Author response for egusphere-2024-4061

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Dear Editors and Reviewers,

We are extremely pleased that the editor and reviewers are satisfied following the first round of major revisions. With this round of revisions, we have aimed to significantly improve the clarity of the experiment design. This includes a more extensive methodology to explicitly clarify what and how we are testing, plus the removal of ambiguous wording, with the aim of the study clearly and concisely reiterated throughout. We have taken onboard the comments regarding the EOFs, and performed new, clearer analysis, taking into consideration the variance of the different experiments. This is accompanied with thorough statistics, helping to guide the reader further. We now delve deeper into deep water changes, with new analysis including; Hovmöller diagrams of salinity and temperature anomalies at 60°S and Potential temperature (θ)–salinity (T–S) diagrams of the Southern Ocean. We have also restructured and improved the discussion and conclusion, providing a clearer framing to better understand the mechanisms involved and contextualise our results relative to previous studies. We hope, with this new revision of the manuscript, we have addressed all remaining concerns, deeming the paper publication worthy.

Below follows a reply to each reviewer comment.

Sincerely,

Authors

Reviewer 1

Abstract: Changes to GrIS are prominent in the abstract. I understand that the GrIS albedo was also changed, however it is not shown in the manuscript, and quite unclear whether GrIS changes contribute to the pSH climatic changes discussed in the manuscript. I am not saying that you should not mention GrIS changes, however if you do not think that GrIS changes impact the pSH, then you might tone down the GrIS in the abstract.

#Response#: Thank you for highlighting, we have now changed the tone of the abstract to avoid focusing too much on the GrIS. It now reads:

"The Earth's ice sheets, including the Antarctic Ice Sheet (AIS), are critical tipping points in the climate system. In recent years, the potential future collapse has garnered increased attention due to its cascading effects, that could significantly alter global climate patterns, and cause large-scale, long-lasting, and potentially irreversible changes within human timescales. This study investigates the large-scale response of the polar Southern Hemisphere (pSH; comprising the Southern Ocean and Antarctica (60-90°S)) to the geometric reduction of ice-sheets to a reconstructed Late Pliocene (LP) extent, and imposing increased greenhouse gas forcing in the Earth System. Using the PRISM4D reconstruction, where ice sheets such as the West Antarctic Ice Sheet (WAIS) were significantly diminished, we conducted multi-centennial simulations with the EC-Earth3 model at atmospheric CO_2 concentrations of 280 ppmv and 400 ppmv."

L. 30: "oceanic" circulation

#Response#: Altered

L. 33: Is there really reduction of snowfall on the AIS and GrIS atm? There is more snowfall on EAIS. Please make sure this statement is precise.

#Response#: Thank you for pointing out the broad tone, the sentence is now clearer:

"However, the stability of these ice sheets is currently at risk due to climate change driven enhanced surface and basal melting, with melting rates projected to intensify in the following decades".

L. 43: MIS5e is mentioned here at 125,000 yrs ago, while in the discussion you mention the LIG. Please pick one or the other and use it consistently throughout the manuscript, noting that the PMIP LIG timeslice is at 127ka.

#Response#: To avoid inconsistencies we now only refer to the LIG.

Figure 1: What do the green lines represent? Please include it in the caption.

#Response#: Thank you for bringing this to our attention, the greenlines were used previously as an outline but looked messy and were not necessary to include here. We have reproduced a clearer figure (figure **??**) shows the ice mask, plus the continental outline of modern Antarctica to better understand the changes in extent of the ice mask.

Figure 4: Some statistics on the SAM indices are needed as it is hard to compare the 3 simulations in this way.

#Response#: We appreciate the comment and agree that the PC1 raw indexes were tricky to decipher. After reexamining the previous EOF analysis, we realised a crucial step had been missed in the processing of the data. Therefore we have reconducted EOF analysis, including a regression analysis of the seasonal zonal wind anomalies onto the first principal component (PC1) representing the SAM index. The new results are displayed in Figure 3, complete with variance. Reperforming the EOF calculation gave insight to much clearer patterns and results, which we now discuss with detail. With regards to your comment, we have also included a comprehensive statistics table showing variability, kurtosis and occurrence percentages of positive/negative and extreme events, describing the temporal behavior of the SAM PC1 in each experiment (Table 1). The new PC1 figure (figure 4) now also includes a 10 year smoothed running mean to highlight multi-decadal variability. We have, therefore, re-written this section.

	Median	**Std Dev**	**Kurtosis**	**Positive ($>$ 0)**	**Negative ($<$ 0)**	**Extreme Positive ($> +1\sigma$)**	**Extreme Negative ($< -1\sigma$)**
E280	0.040	43983.738	0.086	51.9%	48.1%	15.589	15.630
E400	-0.071	44550.820	0.217	46.7%	53.3%	15.962	14.712
Ei400	-0.035	45612.121	0.246	48.6%	51.4%	15.133	15.547

Table 1: Summary statistics - mean, variability (std), kurtosis, occurrence percentages of positive/negative and extreme events, describing the temporal behavior of the first principal component (PC1) representing the SAM index in each experiment. Standard Deviation and kurtosis are based on non-normalised data, all others on normalised data.

295-297: What do you make of this temperature increase at 1, 2 and 3km? It reads like you are linking it to the AABW change but without being explicit. At 2 and 3km, it just looks to me like a drift of the model (ie still trying to equilibrate to new conditions) and without link to AABW. Please also be more explicit with respect to the link between deep salinity and AABW. The downwelling AABW branch (counter clockwise circulation south of 60S) is not visible in Fig. 7, so I hope it is because of the smoothing used. My point: from fig. 7 it really looks like the AABW is much weaker in E400 and Ei400, but whether the AABW timeseries are correct is less clear. And what leads to the AABW recovery in Ei400 is even less clear.
#Response#: We appreciate the reviewer's concern regarding the potential influence of model drift in figure 9. However, we would like to clarify the lines of evidence that indicate that the recovery observed in Ei400 is a forced response to the imposed ice-sheet boundary conditions rather than drift:

1. **Our control simulation does not exhibit model drift.** The pre-industrial control experiment (E280) remains stable over the same integration length, with no comparable trend in the AABW index or in abyssal salinity and temperature, since it is a long simulation already branched off from an equilibrated pre-industrial spinup.
2. **E400 and Ei400 were initialized together with E280.** Both E400 and Ei400 branch from the same equilibrated pre-industrial spinup as E280 to enable their comparison (natural variability vs. imposed forcing), yet their long-term evolutions diverge strongly: E400 shows a persistent decline in AABW strength, while Ei400 exhibits a partial recovery. If the recovery were due to drift, a similar signal would be expected in E400, which is not the case.

#Response#: Even with our careful experimental design, we understand that the previous figures, coupled with our writing, failed to clarify that salinization of the deep ocean is the potential recovery mechanism in Ei400. To further address this concern, we have expanded the analysis, re-written part of the section, and added two new figures: The Hovmöller diagrams (Figure 3) of salinity and temperature anomalies reveal that the changes in thermohaline properties of the Southern Ocean at the latitude of maximum AABW strength (60°S) in response to Ei400 differ fundamentally from those in E400: while E400 shows progressive freshening and warming extending into the deep ocean, Ei400 exhibits stronger surface freshening that isolates the upper ocean and leads to stronger salinization of the intermediate and deep ocean, with less warming due to an initial cooling. This anomaly structure is inconsistent with uniform model drift but dynamically consistent with enhanced surface stratification under our modified ice-sheet boundary conditions.

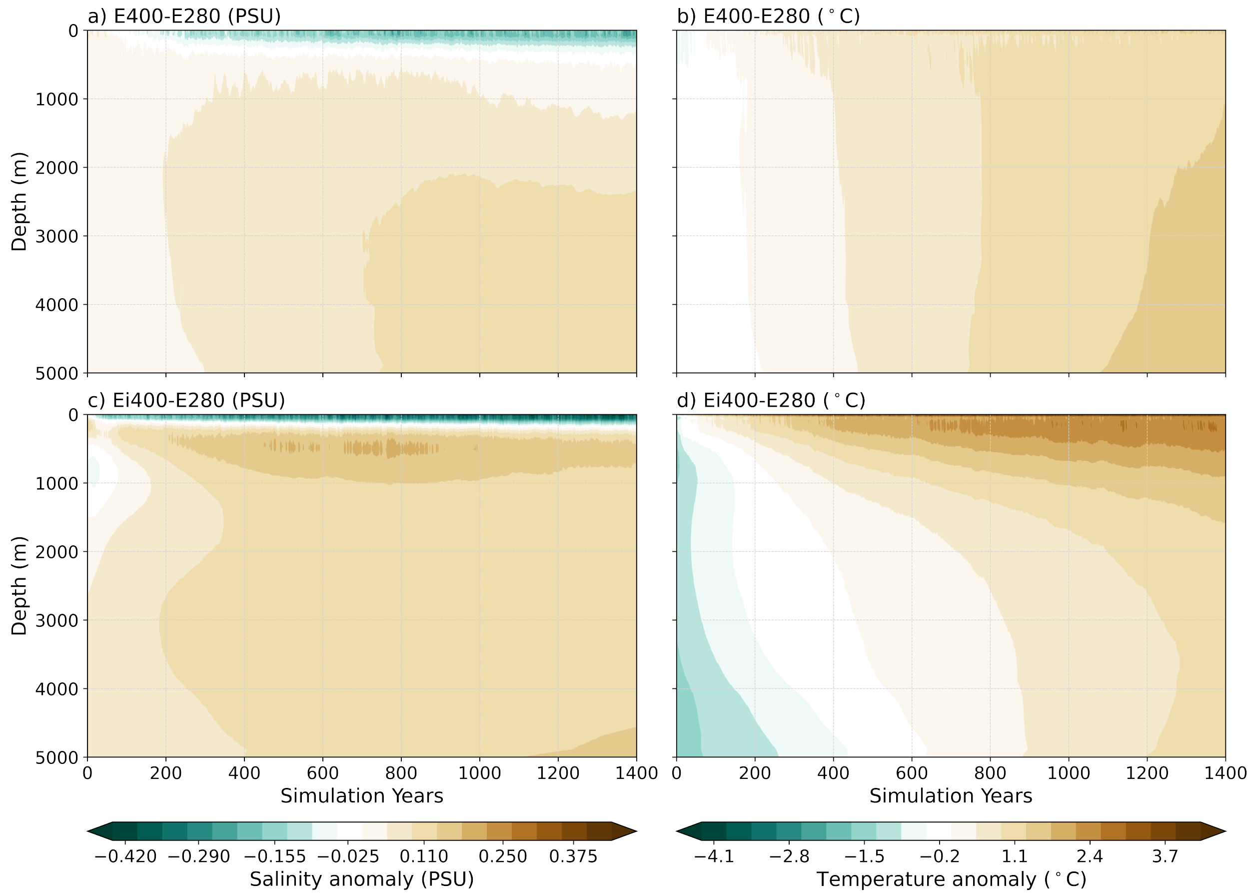

Figure 3: Hovmöller diagrams of salinity (a, b) and temperature (b, d) anomalies at 60°S, relative to E280 average at the same latitude, for E400 (a, b) and Ei400 (c, d).

#Response#: In addition, the T–S diagrams (Figure 4) show that AABW, represented by the deepest water masses (red), forms under warmer, saltier, and lighter conditions in E400 and Ei400 compared to E280, which is expected due to the imposed greenhouse gas forcing. However, relative to E400, the AABW in Ei400 is characterized by higher salinity and a broader temperature range, consistent with its gradual recovery. Finally, the vertical anomaly structures in salinity and temperature and the expansion of AABW formation shown in these figures provide a coherent recovery mechanism: enhanced surface freshening in Ei400 strengthens the halocline, isolating the ocean interior, which subsequently becomes saltier. The deep ocean warming is not as strong as to counteract this salinization effect in Ei400, since it happens progressively after an initial cooling of the ocean interior and limited to about 1°C in the deep ocean. In E400, on the other hand, this warming exceeds 1°C, the isolation effect of the upper ocean is not as strong, and the salinization of the deep ocean is not enough to enable strong AABW formation.

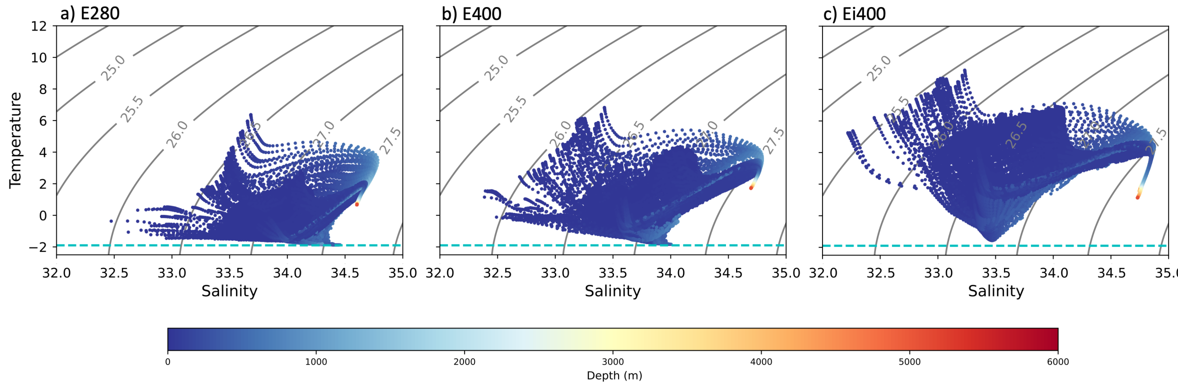

Figure 4: Temperature–salinity (T–S) diagrams of the Southern Ocean for E280 (a), E400 (b), and Ei400 (c), averaged over the last 200 simulation years

#Response#: Regarding the question about the downwelling of the AABW branch south of 65°S not being displayed as a counter clockwise circulation, it occurs simply because of the basin-scale integration that is done to derive the streamfunction output we use to display the overturning. In our simulations, the near-Antarctic overturning south of 65°S is weaker than 1.5 Sv in magnitude, thereby being partially cancelled in the basin-integrated streamfunction. An alternative would be to derive the overturning at different basins, which is out of the scope of our study. Nevertheless, the exported counterclockwise AABW cell is evident north of 65°S below 3000 m. Thus, the apparent red shading south of 65°S reflects the basin integration, not the absence of AABW formation.

L. 385-388: I have no problem with your experimental design, ie just changing the albedo, however I do not agree with stating that completely adjusting the AIS (and GrIS) to LP would lead to additional uncertainties, more over when you conclude (L 420) that the LP paleogeography should be implemented.

#Response#: Thank you for this point. We have revised the manuscript to clarify that this methodology is a deliberate, scope-limited choice rather than claim that orographic modifications are unimportant. We now present application of reconstructed Late Pliocene paleogeography as a complementary follow-up experiment rather than as an argument against it. In the restructuring of the discussion and conclusion sections, this is now within the conclusion and therefore has a improved flow to future work. Additionally, following the recommendations of reviewer 2, we include an example of a possible impact of orography change, L408-426.

L. 404-405: Please be more precise with respect to the results of Gorte et al. Which time period are they looking at? Why do they simulate weaker AABW? . . . In general, a more precise text with clearer conclusions with respect to the mechanisms would be beneficial for the paragraph 397-406. ie you could more precisely summarise what previous studies concluded with respect to the impact of i) meltwater input in the SO, ii) changes in AIS topography, iii) changes in SO stratification due to SO warming. You can then compare the results from iii) with your results and implications from currently missed processes i and ii in your simulations.

#Response#: Following this very helpful suggestion we have restructured the discussion section, L320-

400. This clearer framing improves the mechanistic interpretation and contextualises our results relative to previous studies. We discuss the overall occurrence and implications of changes in ocean circulation regimes during past warm periods, before delving into individual mechanisms with appropriate literature and geological reference. We then discuss the findings of our study and how these compare to previous investigations. The discussion specific to AABW, including the structure recommendations is found L368-400.

L. 420-424: Instead of bullet points, these could be better linked to the text L. 397-406.
#Response#: Thank you for the helpful suggestion regarding the presentation of future research directions. We have revised the manuscript to integrate the list of proposed sensitivity experiments into the narrative text for better flow and clarity, rather than presenting them as bullet points, L420-426.

Reviewer 2

It is still not clear to me what is being modified between the PI and LP experiments and what albedo effect you are testing. In L122 it says the albedo values are not modified and in your response you say 'the model retains the pre-industrial albedo values in those grid cells', referring to the grid cells that are no longer considered 'ice sheet'. Does this mean you are not testing the impact of reducing the surface albedo that comes from reducing ice sheet extent (direct albedo) but instead are only testing the indirect albedo changes due to LW energy balance changes? If this is the case, why do you say in the abstract L20 that you isolate the 'direct albedo effect'. I think it need to be explained more clearly what the effect is you are investigating, what is changed between the grid cells that were considered ice at the PI but not at the LP, and include justification for only testing the indirect effect when direct ice-albedo feedbacks are shown to be very important in modifying climate.

#Response#: Thank you for highlighting the need for a more detailed explanation here. To clarify, our sensitivity experiments isolate the climatic response to the geometric reduction of ice sheet extent while holding surface albedo and orography fixed at pre-industrial values. Therefore, we neither prescribe Late Pliocene-specific albedo changes or vegetation feedbacks, nor do we include freshwater forcing or topographic changes. This approach allows us to focus on the radiative and dynamical impacts of ice sheet retreat under a fixed albedo framework, avoiding the additional uncertainties inherent in reconstructing paleogeographic surface properties. We have updated the manuscript accordingly to reflect this distinction. To avoid ambiguity we no longer refer to the "albedo effect" and all uses of it, including in the abstract have been updated to appropriate terminology. We have revised and extended the methods to explicitly state what we have changed and clearly describe what we are testing L141-163:

L7 and L380 say that LP 'paleogeography' is used which is not completely accurate as orography/bathymetry isn't used only the ice extent.
#Response#: Thank you for pointing this out, we agree the term is misleading. It is now only used when discussing future possible experiments, and not in relation to the experiments of this study.

L131: why do you run the simulations for 1450 years, is it to equilibrium?
#Response#: Our sensitivity experiments, meaning the E280, E400, and Ei400 were all started in parallel after branching off from a quasi-equilibrated PI spinup spanning 800-years. We defined quasi-equilibrium as a global surface air temperature trend of less than 0.05 K per century. The extended integration allows us to examine multi-centennial variability, with a good compromise between the level of information and output frequency required to our simulations, and computational costs therein. However, as we analyze deep water formation regimes, we do not expect full equilibration of the ocean in the E400 and Ei400 experiments, since large-scale ocean circulation exhibit variability ranging from synoptic to multi-centennial and is subject to self-modulation. We have now clarified the spin up period in the manuscript, L112-118:

All simulations were started in parallel after branching off from a quasi-equilibrated PI spinup spanning 800-years, to ensure consistent baseline conditions and that any changes observed in the simulations are due to the perturbation and not model drift. We defined quasi-equilibrium as a global surface air temperature trend of less than 0.05 K per century. Thus, the E280 experiment represents our Pre-Industrial control simulation, which is a core experiment of PlioMIP2/3, while E400 and Ei400 represent our sensitivity experiments, being comprised within the Tier 2 experimental design of PlioMIP2 and continuing as optional but pivotal experiments in PlioMIP3 (Haywood et al., 2020, 2024).

L164: Figure ?? needs correcting
#Response#: Corrected with Figure 2

L275: Figure ?? needs correcting
#Response#: Corrected with Figure 7

L377-380: I think this discussion on gaps in existing research and how and why the LP is a good analogue for future climate scenarios could also be touched on more in the introduction to provide more context (e.g. what the GHG and temp levels were, evidence for how similar the ice sheet extents might be).
#Response#: We agree that adding this context would strengthen the introduction, and have now included comprehensive additional background on the LP, including specifics for Southern Hemisphere. This provides a clearer rationale for using the LP as an analogue for future climate change. We have revised the introduction to explicitly outline the limited number of studies focusing on Southern Ocean and Antarctic mechanisms during the LP, and state how our study addresses this knowledge gap by isolating the albedo effect of Antarctic ice-sheet reduction on the climate of the polar Southern Hemisphere, L40-72.

L384: Give an example of what the impact of including orography changes might be, include reference.
#Response#: We have revised the text to include an example of the potential impacts of orography changes, L410-413.

L428: how do you finding help identify early signals of ice sheet retreat? This isn't touched on or discussed anywhere else.

#Response#: We agree that the link to early signals of ice-sheet retreat was not sufficiently explained and have removed it from the text:

"These insights are critical for refining future climate models, offering a clearer picture of the potential pathways and risks associated with polar ice sheet instability"

Reviewer 3

The paper includes no analysis of any GrISANOM impacts; this should be reflected in both the introduction and results sections.
#Response#: Thank you for highlighting this discrepancy. We have removed most of the text that mentions the GrIS to avoid misleading the readers, as the focus of the paper is in the pSH, and other work already exists discussing the ramifications of changing the GrIS. This we mention in the introduction.

Considering Table 1; are all simulations initialised using the same spun-up pre industrial (PI) ocean state? Thus, E400 is an instantaneous N×PI increase in CO_2? This would mean that Ei280 is also not spun up (a simulation which doesn't seem to be analysed anyway; does it need to be included?).
#Response#: All simulations are initialized directly from quasi-equilibrated PI spinup simulation spanning 800-years to ensure consistent baseline conditions and that any changes observed in the simulations are due to the perturbation (e.g., CO_2 increase). This has been clarified in the text L12-115. The E400 experiment represents an instantaneous increase of CO_2 concentration from PI (280 ppmv) to 400 ppmv, applied directly during branching off from the spin up. This approach isolates the CO_2 effect on the climate system without allowing the ocean and atmosphere to adjust gradually beforehand. This is discussed with detail L116-120. We have removed our mentioning of the simulation Ei280, as it has not been analysed in our manuscript.

The Pliocene Antarctic and Greenland ice extents are mapped onto PI model states via adjustments land surface albedo. It is not clear how, by reducing snow depth, the albedo is modified. As I understand it, EC-Earth3 considers snow over certain depth to be perennial; with a fixed albedo and no accumulation allowed (a proxy for ice sheet surface conditions). If the snow depth is reduced below this threshold, is the surface considered to be seasonal, with albedo a weighted function of αsnow and αrock? With WAIS orographic height re- tained, I would expect multi-year snow to accumulate (unless this is inhibited, as per the perennial snow); maybe that is what is happening (see Fig.2(d), WAIS dα = 0 regions).
#Response#: We thank the reviewer for this detailed question about how snow depth reductions affect albedo in EC-Earth3 when mapping LP ice extents onto the PI model. The snow scheme in EC-Earth3 (based on ECMWF's HTESSEL land surface model) treats snow as perennial when snow depth exceeds a threshold (0.5 m of water equivalent), assigning a fixed high albedo and preventing further accumulation, effectively acting as a proxy for ice sheets. When snow depth is below this threshold, snow is considered seasonal and surface albedo is calculated as a weighted average between snow albedo and underlying surface albedo, reflecting seasonal snow cover variability (Döscher et al., 2022; Balsamo et al., 2009). In our experiments, the West Antarctic Ice Sheet (WAIS) orography is retained, but in regions where snow depth falls below

the perennial threshold, seasonal snow processes dominate, allowing accumulation and melt with seasonally varying albedo. This leads to heterogeneous surface albedo changes, including areas with little or no albedo change, as visible in Fig. 2(d). Thus, reducing snow depth modulates surface albedo by shifting grid cells between perennial snow (high fixed albedo) and seasonal snow (variable albedo), providing a physically consistent approach to represent ice retreat and its radiative effects in the model. This has now been included in the methods section for further clarity, L141-152.

The authors need to justify the usefulness of effectively transforming ice into rock (change of albedo, but no change of orography). What physical information does this provide (beyond what could be obtained from a simple energy-balance model)?

#Response#: We appreciate the reviewer's question about the physical justification for changing albedo without changing orography. This experimental design is intentional and serves to isolate the climatic response to the geometric removal of ice cover while keeping topography and other factors fixed. Specifically, by retaining pre-industrial (PI) albedo and topography values for grid cells where ice is removed, the model uses its default surface properties for that grid cell type, whether bare land or ocean, rather than applying more complex LP specific albedo or vegetation reconstructions. This approach allows us to focus on the first-order radiative effect. By doing so, we isolate both the radiative effect of ice loss, i.e., how the change in surface reflectivity influences the local and regional energy balance, and the dynamical response, including how these changes affect atmospheric circulation, wind patterns, and ocean feedbacks within the model framework. This controlled experimental design avoids confounding influences from additional forcings such as changes in vegetation, soil moisture, or orography, which may otherwise obscure the direct climatic impact of ice retreat. In this way, the experiment acts as a valuable idealized sensitivity test that serves as a baseline for understanding the isolated role of ice sheet retreat on polar Southern Hemisphere climate dynamics—insights that cannot be easily obtained from simplified energy-balance models that lack interactive atmospheric and ocean dynamics. We have ensured to specify this in the methodology section L153-163, ensuring our experiment design and purpose is clear.

The comparison of E400 with E280 is essentially a comparison of contemporary climate with PI (albeit, with an instantaneous CO_2 increase). The resulting Antarctic temperature changes are large when compared to observations or other modelling studies; this seems to be a result of excessive sea-ice loss. Is this similar to that seen previous EC-Earth3 historical simulations? The changes observed in Ei400 are harder to interpret without fully understanding the albedo adjustment (see above). The inland signals are dominated by the warming over the Ross, Ronne and Amery ice shelves; this is understandable as these are low altitude (warm) regimes that have been transformed into storage heaters.

#Response#: The E280 and E400 experiments correspond to pre-industrial (PI, 280 ppmv CO_2) and contemporary (400 ppmv CO_2) atmospheric CO_2 concentrations, respectively. The E400 experiment involves an instantaneous jump in CO_2 concentration to 400 ppmv and represents a "PI with elevated CO_2" scenario. This setup is part of the "Pliocene for Future" (P4F) agenda, designed to isolate and understand the effect of CO_2 changes alone on the climate system. Specifically, E400 was a Tier 2 experiment in PlioMIP2 and continues as an optional, but of high-importance, experiment in PlioMIP3 (Haywood et al., 2020, 2024), designed for forcing factorization analyses. Therefore, its primary purpose is to clarify how an elevated CO_2 level, without other boundary condition changes, affects climate, helping to separate CO_2 driven changes from other paleoclimate forcings. We have improved the experiment description accordingly (L116-120).

#Response#: We acknowledge that the Antarctic temperature increases observed through our findings are larger than those reported in observational studies and other modeling efforts. This is consistent with known biases in the EC-Earth3 model where enhanced warming is closely associated with significant sea-ice loss, amplifying regional warming. Döscher et al. (2022) demonstrates this, showing the EC-Earth3 historical ensemble underestimates maximum and minimum Antarctic sea-ice extent by approximately 5 and 2 million km^2 respectively, primarily due to a warm bias in the Southern Ocean. This underestimation leads to reduced sea-ice cover, which in turn contributes to higher surface temperatures and further sea-ice loss. This has been discussed in the discussion section. However, we would like to emphasize that E400 does not configure a modern climate experiment as per CMIP6 guidelines, since this is represented by the historical simulation, which includes many other radiative forcings through a transient approach. Therefore, comparisons from our results in this manuscript can only be completely made with reconstructions, respecting the evidence of biases also included in them, which does not affect the quality of our results and the significance of our modelling effort to the community. We discuss this L337-345.

#Response#: We also acknowledge that the climatic changes observed in the Ei400 experiment are more complex to interpret due to the albedo adjustment applied. As explained above, the adjustment in Ei400 involves changing surface reflectivity by effectively removing ice cover without altering orography or vegetation, isolating the radiative and dynamical impacts of ice loss alongside CO_2 increase. While this approach clarifies the direct effect of ice retreat on regional climate, it also might introduce nonlinear feedbacks and interactions within the atmosphere and ocean that can complicate interpretation. Ei400 should be viewed as an idealized sensitivity test rather than a fully realistic paleoclimate scenario, with the goal of disentangling individual processes. Finally, the observed inland Antarctic warming pattern dominated by the Ross, Ronne, and Amery ice shelves is consistent with physical expectations, as these low-altitude, relatively warm ice shelves act as heat reservoirs, facilitating heat transfer inland. We have highlighted this interpretation in our revised manuscript accordingly, L336-338.

Differencing of same field regressed principal component loading patterns (Fig 3(a,b)) is unusual, and difficult to interpret without any measure of significance. Does EOF1 in the three regimes represent a similar percentage of variance? If not, these differences would be problematic. Rather than (or in addition to) the EOF formulation, SAM indices of the Plat1 - Plat2 form would be easier to compare. Figure 3(c) shows an apparent intensification of SAM, but without the normal peninsular warming; the following ocean analysis also shows a decrease in on shelf CDW (the opposite to that expected) . It is suggested that E400i has a more chaotic pattern; this isn't reflected in Figure 4(c) (again variance measures are required). The wind pattern (Fig. 3(d)) is indicative of a weaker SAM, with a shift of the jet to more northern latitudes.

#Response#: Thank you for your insightful comments regarding the interpretation and comparison of the SAM patterns and variability across experiments. We agree that direct differencing of EOF loading patterns without accompanying statistical metrics can be difficult to interpret. After reexamining the previous EOF analysis, we realised a crucial step had been missed in the processing of the data, resulting in our previous results. We have reconducted EOF analysis, and now include a regression analysis of the seasonal zonal wind anomalies onto the first principal component (PC1) representing the SAM index. This provides a much improved insight into atmospheric changes. The new results are displayed in Figure 3, complete with variance. We have also included a comprehensive statistics table, describing the temporal behavior of the SAM PC1 in each experiment Table 1. Re-performing the EOF calculation gave insight to much clearer patterns and results, which we now discuss with detail. This clears up the opposite to expected effect surrounding the CDW, as now with increased CO_2 we establish a shift to a negative SAM phase - associated with reduced upwelling of cold, deep ocean water onto the Antarctic continental shelf. Furthermore, the wind and EOF patterns now align (Figure 5) and facilitates the interpretation of the transfer of near-surface climate response in our idealized experiments to the deep ocean.

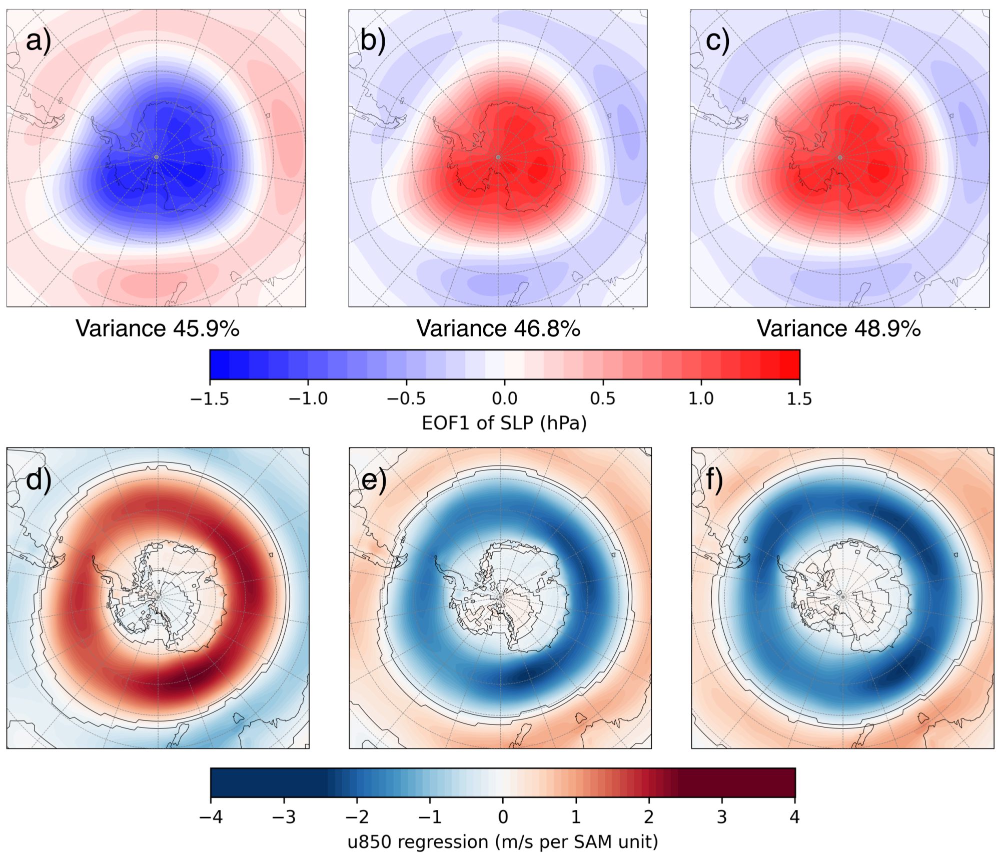

Figure 5: The Southern Annular Mode mean state as the first EOF of the SLP in hPa, for the a) PI Control, b) E400 and c) Ei400 experiments with % of variance explained by EOF1 notated for each. Regression of the austral summer (DJF) 850 hPa zonal wind onto the leading SAM principal component for d) E280, e) E400 and f) Ei400. Colors indicate the regression coefficient ($\mathrm{m\,s^{-1}}$ per SAM unit). Black contours denote statistically significant values ($p < 0.05$). % of variance explained by EOF1

The AABW recovery in Ei400 is of interest, and the mechanisms should be analysed further. There is an increase in salinity at depth, but this is associated with an increase in temperature; it would be beneficial to see the density profiles.

#Response#: We thank the reviewer for this constructive suggestion. We note that this point is closely related to Reviewer 1's comment regarding the interpretation of the recovery mechanism. In response, we have expanded our analysis and included additional diagnostics (Hovmöller diagrams (Figure 3) and T–S diagrams (Figure 4) that directly address the combined salinity and temperature changes and their implications for density. These are discussed L284-318. These figures demonstrate that AABW in Ei400 forms under warmer and saltier conditions than in E280, but that compared to E400 it is saltier and spans a broader temperature interval, consistent with a gradual recovery. We therefore believe that the new analysis requested by Reviewer 1 also covers the reviewer's helpful suggestion here.

When showing difference plots, show some measure of significance
#Response#: Significance tests have been performed for all difference plots, with only results statistically significant at the 95% confidence level displayed. This is now added into the figure captions.

Line 199; missing figure number
#Response#: Corrected

References

Balsamo, G., Beljaars, A., Scipal, K., Viterbo, P., Van Den Hurk, B., Hirschi, M., and Betts, A. K. (2009). A Revised Hydrology for the ECMWF Model: Verification from Field Site to Terrestrial Water Storage and Impact in the Integrated Forecast System. *Journal of Hydrometeorology*, 10(3):623–643.

Döscher, R., Acosta, M., Alessandri, A., Anthoni, P., Arsouze, T., Bergman, T., Bernardello, R., Boussetta, S., Caron, L.-P., Carver, G., Castrillo, M., Catalano, F., Cvijanovic, I., Davini, P., Dekker, E., Doblas-Reyes, F. J., Docquier, D., Echevarria, P., Fladrich, U., Fuentes-Franco, R., Gröger, M., v. Hardenberg, J., Hieronymus, J., Karami, M. P., Keskinen, J.-P., Koenigk, T., Makkonen, R., Massonnet, F., Ménégoz, M., Miller, P. A., Moreno-Chamarro, E., Nieradzik, L., van Noije, T., Nolan, P., O'Donnell, D., Ollinaho, P., van den Oord, G., Ortega, P., Prims, O. T., Ramos, A., Reerink, T., Rousset, C., Ruprich-Robert, Y., Le Sager, P., Schmith, T., Schrödner, R., Serva, F., Sicardi, V., Sloth Madsen, M., Smith, B., Tian, T., Tourigny, E., Uotila, P., Vancoppenolle, M., Wang, S., Wårlind, D., Willén, U., Wyser, K., Yang, S., Yepes-Arbós, X., and Zhang, Q. (2022). The EC-Earth3 Earth system model for the Coupled Model Intercomparison Project 6. *Geoscientific Model Development*, 15(7):2973–3020.

Haywood, Tindall, J. C., Dowsett, H. J., Dolan, A. M., Foley, K. M., Hunter, S. J., Hill, D. J., Chan, W.-L., Abe-Ouchi, A., Stepanek, C., Lohmann, G., Chandan, D., Peltier, W. R., Tan, N., Contoux, C., Ramstein, G., Li, X., Zhang, Z., Guo, C., Nisancioglu, K. H., Zhang, Q., Li, Q., Kamae, Y., Chandler, M. A., Sohl, L. E., Otto-Bliesner, B. L., Feng, R., Brady, E. C., von der Heydt, A. S., Baatsen, M. L. J., and Lunt, D. J. (2020). The Pliocene Model Intercomparison Project Phase 2: large-scale climate features and climate sensitivity. *Climate of the Past*, 16(6):2095–2123.

Haywood, A. M., Tindall, J. C., Burton, L. E., Chandler, M. A., Dolan, A. M., Dowsett, H. J., Feng, R., Fletcher, T. L., Foley, K. M., Hill, D. J., Hunter, S. J., Otto-Bliesner, B. L., Lunt, D. J., Robinson, M. M., and Salzmann, U. (2024). Pliocene Model Intercomparison Project Phase 3 (PlioMIP3) – Science plan and experimental design. *Global and Planetary Change*, 232:104316.