison (Yan et al., 2016; DeConto and Pollard, 2016; DeConto et al., 2021). However, assuming a constant warming may lead to overestimation of sea-level contributions. As shown in Stap et al. (2022), the simulated Antarctic ice-volume evolution for the Miocene period reduces when the forcing is transient, rather than static. To our knowledge, only one study has simulated the transient evolution of the AIS under the Pliocene. The climate forcing in the transient evolution of Berends et al. (2019) did not reach the necessary conditions to lead to a retreat in the Wilkes basin, and thus simulated a lower sea-level contribution (Fig. S4c).

It is important to mention that exceeding a tipping point does not mean that the ice sheet will collapse immediately, but rather that it has reached the threshold temperature by which a retreat will be induced and further amplified by MISI. By plotting the one dimensional evolution of the WAIS (Fig. S2), we observe that the WAIS collapse usually occurs with a lag of 1000-5000 years from the application of the forcing. In some cases it can reach up to 25000 years. MISI is not only a matter of the oceanic temperature threshold, but also depends on the grounding-line position and the thermal forcing at this location, as well as precipitation. Thus, a transient character in the forcing could avoid certain ice collapses if the warming is not sufficiently long. Other factors, such as ice dynamics, could also delay (or accelerate) the grounding-line position reaching a pronounced retrograde bedrock that leads to a full collapse of the WAIS or other marine basins."

Specific comments

L12:
"…a higher-order ice-sheet model." DIVA is not a higher-order flow model. As Goldberg (2011) states, it is "a depth-integrated approximation to a higher-order flow model". Please change this here and throughout the manuscript.
L106:
"…similar to other 3D higher-order models." Same as before.

This raises an interesting question about model hierarchy. We followed the description of Lipscomb et al., (2019) which distinguishes between: (a) SIA and SSA approximation, (b) DIVA: depth-integrated higher-order approximation, (c) 3-D higher-order approximation based on Blatter-Pattyn (BP). As Lipscomb et al., (2019) we believe that DIVA constitutes a major improvement compared to hybrid-models and this should be reflected in the manuscript.

Schoof and Hindmarsh (2010) developed a depth-integrated model (see also "L1L2" model in Hindmarsh, 1993) highlighting the peculiarity that velocities can be computed to order $O(\varepsilon^2)$ with a normal stress that is truncated at order $O(\varepsilon)$ (see Eq. 3.32 in Schoof and Hindmarsh, 2010). Therefore, a second-order accurate velocity solution can be obtained only by keeping first-order terms in the normal stress. In our manuscript, we have changed the text from "higher-order model" to "depth-integrated higher-order model" following Lipscomb et al. (2019), but we believe it is accurate to explicitly state that the DIVA solver is second order accurate in the velocity solution.

L15: "…related to initial topography…" Do you mean initial ice thickness, or bed topography?

Ice thickness. We changed it in the manuscript.

L20: "…the likelihood of crossing them under future emission scenarios". You have not done any simulations involving future scenarios though, so I think this is phrased too strongly here.

We removed the last sentence to avoid misinterpretation.

L22: "ocean expansion" change to "thermal expansion".

Done.

L30-31: what is/are the source(s) for the different temperature thresholds you state here?

We included the references.

L39: "The thinning of ice shelves…" you mention hydrofracturing as a process causing this, but hydrofracturing causes shelves to disintegrate, not to thin. Choose a more appropriate phrasing.

We changed it to "The stability of ice shelves depends on several processes, such as…"

L48: Is there no more recent data on Pliocene CO2 concentrations than Haywood 2016?

We added two new references.

L113: This needs some more explanation. Provide the expressions for lambda and N, and explain what you mean when you say that "we will use it [c_f] for calibration of the model".

In the next submission we will add details on λ, N_{eff} and c_f.

L120-121: This is maybe a bit too cautious. Earlier work on sub-grid scaling of friction (Feldmann et al., 2014; Leguy et al., 2021; Berends et al., 2022), has quite clearly shown that you can get good results at much coarser resolutions than the <100 m numbers mentioned in the MISMIP papers.

We included those references in that section to point out that sub-grid scaling of friction leads to good results at coarser resolution.

L123-125: The last word has not yet been said on the problem of sub-shelf melt at the grounding line. It is not automatically so that the NMP scheme is the best choice at coarse resolutions (see e.g. Berends et al., 2023, Journal of Glaciology), so I'd like to see a bit more discussion on how this affects your results.

Indeed, applying melting at the grounding-line would very likely have a big impact on the results. We have extended the discussion as follows:

"Another source of uncertainty is the melting at the grounding line. Observations have established that the ocean-induced basal melting is highest close to the grounding line and decreases towards the ice-shelf front (Adusumilli et al., 2020). Ice-sheet models use different approaches which are typically no ocean-induced melting or partially ocean-induced melting (Seroussi and Morlighem, 2018; Leguy et al., 2021). In many coarse resolution ice-sheet models (more than 2 km resolution at the grounding line), no melting is applied directly at the grounding line since it can lead to overestimation of sub-shelf melting (Seroussi and Morlighem, 2018). Nonetheless, the work of Berends et al. (2023) demonstrated that applying melting at the grounding line (via a flotation criterion) in their model resulted in more appropriate for some settings. This could suggest that our results correspond to a lower limit since no melting is applied at the grounding line in our experiments. We expect that by adding melting at the grounding line, the collapse of the Wilkes basin would have been more likely for those AOGCM climates with higher oceanic thermal forcing. Basal melting parameterisations remain a source of uncertainty which need further investigation."

L133: "lambda_srf and c are parameters used to calibrate the AIS" What do you mean by this? Do you calibrate to achieve an SMB similar to observations/regional climate models when forced with reanalysis climate? Or do you calibrate to produce a steady-state ice sheet similar to observed? Both could be seen as appropriate for this work, but you should explain what you do and why.

We calibrated to PD ice thickness observations assumed at steady state. This point will be made clearer in the next submission.

L140:
More often, people adjust precipitation by ratios rather than differences (e.g. P = P_pd * (P_mod_Plio / P_mod_PD). Explain why you deviate from this practice.
L237-243:
This section is unclear. What do you mean when you say that the "precipitation anomaly lies around 85% of PD precipitation"? Do you mean that (P_mod_Plio - P_mod_PD) / P_PD = 0.85? Or that P_mod_Plio / P_mod_PD = 0.85? Or 1.85? The phrase "Thus, we see that a thermal forcing below 0.5 K can lead to a collapse of the WAIS if precipitation stays below 80% of PD" seems to suggest that the WAIS will collapse if the ocean is warmer than present, AND the precipitation is lower than present. Please rewrite this in a more understandable manner.
L248-250:
Again, your treatment of precipitation anomalies is very confusing. You state that a precipitation "three times more than PD rates" is similar to an anomaly of "around 130% of PD rates". I see no way for those numbers to be similar.

We do employ precipitation ratio anomalies, not differences. This was a typo we did not see and we thank the reviewer for pointing it out. We believe that this now clarifies all the other points related to the treatment of precipitation. We will correct this typo in the next submission and it will be made clearer.

L142: There are more recent RACMO simulations of Antarctica than this.

Indeed, we could update our RACMO fields. Since we do not expect big changes with the latest RACMO version we went on with the previous dataset. Nonetheless, we will update to the latest RACMO version for future work.

L146-147: "…a lapse rate correction factor is applied…" What do you use as the reference surface elevation? RACMO uses a different elevation than the GCMs you used, how do you account for this difference?

To scale the PD climatologies we use the RACMO dataset (i.e., the surface elevation from RACMO2.3). Climatologies from the GCMs are scaled with the surface elevation provided by the PlioMIP2 protocol for the pre-industrial and the mPWP period. This will be made clearer in the manuscript.

L162: "PD fields are obtained from the ISMIP6 protocol" Technically, this protocol only dictates how to extrapolate any ocean temperature dataset into the sub-shelf cavities, and into the space currently occupied by ice or bedrock. I assume you mean you applied this protocol to the World Ocean Atlas dataset? If so, mention this.

Yes, we referred to the World Ocean Atlas dataset with sub-shelf temperatures extrapolated by the ISMIP6 protocol. We changed it in the manuscript.

L162-163:
"For computing the basal-melting rates at the mPWP, the Tf and So fields are changed with an anomaly method analogous to equation 4". The GCMs only provide temperatures for the open ocean. How do you compute the anomalies in the sub-shelf cavity? Do you apply the same ISMIP6 extrapolation protocol to the GCM data?
L393-398:
Ah, here is the detail I was missing earlier. This should already be explained in your methodology section.

We moved the paragraph to the methodology section.

L165: "For those cases, a spatially homogeneous temperature anomaly field of one fourth of the atmospheric anomaly was applied, following work by Golledge et al. (2015) and Taylor et al. (2012)." This seems rather ad-hoc. If you were to apply this method to the GCMs where you do have ocean data, how would the result compare to the actual GCM ocean temperatures?

Indeed, comparing with data from the other AOGCMs may provide more information here. The mean ratio between surface temperature and oceanic forcing for the AOGCMs here is 0.43, but it spans from 0.04 (MIROC4m) to 1.5 (NorESM1-F). Of course, depending on if the rate is higher than 0.25, then it would be likely to have more retreated marine basins, whereas if we took a smaller value we would expect a smaller sea-level contribution. We will maintain 0.25 (Golledge et al., 2015, Bulthuis et al., 2019) since it is based on other values from literature, and we would expect this uncertainty to lie within the inter-model uncertainty from the climatic forcing.

L169-170: "First we perform an ensemble of 180 ice-sheet simulations for the AIS with different dynamic configurations under steady PD climatic conditions using the ." Using the what? Oh, the suspense!

Fixed.

L177-180: How do your initialized geometries compare to the present-day in terms of root-mean-square errors in thickness and velocity? Having an approximately correct volume is a nice start, but in theory this could also be achieved by collapsing the Wilkes basin but grounding the Ross.

Indeed, we added to the SM the following plots of the simulated RMSE of ice thickness and velocity together with a spatial map of the mean. The 2D ice thickness and velocities both show reasonable distributions.

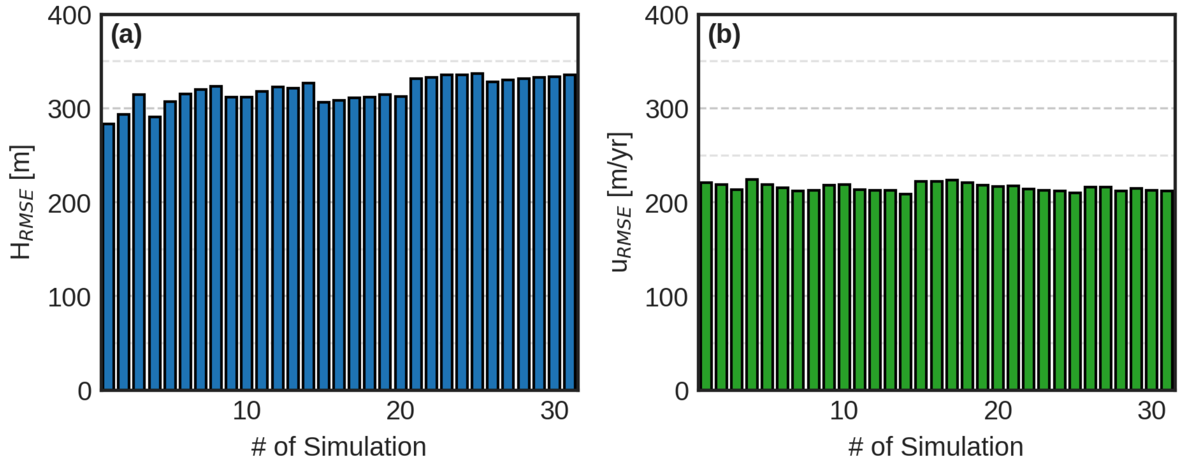

Figure 1: Bar chart of the simulated PD RMSE in (a) ice thickness and (b) surface velocity for every simulation.

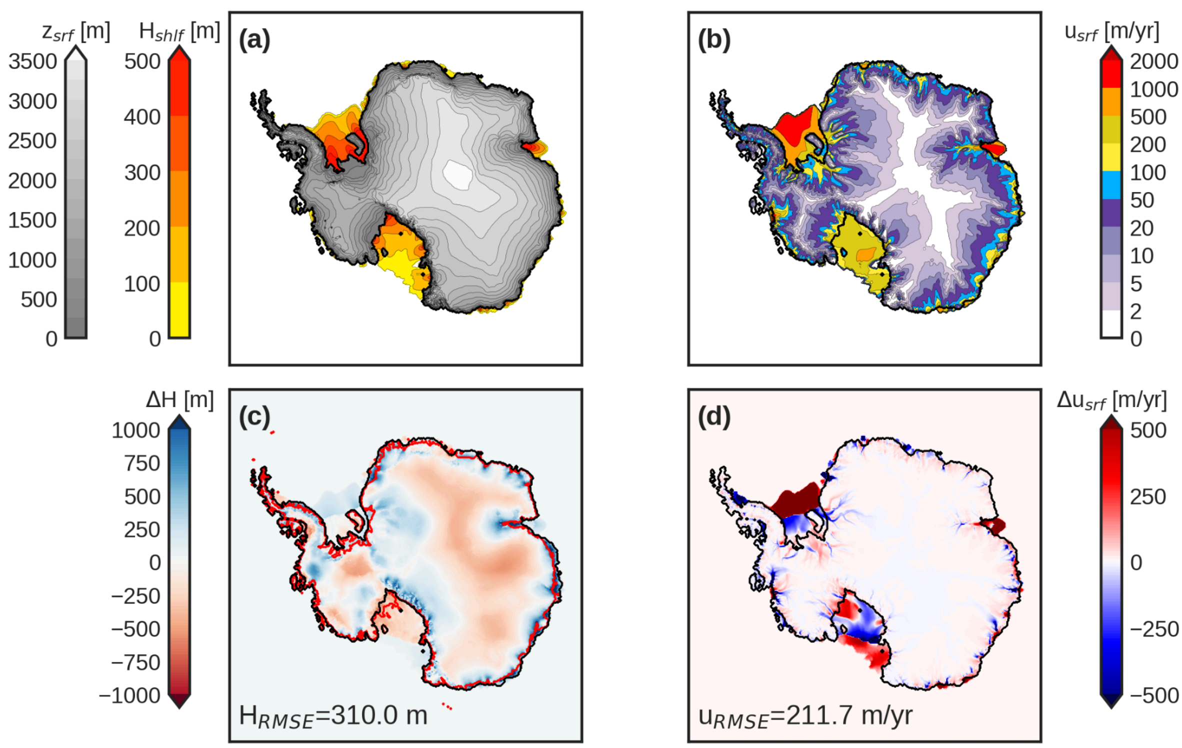

Figure 2: Mean PD state of all the PD simulations. (a) surface elevation (grey colors) and ice shelf thickness (orange); (b) surface velocity; (c) ice thickness (d) surface velocity anomalies with PD observations and its respective RMSE.

L197: “Ice extension ranges from 9.2 times 10^6 km2” Why spell out the word “times”? I’ve not seen this notation anywhere else.

We changed it to “x”

L215: “The ice thickness is practically always negative” I assume you mean the thickness anomaly.

Yes, fixed. Thank you!

L222-224: “This spread in the EAIS and more specifically in the Wilkes basin points to an important role of the applied boundary conditions in the model response.” What boundary conditions do you mean?

We changed it to

“This spread in the EAIS and more specifically in the Wilkes basin points to an important role of the ice dynamics.”

L228: “Since an increase in oceanic forcing is thought to be the main driver of MISI…” Thought by whom? Also, consider a different phrasing than “driver” – by definition, an instability will result in a retreat that continues even in the absence of continued forcing, which only needs to act as a trigger.

Indeed, trigger is more appropriate in this context. We use oceanic forcing as a metric since it has a strong effect on the stability of ice shelves, which in turn reduces buttressing effect (Fürst et al., 2016). Now it reads:

“We find that oceanic forcing is the main forcing which defines the ice extent of marine basins (Figure 8).”

L229: “the ice extension” I think you mean ice extent, here and in other places throughout the manuscript.

Corrected.

L231: “this model result does not show realism in terms of sea-level equivalent” What do you mean by this?

We mean that this is the only AOGCM that simulates a negative sea-level contribution, which strongly disagrees with sea-level reconstructions and other proxy data. This will be made clearer in the next submission.

L235-236: “we focus on the four models that do not exceed 1 degree of oceanic anomaly” How do you define this anomaly? Global, Southern Ocean, Amundsen Sea, sub-shelf cavity? Sea surface or vertical average?

It is defined by basins (Figure S3) at grounding-line depth. This will be made clearer in the next submission.

L245: “we assume that thermal forcing is the main trigger” Do you mean oceanic or atmospheric thermal forcing?

We mean oceanic thermal forcing, this was made clearer in the manuscript.

L270-279: This section is unclear. Did you use a different initial ice thickness, or (also) a different bed topography? Did you use that as the initial state for your spin-up simulations, or only for your GCM-forced simulations? What do you mean by “the parameters from the ensemble that produced results closest to the mean value for every AOGCM forcing”?

We used the PD bedrock topography with the PRISM4 ice thickness. Instead of running the whole ensemble, we took for every AOGCM the dynamics parameters (c_f, E_f and q) which simulated the closest value to the mean.

L305-307: This work by Richards et al. (2022) really should already be mentioned in the introduction.

Indeed, and we are grateful for this suggestion. We included a new paragraph in the introduction which reads as follows:

“Another approach to infer sea-level estimates from a modeling perspective is through Geodynamic Models. These models use glacial isostatic adjustment and mantle dynamic topography to compute and correct for sea-level records. An advantage is that they account for potential rebound effects which are difficult to assess on in-situ sea-level records. In the work from Hollyday et al. (2023) they used such a model to simulate the mantle flow from the Patagonian region. This resulted in lower mPWP sea-level estimates of 17.5±6.4 msle, and specifically an AIS contribution of 9.5±6.9 mSLE. Similar results are obtained in the work by Richards et al. (2022) where they simulate the Australian mantle deformation and compare it with proxy data from that region. They obtain a mPWP sea-level stand from 10.4-21.5 mSLE. Moucha and Ruetenik (2017) simulate a global sea-level contribution of 15 mSLE based on the US Atlantic shoreline. These studies reflect an overestimation in sea-level rise of in-situ records since lithospheric rebound is poorly considered.”

L315: “this threshold is highly sensitive to structural dependence” What do you mean by “structural dependence”?

What we mean with structural dependence are model parameters or choices of parameterisations (like grounding line treatment) which have a great impact on the outcome and its application remains a matter of debate. We switched the term to “structural (i.e. related to the parameterisation choice) and parametric model dependence”.

L304-316: This section contains a number of spelling/grammar errors.

This section will be corrected in the next submission.

L318: “This might seem counterintuitive…” It does. Your suggested explanation that “ice does not flow sufficiently fast to readvance again” contradicts the findings of the MISMIP experiments, which are founded on basic ice dynamics.

Indeed, it contradicts the findings. We believe it is related to the initialized PD state. We rephrased our text as follows:

“In our study we do not find a clear relation between ice dynamics and the simulated ice extent. Simulations forced with CESM1.0.5, simulate a slightly more retreated Totten basin for low enhancement factors (Fig. S5). We believe this is rather a consequence of the simulated PD state rather than ice dynamics, since we did not use any ice extent metric in the EAIS. In the MIROC4m model we find that a WAIS collapse is more likely to occur for high enhancement factors and low friction exponents, which promotes faster ice flow. In summary, although we observe some trends associated with the dynamic configuration for CESM1.0.5 and MIROC4m, no clear relationship can be found.”

L323-324: “Such an analysis of structural dependence allows us to assess the sea-level uncertainties that arise from dynamical configuration and climatologies.” Unclear what you mean by this.

We switched it to “Our analysis allows us to assess the sea-level uncertainties that arise from structural (model-related) uncertainties within one ice sheet model and climate-forcings from different AOGCMs ”

L328-329: “Thus, a large ensemble parameter constraint like in our study, helps considerably to reduce uncertainty from ice-sheet models.” No, you only used one ice-sheet model. Dolan et al. (2018) used three completely different models (ANICE, Sicopolis, and BASISM). It’s not at all surprising that they find a larger spread.

Yes, our results refer to ice dynamics within one ice sheet model. We rephrase the paragraph to

“We find that the climatologies yield a larger uncertainty (~7 msle) than that resulting from the dynamic configuration if parameters are constrained with PD observations. Dolan et al. (2018) obtain more than 10 msle between different ice-sheet models, whereas we obtain less than 2 msle differences for simulations which are not close to tipping, and up to 5 msle differences for CESM1.0.5 due to the proximity of Wilkes basin to tipping or not(Error bars Fig. 5).”

L331-333: “Consistent with our results, ISMIP6 simulations forced with these and other climate models predict that Antarctic tipping points could be reached within this century” Your results do not support this at all. You have performed no future simulations.

The other reviewer also had concerns regarding this point. To avoid confusion we removed the paragraph since it is not clear how our results affect future projections.

L335-336: “Two of the models employed here (EC-Earth3.3, HadGEM3, see Table S1) belong to CMIP6 whereas the rest belong to CMIP5” What does that imply for your results?

This paragraph was removed to avoid confusion.

L339-245: This section is vague, but I think it tries summarise one of my major concerns mentioned before: that there is a difference between equilibrium response and transient response, and that tipping points concern the latter rather than the former.

Yes, we will improve this section by adding a more detailed discussion of the differences that may arise between a transient forcing and a steady-state forcing, as discussed above.

L348: “an ensemble large enough to be statistically significant (more than 30 simulations)” How many simulations would you define as “statistically significant”? I find it hard to believe that you can put a number on this.

Statistically speaking, the more data you have, the more robust the conclusion is. In probability theory the distribution of a random sample converges to a normal distribution if the sample is large enough (the central limit theorem). Usually the number 30 is taken as a “thumbnail rule” to consider that the central limit theorem is applicable.

L352: “the transient evolution of Berends et al. (2019) allowed only for a WAIS collapse, avoiding other tipping points” Untrue. This model set-up “allowed” for ice-sheet collapse anywhere. That the climate forcing applied there did not result in a collapse is another matter.

Yes, the reviewer is correct. We rephrased it to

“The climate forcing applied in the transient runs of Berends et al., (2019) did not lead to a retreat in the Wilkes basin, and thus simulated a lower sea-level contribution (Fig. S4c).”

L357-358: “their results also show that starting from PRISM4 conditions leads to higher sea-level contributions and a less extended AIS during the mPWP. This result is expected” Those results were obtained with ice-sheet models that did not yet include any grounding-line treatment. Any hysteresis they found could well be a model artefact rather than a physically meaningful result.

Indeed, it could be an artifact, but smaller ice sheets also take into account the melt-elevation feedback. Now it reads:

“Their results also show that starting from PRISM4 conditions leads to higher sea-level contributions and a less extended AIS during the mPWP. This result is expected since a smaller ice sheet is forced by warmer surface temperatures due to the melt-elevation feedback, captured in our experiments through a lapse-rate factor. In addition, growing back on a retrograde marine basin needs a strong decrease in ocean temperature due to the hysteresis behavior of the ice sheet.”

L358-360: the arguments after “the one hand” and “the other hand” seem to point in the same direction?

Yes, we changed it to avoid confusion.

L362-363: “before the mPWP, CO_2 concentrations were below the pre-Industrial period, with sea-level estimates also below PD” Do you mean the M2 cold excursion? Be specific.

Yes, we refer to M2. We have rephrased the text as follows:

“The mPWP was preceded by a large global glaciation during Marine Isotope Stage M2 ca. 3.3 Ma BP (Rohling et al., 2014; Stap et al., 2016). During that period, the AIS evolved towards a modern-like configuration (Berends et al., 2019).”

L366-367: “only 3 out of 12 AOGCM models can be considered to realistically simulate warm Pliocene conditions, according to our simulations” I do not think your results are strong enough to discredit the results of any of the GCMs. Phrase this less strongly.

Indeed, this sentence sounds too strong the way it is framed and we cannot conclude this from just one ice-sheet model. We rephrased it to

“ Our model only simulates a retreat in the Wilkes basing, supported by reconstructions, for 3 out of 12 AOGCM models.”

L373-383: Unclear where you are going with this. Comparing to the Abumip results is meaningless here, as in those simulations all floating ice is destroyed in the models. The flux condition is not something you use, nor something you need – Alex Robinson already showed in the original Yelmo paper that your sub-grid friction scaling scheme works just fine.

We removed the part of ABUMIP and left a discussion paragraph of grounding-line treatment in our ice-sheet model. Now it reads:

“As shown by Pattyn et al., (2013), high resolution is needed at the grounding line to simulate accurate grounding-line migrations. In order to overcome this, ice-sheet models use different techniques at the grounding line to compensate for coarse resolution, such as flux conditions (Schoof 2007, Tsai et al., 2015) or scaling friction at the grounding line with its grounded ice fraction. In our study we use the latter technique which has shown to simulate realistic grounding-line migrations on idealized domains (a thorough description is presented in Robinson et al., 2020). We also ensure that effective pressure, which enters the basal friction equation, tends to zero as the ice thickness approaches flotation (Leguy et al., 2014). Nonetheless, grounding-line parameterisations remain as a source of uncertainty that can strongly influence the retreat of marine based glaciers prone to MISI”

L385: “the particular melting implementation at the grounding line is somewhat arbitrary” A strange way to phrase this. The fact that no perfect solution exists yet does not make the existing imperfect solutions “arbitrary”.

We rephrased it to

“Ice-sheet models use different approaches which typically range from no ocean-induced melting to partially ocean-induced melting.”

L391-392: “Given that we do not apply flux conditions or grounding-line melting, our results are more conservative than other studies” I’d argue that it is the other way round. Your model includes a sub-grid friction scaling scheme, which has been shown to work well. The models you compare with did not have any special grounding-line treatment, so that they likely would have severely underestimated any grounding-line retreat.

Yes, this is true, but since the focus of that paragraph is grounding-line melting and we do not apply grounding-line melting we believe that our results are rather conservative compared to other studies. We mention both effects in the discussion section.

L410-411: "Consequently, the model initialized with the PRISM4 ice-sheet thickness displayed persistent differences in simulated AIS characteristics compared to other initializations." That begs the question of how the ice sheet came to be so small in the first place, which I think needs to be discussed somewhere.

This raises an important question regarding PlioMIP2 boundary conditions. Direct evidence for ice free conditions are scarce for the Antarctic ice Sheet. Proxy records are limited to some marine records and land regions in the McMurdo Death valley (Shakun et al., 2018). PRISM4 boundary conditions are the same as PRISM3, and were generated using the British Antarctic Survey Ice Sheet Model (BASISM) with boundary conditions from an Atmospheric General Circulation Model (Dowsett et al., 2016).

Although this is not the aim of this study, we believe, based on our results and latest GIA model results (Richards et al., 2022; Hollyday et al., 2023), that the employed PlioMIP2 boundary conditions represent a lowest case scenario and that future work should potentially focus on the possibility of a larger Antarctic Ice Sheet during the mPWP. We added the following discussion paragraph:

"Since our Antarctic sea-level contributions do not exceed 10 mSLE, our simulations do not support a global sea-level contribution of more than 20 mSLE as suggested by some reconstructions (Dumitru et al., 2019; Hearty et al., 2020). Nonetheless, recent work done with Geodynamic Models suggest a lower contribution at the mPWP than proxy data. These models simulate dynamic topographic changes on specific domains, namely the Patagonian region (Hollyday et al., 2023), the Australian region (Richards et al., 2022) and the Atlantic shoreline (Moucha and Ruetenik, 2017). The main advantage compared to proxy data is that processes that are difficult to assess on in-situ measurements and have a big impact, such as geostatic uplift, can be considered. These results are then compared to proxy measurements from that region to assess the reality of their simulation. The new sea-level estimates reduce the global sea-level contribution significantly: 17.5 ± 6.4 msle (Hollyday et al., 2023); 16.0 ± 5.5 msle (Richards et al., 2022); 15 msle (Moucha and Ruetenik, 2017). Assuming that Greenland was almost fully melted (~ 7.4 msle, Morlighem et al. (2017)), with such a revised sea-level reconstruction, our results are inside the geological constraints if Wilkes basin collapses via high oceanic thermal forcing or with low precipitation rates, as in MRI-CGCM2.3 (Table 1 in SM). Richards et al. (2022) even go one step further and argue that the impact of the proposed MICI mechanism (DeConto and Pollard (2016; DeConto et al., 2021) is overestimated. Though this is not the scope of our work, these new results could highlight the need for new boundary conditions during the mPWP for the AOGCM, mainly a larger and thicker AIS than previously thought."

L417: "a lowering of PD precipitation could lead to such an irreversible retreat." Do you think a future decrease in precipitation is realistic?

This seems unrealistic since ISMIP6 projections show increased precipitation. We rephrased it to:

"[...] ...a lowering of PD precipitation, although not projected by AOGCMs in Antarctica (Seroussi et al., 2020), could lead to an irreversible retreat. Still, in the long term, such an instability could be also caused by an increase in melting. "

L419: "our simulated sea-level contributions ranged from -1.8 mSLE to -9.6 mSLE considering the whole ensemble." A negative contribution indicates a sea-level drop.

Indeed, we changed the sign.

L423: "as well as grounding-line migrations" Unclear what you mean here.

We changed it to "Our results reinforce the hypothesis …"

L424: "sea-level standings" = sea-level high stands?

Corrected.

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