Answer to reviewer 2 of Mathiot et al. (2026)

General Comment:

This study investigates the reversibility of oceanic and ice-sheet changes in Antarctica using a coupled ocean–ice-sheet model. The topic is important because the mass balance of the Antarctic Ice Sheet directly affects future sea-level rise, and ocean-driven melting beneath ice shelves can influence the dynamics of the grounded ice sheet through changes in buttressing and grounding-line retreat. Understanding how the Antarctic Ice Sheet responds to strong oceanic and atmospheric perturbations is therefore of broad relevance beyond the scientific community.

The coupled modeling approach adopted in this study is valuable, and the contrast between a relatively rapid recovery of the ocean state and a much slower, partly irreversible ice-sheet response is potentially interesting. However, I have several concerns about the interpretation of the results, particularly regarding the robustness of the ocean reversibility claim and the influence of model assumptions such as the fixed ice-front position and the treatment of pseudo-collapsed ice shelves. I also think that additional diagnostics of the atmospheric forcing, sea-ice processes, regional ocean response, and ice-shelf geometry are needed to support the main conclusions.

I hope the authors will address these points in a revised manuscript.

We thank the referee for their constructive feedbacks. The suggested additional diagnostics and discussions on the perturbation, the underlying processes, and the model limitations have been implemented and ensure that the conclusions of this study are more robust, balanced, and transparent. Below, you will find a point-by-point response, with the reviewer's comments in blue, our answers in black, and the changes made to the manuscript in purple.

Major Comments:

1. Interpretation of ocean reversibility under the fixed ice-front assumption

C1: One of my main concerns is that the conclusion regarding the rapid reversibility of the ocean state may depend strongly on the model assumptions, especially the fixed position of the ice front / calving front. In the present model framework, the ice-shelf fronts do not retreat, even under very large increases in basal melting. As a result, the experiments do not represent potentially important changes in ocean geometry associated with ice-shelf front retreat, ice-shelf collapse, or the transition from an ice-shelf cavity to open ocean.

This limitation is important for the interpretation of the reversibility experiment. The manuscript shows that ocean properties and basal melt rates largely recover after the atmospheric perturbation is removed. However, this recovery occurs within a framework in which the ice-front geometry is artificially maintained. Therefore, the result should be interpreted more cautiously as ocean reversibility within a fixed ice-front configuration, rather than as evidence that the Southern Ocean–ice-shelf system would generally recover rapidly after major ice-shelf retreat or collapse.

I therefore think that statements such as the Southern Ocean itself would not contribute to making large climate transitions irreversible are too strong in the current form. The authors should qualify this conclusion more explicitly and discuss how the fixed ice-front assumption may affect the simulated ocean recovery, especially through its influence on cavity geometry, sea-ice formation, dense-water production, and CDW access to the continental shelf.

Related to this point, the treatment of pseudo-collapsed areas should also be examined more carefully. In the model, strongly thinned ice-shelf areas are retained as 1-m-thick ice shelves, whereas in reality such areas might become open ocean after ice-shelf breakup or calving-front retreat. A useful sensitivity experiment would be to remove the pseudo-collapsed areas and treat them as open ocean at the start of the REV experiment. This would help assess whether the rapid ocean recovery is robust to a more realistic representation of ice-shelf collapse and changes in cavity geometry.

R1:
We thank the reviewer for raising this important point and for questioning the robustness of our conclusions regarding ocean reversibility. The limitation of the fixed ice-front assumption was also noted by Reviewer 1. We have addressed this concern by adding discussion throughout the manuscript (in the ocean reversibility discussion, structural limitation section, and conclusion).

The ocean reversibility discussion as been updated as follows:

A new result is that transitions to a warm state appear to be reversible within 40~years for most shelves in our simulations (Fig. 5). It is important to stress that this was estimated by assuming that the atmospheric state can be reversed instantaneously, which is not realistic *, and by assuming a fixed ice-front. The potential impacts of ice-shelf collapse and evolving cavity geometry on ocean reversibility are discussed in section 5.* We nonetheless believe that these results are useful for understanding the climate system, as they indicate that the Southern Ocean itself *may* not contribute making large climate transitions irreversible as it was suggested in previous work.

In the structural limitation section, we added a detailed discussion on the possible impacts of such model limtations on the ocean and the ice sheet:

Our study focuses on ice–ocean interactions and does not include a fully coupled ocean–atmosphere–ice sheet system with evolving land, ocean, and ice sheet fractions, so we cannot quantitatively assess how atmospheric forcing over newly ice-free regions would influence the ocean-ice sheet system's reversibility. In the absence of moving calving front, for stability reason, we impose a minimum thickness of 1 m, which maintains a thin ice layer that isolates the ocean from direct atmospheric forcing compared to open water, while also facilitating regrowth under cold conditions compared to a real ice shelf front that would need to readvance little by little.

Our perturbation induces retreat of most ice shelf fronts (Amery, Ross and FRIS) or even pseudo-collapse for some ice shelves (e.g., Getz, Dotson/Crosson, Abbot, Cosgrove, the Larsens, ...). Such changes, in a fully coupled system, would result in a reduced ice shelf area *and more open water exposed to the atmosphere, ocean heat transport into the ice shelf cavities (Bradley et al., 2022), with significant uncertainties related to the presence of sea ice, icebergs and polynyas in deglaciated areas.*

In the conclusion section, we have explicitly mentioned this limitation and open on the need of coupled ocean / atmosphere / ice sheet models. The changes are:

However, this conclusion must be interpreted with caution. The fixed ice-front assumption used in our modeling framework prevents us from capturing the feedbacks associated with ice-shelf weakening or collapse, open-ocean exposure, and atmospheric interactions that would occur in a fully coupled system. The reduced ice-shelf area or ice-shelf collapse should increase the ocean surface seen by the atmosphere, which may enhance sea-ice formation and brine rejection or limit CDW intrusions, potentially favouring ocean reversibility. However, the associated atmosphere warming over the newly exposed ocean is unknown and may suppress the enhanced sea ice formation. This highlights the need for fully coupled ocean–atmosphere–ice-sheet models to realistically assess the system's response to perturbations and the potential for cascading feedbacks.

Unfortunately, it is not possible to perform the suggested sensitivity experiment without a fully coupled ocean–atmosphere–ice-sheet model, as the atmospheric forcing used in our study is based on the JRA atmospheric reanalysis, which assumes an ice sheet surface rather than open ocean in pseudo-collapsed areas. This would mean applying atmospheric properties inconsistent with the actual surface type and potentially much colder than over ocean surface.

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2. Interpretation of basal melt in pseudo-collapsed areas

C2 A substantial fraction of basal melt occurs in pseudo-collapsed areas, where the ice shelf is artificially maintained at a minimum thickness of 1 m. Melt in these regions may not represent typical CDW-driven melting in deep ice-shelf cavities. Because the ice base is located close to the ocean surface, melting there may be more strongly affected by near-surface warming and sea-ice loss, corresponding more closely to the "mode 3" melting described by Jacobs et al. (1992).

More generally, the manuscript mainly discusses CDW-driven basal melting and MISI-driven grounding-line retreat. I think the introduction should provide a broader description of ice-shelf melt regimes, for example following Jacobs et al. (1992), and clarify which melt mechanisms are relevant in the present experiments.

Jacobs, S.S., H.H. Hellmer, C.S.M. Doake, A. Jenkins, and R.M. Frolich. 1992. Melting of ice shelves and the mass balance of Antarctica. Journal of Glaciology 38(130):375–387.

R2
We thank the reviewer for its insightful comment. To address this, we have now included a broader description of ice-shelf melt regimes in the introduction, following Jacobs et al. (1992). This clarifies the different mechanisms at play (Modes 1, 2, and 3) in a context of oceanic tipping points.

The introduction now includes:

In Antarctica, two plausible tipping points have been suggested: (i)~an ocean tipping point in which an ice-shelf cavity switches from a cold state with low melt rates to a warm state with high melt rates (Hellmer et al., 2017), and (ii)~an ice-sheet tipping point, often described in terms of ice-sheet instability (Weertman et al., 1974), in which the grounded ice flow abruptly accelerates, thinning the ice sheet and ice shelves, while the grounding line retreats inland. *These cold and warm states correspond to different melt regimes, as identified by Jacobs et al. (1992): melting by high-salinity shelf water formed during winter freezing (Mode 1) and melting by warm Circumpolar Deep Water intrusions (Mode 2). A third mode of melting in shallow areas driven by tidal pumping and seasonal warming of coastal currents is also identified. If ice shelves thin, Mode 3 may become increasingly relevant in a coupled system, particularly if ocean and ice sheet tipping points are crossed.*

One sentence has also been added to the discussion on the ocean reversibility:

In these shallower ice shelves, melting may also be more influenced by near-surface processes, such as seasonal cycle of surface water (Mode 3 melting; Jacobs et al., 1992), potentially further reducing basal melt rates compared to mode 1 (HSSW) or 2 (Getz).

C2.1 Similarly, the discussion of ice-sheet retreat could briefly acknowledge other possible instability mechanisms, such as marine ice-cliff instability, even if they are not represented in the model. This would help place the simulated CDW–MISI pathway in a broader physical context.

R2.1 Hydrofracturing and MICI has been added in the general discussion in the paragraph concerning the structural limitations of our work:

In this study, under present day surface mass balance, ice shelves remain safe from hydrofracturing. Using surface mass balance typical of 2300 as it is done for the ocean may lead to hydrofracturing and larger ice shelf collapse (Jourdain et al., 2025), leading to potential Marine Ice Cliff instability (MICI; Pollard et al., 2015).

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3. Need for a more self-contained description of the ocean forcing and transition

C3: The ocean transition mechanism appears to be largely the same as in MJ2023. This is not necessarily a problem, but the present manuscript relies too heavily on MJ2023 to explain the ocean response. As a result, it is difficult to understand the physical meaning of the perturbation and the resulting cold-to-warm transition from this manuscript alone. Because the imposed atmospheric perturbation controls the entire PERT experiment, the manuscript should show the key atmospheric anomalies, at least in the main text or supplementary material. Useful diagnostics would include near-surface air temperature, wind stress, freshwater flux, and radiative flux anomalies. Overall, I think the manuscript should be more self-contained in its description of the ocean forcing and ocean transition, so that readers do not need to rely on MJ2023 to understand the main physical mechanism.

R3: We agree that the present manuscript heavily rely in MJ2023 as the ocean model configuration and the atmospheric perturbation over the ocean is the same. In the revised version, we have done our best to limit the link to MJ2023 without increasing much the length of the paper.

- **Atmospheric forcing:** as suggested, we have added in appendix the anomaly figure from MJ2023 and added a description on the strength on the anomalies on air temperature, precip and wind.

  The atmospheric anomalies exhibit a pronounced increase in precipitation, particularly along the ice sheet margins; surface air warming significantly exceeding the global mean, most notably during the austral winter with more than 20°C of warming over the Antarctic continental shelf (polar amplification); and a strengthening and southward shift of the westerly winds around Antarctica, mostly in austral summer and autumn (Fig. A1).

- **Melt rate comparison:** we added Rignot et al. (2013) and MJ2023 REF and PERT meltrate on figure 4 to make the discussion of this results self contained. We also added one sentence to explicitly mentioned that REF basal melt represents well the observational estimates:

  Similar to MJ2023 REF, the mean basal mass loss in the coupled REF simulation (1,204 Gt a^{-1}) is also consistent with observational estimates for Antarctica of 1,325 ± 235 Gt a^{-1} for the 2000s (Rignot et al., 2013) and of 965 ± 265 Gt a^{-1} over 1992–2017 \citep{paolo23}. At the scale of individual ice shelves, the mean basal mass loss also generally matches observational estimates (Fig. 4).

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4. Need for sea-ice and dense-water diagnostics

C4: The manuscript explains the ocean transition as a consequence of reduced sea-ice formation, weakened brine rejection, and the collapse of dense shelf-water formation. However, these key processes are not sufficiently documented in the results.

I suggest that the authors show diagnostics such as sea-ice concentration or extent, sea-ice production, brine rejection, and dense shelf-water formation. These diagnostics should be shown for REF, PERT, and REV, because they are central to the proposed mechanism of both the cold-to-warm transition and the subsequent ocean recovery.

R4: We agree. A new figure showing time series of sea ice area, mean ice production (wich is a proxy for brine rejection) as well as mean bottom salinity (proxy for dense water formation) has been added on Ross and Filchner/Ronne continental shelf. This new figure supports the result discussion on the processes that drive the ocean reversibility.

Ocean properties on the continental shelf appear to be entirely reversible within 70 years. Switching back to the reference atmospheric forcing is enough to restore sea ice area *(Fig. 5a)* and brine rejection *(Fig. 5b)* to the reference states. This triggers the formation of new HSSW *(Fig. 5c)* that spreads on all former dense shelves, rebuilding shelf dense water and the density barrier that prevents intrusion of warm CDW (Fig. 3).

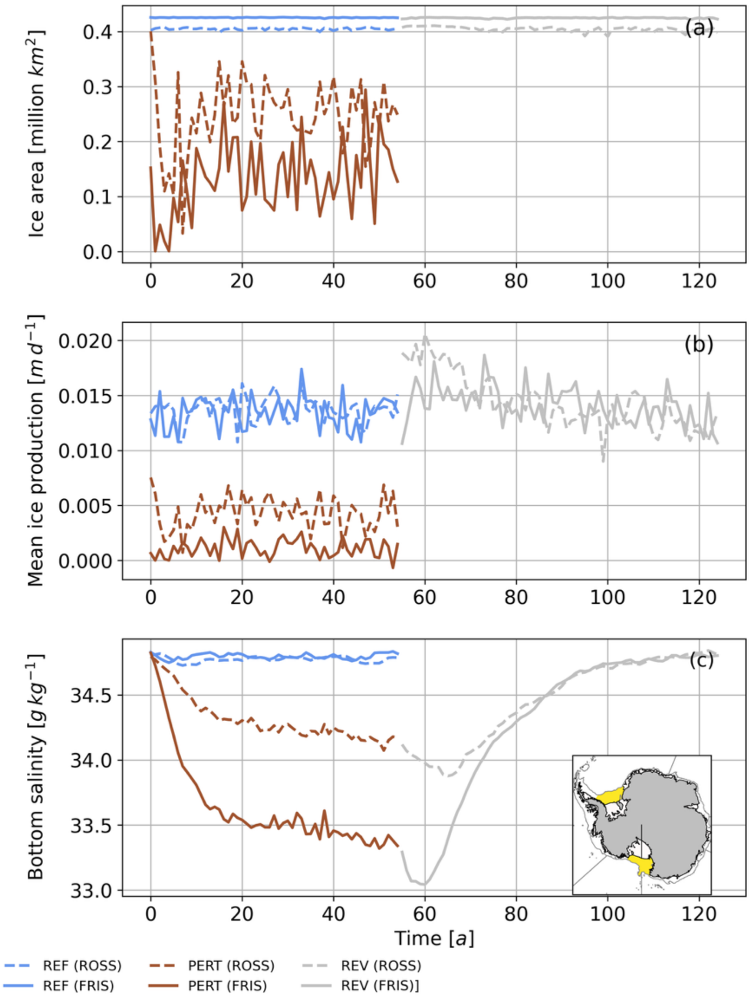

Figure 5. (a) Time series of sea ice area, (b) mean sea ice production, and (c) and bottom salinity on Ross and Filchner-Ronne continental shelf.

5. Need for a more regional analysis of the ocean response

C5: The ice-sheet response is analyzed in detail at the basin and ice-stream scale, but the ocean response is mostly discussed using Antarctic-wide averages and a few representative sections. This makes the ocean analysis less balanced than the ice-sheet analysis. I suggest that the authors provide more regional ocean diagnostics, especially for sectors where irreversible ice-sheet retreat is identified.

R5: We appreciate the reviewer's suggestion to include more regional ocean diagnostics. However, our analysis reveals that the ocean response to the perturbation and its reversibility are remarkably consistent across all continental shelf sectors, regardless of whether the ice sheet exhibits a strong response to the perturbation or signs of irreversibility. Given this uniformity, we believe that Antarctic-wide averages, Antarctica spatial map and representative sections sufficiently capture the ocean's behavior in this context. Last but not least, we remind that a comprehensive description of the ocean transition to a warm state was provided in Mathiot and Jourdain (2023).

Other comments:

C6: The discussion of basal melt rates in Section 3.1 refers to differences from MJ2023 and to the observational estimates of Rignot et al. (2013). However, these values are not shown in Fig. 4, which makes the comparison difficult to follow. I suggest adding the MJ2023 and Rignot et al. (2013) values to Fig. 4, or providing an additional figure/table summarizing these comparisons for the major ice shelves.

R6: MJ2023 and Rignot et al. (2013) basal melts have been added to the figure 4 as suggested.