Answer to reviewer 1 of Mathiot et al. (2026)

We sincerely thank the referee for their thorough and insightful feedbacks. The additional model validation and the model limitations discussion have significantly improved the robustness, balance, and transparency of our conclusions. 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.

GENERAL COMMENTS

C1: The authors are investigating the effect of a sudden increased warming to the Southern ocean and the Antarctic ice sheet in a coupled ice-ocean simulation.

They base their simulation setup on an earlier stand-alone simulation and chose a high warming scenario to investigate the possibility of reversibility after a 50yrs increased warming period. Finding that while the ocean is relatively quickly reverting to its initial conditions that the ice sheet takes longer to revert and that a few ice streams undergo irreversible retreat.

The study fits within the scope of TC, presents its findings in a clear way and is well written.

The findings are not especially novel, but contribute to the ongoing discussion of tipping points as part of several studies with the important aspect of ice-ocean coupling which is needed to better understand the complex Antarctic system of atmosphere, ocean and ice.

R1: We acknowledge that ice-sheet or ocean tipping points have been discussed in previous studies, but we believe that a novel aspect is the systematic study of both ocean and ice-sheet potential tipping points at the scale of Antarctica, analysing tens of ice shelf cavities and ice streams. Previous work focusing on the Weddell Sea had shown potential for the Weddell Sea to irreversibly tip to a warmer state (Hellmer et al., 2017; Hazel et al. 2022), which raised the fear that there might be domino effects between the ocean and ice sheet tipping points. Here, with a fully coupled ocean-ice sheet model at the scale of Antarctica (such models have only been used in 2-3 studies), we show that such domino effect is not found in any region of Antarctica, at least with the type of perturbations that we have prescribed. We also believe that the identification of tipping points for specific ice streams provides new insights into the reversibility of the ice sheet dynamics, complementing the broader discussion on ice sheet tipping points.

Overall comments:

Observations and present-day conditions:

C2: How do your ocean and ice conditions at the start of the simulation relate to observations or other studies during the initial time period? Are your melt rates reasonable and the ice velocities and thickness reasonable? This information would be important for readers to better place the changes during the warming period, and the subsequent reversed state, into context.

R2:
We thank the reviewer for this important comment. To provide context for the changes during the warming period and subsequent reversed state, we have added validation for the ocean and ice sheet over the first 15 years of the REF simulation. For the ocean, we focus on water mass properties and provide comparisons to the WOA2018 climatology. For melt rates, we include observational comparisons and references to Mathiot and Jourdain (2023). An assessment of the mean ice sheet state (e.g., volume trends, grounding line flux) has been added and discussed in the results section.

For the ocean validation, the following text has been added:

The present-day simulation described in this paper extends the MJ2023 REF configuration by coupling it with an ice sheet model. Given the similar geometries, REF provides a relatively accurate representation of ocean-ice properties in the Southern Ocean, comparable to MJ2023 REF. Here, we focus on the shelf properties that drive ice shelf basal melt, the key factor for ice sheet–ocean interactions. Based on the climatology of the last 10 years of the present-day simulation (2009–2018), our REF simulation produces High Salinity Shelf Water (HSSW) in key regions such as the Ross Ice Shelf, Terra Nova Bay polynyas, and Ronne polynya, with a reasonable fresh bias of 0.05 g/kg, though it is slightly too fresh in Prydz Bay and East Antarctica, likely due to the absence of iceberg-induced polynyas (Fig. A2 abc). REF also accurately represents weakly modified Circumpolar Deep Water in the Bellingshausen and Amundsen Seas, consistent with WOA2018 data (Locarnini et al., 2013; Zweng et al., 2019), and simulates near-freezing temperatures in the Weddell and Ross continental shelves. While simulated temperatures are colder than WOA2018 in the Indian Ocean sector of East Antarctica (except Prydz Bay/Amery), this comparison should be interpreted with caution due to sparse observations in this region (Fig. A2def). A detailed validation of the ocean standalone simulation is available in MJ2023.

For the melt rate validation, the following text has been added:

Similar to MJ2023 REF, the mean basal mass loss in the coupled REF simulation (1,182 Gt a^{-1}) is consistent with observational estimates for Antarctica of 1,325±235Gt a^{-1} for the 2000s (Rignot et al., 2013) and of 965 ± 265 Gt a^{-1} over 1992–2017 (Paolo et al., 2023). At the scale of individual ice shelves, the mean basal mass loss also generally matches observational estimates (Fig. 4).

For the ice sheet validation, the following text has been added:

Despite a geometry initialization from Bedmachine v2 and viscosity and friction fields derived from an inversion constrained by observed velocity (see Sec. 2.1.2), the model simulates a grounding line (GL) flux of 1554 Gt a^{-1}, which is 25\% lower than the estimates of Rignot et al. (2013). Therefore, the mass trend for the same period (+1 Gt a^{-1}) is higher than the IMBIE estimates of -106 Gt a^{-1} for 1995–2014 (Shepherd et al., 2018). This growing bias is quite common in ice-sheet models (Seroussi et al., 2020; Aschwanden et al., 2021) and arise from challenges in simulating the high melt rates observed at the GL with NEMO (MJ2023), the absence of ice shelf front retreat and associated buttressing loss due to calving, the lack of evolving viscosity due to damage, or the omission of subglacial hydrology. However, since PERT and REV are analyzed relative to REF or REF_{ext}, these biases are unlikely to substantially affect the main conclusions of the analyses presented here.

Ocean boundary conditions:

C3: While you address a few times the atmospheric forcing being the driver of changes and reversibility, I am missing the mentioning or explanations of the setting for the ocean-ocean boundary conditions. Do they play a role? Are they too far away to impact the continental shelf and ice cavities? (see also comment below in "specific comments"). It would be helpful to mention somewhere (2.1.1?) how this boundary forcing is implemented and to clarify why they are not important for or discussed in the results and discussion sections.

R3: As stated in the manuscript in section 2.1 (Coupled ocean/sea-ice/ice-sheet Model) and section 2.1.1 (Ocean/sea-ice component: NEMO), we used a global ocean model configuration so that there are no ocean lateral boundary conditions to prescribe.

1m ice shelf minimum thickness

C5: L111-115: I understand the choice of minimum 1m ice shelf thickness. However, how does this additional 1m of ice impact the ocean compared to having no ice? And are those locations crucial for the ice shelves itself providing stability versus no ice might lead to quicker retreat? You discuss this briefly in line 334ff, but I think it is worth explaining whether, and to what extent, this choice affects ocean circulation, particularly if a larger area in front of the ice shelf is directly exposed to atmospheric forcing rather than being covered by a thin ice layer. Related to this, how might this influence ice-shelf or grounding-line retreat, beyond the overestimated meltwater flux into the ocean?

C6: Is the affected area large enough that atmospheric forcing over these ice-free regions would significantly influence the system, potentially making reversibility more difficult or slowing it down? Furthermore, it would be useful to explicitly acknowledge the limitation of not including feedbacks from the ocean to the atmosphere, and to discuss whether such feedbacks could enable or strengthen tipping-point behavior. While I understand that the focus of the study is on ice–ocean interactions, a brief discussion of the neglected feedbacks, and a qualitative estimate of their potential impact on the main conclusions, would strengthen the manuscript, particularly given the emphasis on tipping points and reversibility.

R5 and R6: These are very good questions. It is very hard to answer because to answer these points we would need a coupled ocean / atmosphere / ice sheet model with evolving land/ocean fraction in the atmosphere model to allow the atmospheric dynamics to adjust to the new exposed water. Such tool is not available yet. In the general discussion, we have added a paragraph mentioning potential mechanism neglected and there impact. This paragraph has been added to the text:

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 seen by the atmosphere, so we cannot quantitatively assess how atmospheric forcing over newly ice-free regions would influence the 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, will result in a reduced ice shelf area and more open water exposed to the atmosphere, affecting 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.

And in the conclusion, we also added a paragraph to mentioned that too and open on the need to use atmosphere / ocean / ice sheet coupled model:

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.

SPECIFIC COMMENTS

C7: L183ff: The reversibility is described as being directly affected by the change in atmospheric forcing. How are ocean-ocean boundary conditions prescribed? Are they important or maybe just mention that they aren't and why.

R7: See **R3**

C8: L220-227:This paragraph seems important, but I struggle to understand the relationships, relative importance, and impacts of the factors mentioned on the overall outcome, as well as what the authors intend to convey here. Could you please reformulate this paragraph or add clarifying information to make the main points more clear?

R8: We reformulate the paragraph to be more explicit on the relationship and the impact of the various factors mentioned. The new paragraph is:

Second, the tipping/reversal threshold is determined by the interplay between the activity of the Ronne ice-shelf polynya (Moorman et al., 2023; Saddier et al., 2026), basal melting beneath Filchner-Ronne (Hellmer et al, 2017), and advection from the westward coastal current (Hazel et al., 2020; Bull et al., 2021). Therefore, any model bias or difference in the modelling setup may impact the tipping behaviour.

In particular, polynya activity plays a critical role by setting the dense shelf water density, which is the main driver of the density barrier preventing CDW from spreading onto the continental shelf (Naughten et al., 2021). This activity is strongly sensitive to the resolution and fidelity of atmospheric forcing. For example, the reanalysis used in our study and in Haid et al. (2023) may represent this activity more accurately than in the coarse climate models used in Hellmer et al. (2017). Similarly, the properties of CDW in regional simulations, such as those in Hellmer et al. (2017) and Hazel et al. (2020), are highly sensitive to the choice of ocean lateral boundary conditions compared to the global ocean simulations used in our study and in Haid et al. (2023). These modeling choices affect the response of CDW to changes in atmospheric forcing and, consequently, its ability to intrude onto the continental shelf.

C9.1: Fig 6: Please add a legend for the grounding line colors. Rename second (b) to (c). Maybe consider switching the color scheme: red for less ice and blue for more ice?

R9.1: DONE

C9.2: Fig 6: (c) and (d): it looks like there is thinning happening around the fast flowing areas on the grounded ice areas. Is this due ice velocity and grounding line change in reaction to the reverted ocean conditions?

R9.2: The thinning observed in Figure 6 (c) and (d) around fast-flowing grounded ice areas is linked to changes in ice velocity and grounding line position. This thinning primarily results from grounding line retreat during PERT that reaches unstable positions. All locations where this thinning occurs in REV (relative to PERT) correspond to areas with potential irreversibility at the end of REV, i.e. where the grounding line has retreated beyond its original PERT position and does not re-advance even after ocean conditions revert to their original state. This retreat leads to reduced buttressing, accelerated ice flow, and dynamic thinning. As these 2 figures where not discussed in the result section, we added this in the reversibility part of the results:

After 70 years, REV exhibit locations thinner than at the end of PERT (Fig. 6cd) where the grounding line has retreated beyond its original PERT position and does not re-advance even after ocean conditions revert to their original state (Fig. 6cd and Fig. 7). This retreat of grounding line beyond its position in PERT leads to reduced buttressing, accelerated ice flow, and dynamic thinning. These locations highlight ice sheets susceptible of possible irreversibility.

C10: Fig 7: For better readability, I recommend to use REF, PERT and REV also in the Figure label - even though the explanations are nice.

R10: DONE

C11: Fig 8c: The VAF seems to be increasing in REF & REF_ext for most of the basins. Is this an expected trend? wouldn't we expect a more constant or even shrinking VAF in REF&REF_ext? Or is this due to the choice of SMB or due to underestimated melt rates?

R11: We thank the reviewer for raising this important observation. This apparent trend is likely due to model biais already in place over the observational period. The exact causes, such as constant viscosity, the lack of subglacial hydrology, fixed ice fronts, or insufficient melt rates at the grounding line, are not yet fully understood yet. This bias does not substantially affect our main conclusions, as we primarily compare PERT and REV to the reference run. We now discuss the model state in REF over the period 2004–2018 (see *R2*). Regarding the evolution of the ice sheet's volume trend over the entire simulation, it remains difficult to assess, as the present-day ice sheet is likely not in equilibrium.

C12: L291ff: How long does Garbe et al. or other studies you mention here enforce the warmer conditions before reverting back? This would be useful information to put your results into perspective. Especially in regards to the thought that your 50yrs choice might be too short. Furthermore, Garbe et al., state very strongly that there is no reversibility in certain areas under certain conditions, right?. Does the difference between Garbe et al. and this study imply that the reversibility depends on the choice of ocean and ice model (and setup)?

R12: In Garbe et al. (2020), the experimental setup is fundamentally different from ours. Their objective was to map the full hysteresis cycle of the Antarctic Ice Sheet by applying millennial-scale temperature perturbations (increases of >10 K at a rate of 10^{-4} K a^{-1}). Mengel & Levermann (2014) used a different approach, applying ocean temperature anomalies of 1.0–2.5 °C for 200–800 years and identifying an "ice plug" threshold of 60–80 mm sea-level equivalent (22,200–29,600 Gt); removal of this ice volume triggers irreversible retreat of the Wilkes Basin. Such long-timescales enable a comprehensive description of even small tipping points but are computationally prohibitive for coupled ocean–ice sheet models.

Regarding reversibility, Garbe et al. (2020) and Mengel & Levermann (2014) do indeed conclude that some regions (e.g., Wilkes and Aurora Subglacial Basins) exhibit irreversible retreat under certain conditions. However, these conclusions depend strongly on both the model and experimental setup. For ocean models, different representations may respond differently to the same perturbation due to biases in high-salinity shelf water properties or polynya activity (e.g., resolution, sea ice modeling, or parameterizations) may break the density barrier between HSSW and CDW in one model and not in the other. For ice sheets, even the same model may show reversible or irreversible behavior depending on whether the grounding line retreats beyond critical topographic features, such as ridges or pinning points.

The paragraph has been modified as follows:

At the basin scale, the integrated trends in GL positions reveal that most basins either re-advance immediately or after a delay following the perturbation. However, some basins, such as the Wilkes Subglacial Basin, are known for their potential instability. *Mengel et al. (2014), using ocean temperature anomalies of 1.0–2.5 °C applied for 200–800 years, demonstrated that the removal of a specific coastal ice volume—acting as a protective "ice plug" of 60–80 mm sea-level equivalent (22,200–29,600Gt) would destabilize the entire Wilkes Basin. Similarly, Garbe et al. (2020), who applied millennial-scale temperature perturbations (increases of >10 K at a rate of 10^{-4} K a^{-1}), show that sectors such as the Wilkes and Aurora*

basins can exhibit irreversible retreat under such strong and sustained forcing. In contrast, our simulations show only a modest ice loss (1,200Gt), far below the 30,000Gt required to remove the protective "ice plug" (Mengel et al., 2014). This discrepancy likely arises from the short duration of our perturbation, which may not have been sufficient to reach the tipping point. Similarly, the Aurora Subglacial Basin, another region prone to irreversible retreat (Garbe et al., 2020), shows no signs of instability in our simulations, further suggesting that longer or stronger perturbations are necessary to cross critical thresholds.

C13: L306-315: You do not mention the choice of ice model resolution: 1km finest resolution is fair for Antarctica due to computation time, but could impact grounding line dynamics and the choice of resolution would define existing pinning points (or omit them). On top of this are the uncertainties in the bedrock data.

R13: Instead of adding these technical details to the discussion we added those points into the ice sheet model description. The new paragraph on the resolution is this one:

The grounding line position is determined using a flotation criterion, and basal friction in partially floating elements is treated using a sub-grid parameterisation (SEP3; Seroussi et al., 2014). The computational mesh is anisotropic, with a horizontal resolution ranging from 1km near the grounding line to 50km in the interior, and remains fixed during the simulations. Such a resolution in the grounding line area, combined with sub-element parameterisation, is know to be sufficient to accurately represent grounding line migrations (Seroussi et al., 2014). Furthermore, this resolution also allows accurate representation of small pinning points, which are critical for ice sheet stability (Favier et al., 2016).

References:

C14: please go through the list. A few are missing doi, a few seem to have a different format or different information supplied

R14: We checked all the references and downloaded the bibtex information provided be the reviews of all the references with missing DOIs.

C15: L399: Remove the three times number-code in the reference: e2022JC018621

R15: DONE

C16: L423ff Garbe double reference? 2020a and 2020b or wrong reference in 2020b?

R16: It was a double reference. DONE

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).

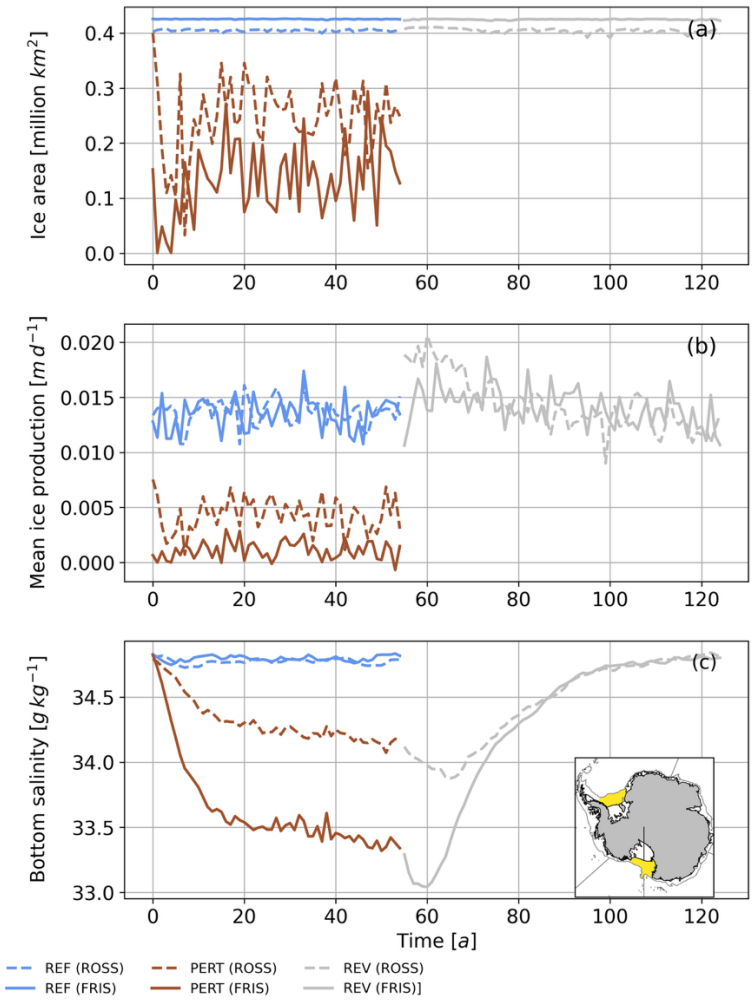


Figure 5. (a) Time series of sea ice area, (b) mean sea ice production, and (c) and bottom salinity on Ross and Filchner-Ronne continental shelf.

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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.

Editor Review Mathiot et al. (2026)

We thank the editor Felicity McCormack for their positive and constructive comments on the updated manuscript. These comments have allowed us to clarify some aspect of the text.

Given the ongoing discussion in the community at the moment about tipping points / instabilities and how we define and refer to them, and some of the points raised by R2, I would request that you carefully review your use of "tipping points" and "instabilities" throughout the text. E.g.:

C2: Introduction, L33-42: It would be good to cite a recent paper that discusses the concept of instabilities, particularly with respect to marine basins: Sergienko et al. (2026). See their Box 1.

R2: We thank the editor for this suggestion. We have revised the Introduction paragraph on MISI to better reflect the current discussion regarding the definition and use of the terms *marine ice sheet instability* and *tipping point*. We now cite Sergienko et al. (2026) and explicitly acknowledge that MISI is used in the literature to describe several related but distinct concepts, including the original configurational instability of Weertman (1974) and Schoof (2007) as well as externally forced grounding-line retreat.

The new paragraph is now:

The ice-sheet tipping behaviour in Antarctica is often discussed in the context of the so-called marine ice sheet instability (MISI). As discussed by Sergienko et al. (2026), this term is used in the literature to describe several related but distinct concepts, some of them purely related to the ice and bedrock configuration (e.g. Weertman, 1974; Schoof, 2007; Gudmundsson et al., 2012), and others involving the forcing by ice-shelf melting and the consequences on ice-shelf thinning, reduced buttressing, and grounding line retreat (e.g. Hill et al., 2023; Reese et al., 2023). In this article, we use "instability" to refer to a forced ice-sheet response that becomes self-sustained (for any reason) for at least several centuries.

While the recent retreat of Antarctic grounding lines may not yet be irreversible or self-sustained (Hill et al., 2023), there is observational evidence of recent ocean and sea ice regime shifts in Antarctica (Abram et al., 2025). Further grounding line retreat could eventually lead to the irreversible collapse of some marine regions of West Antarctica even under present day climate conditions (Reese et al., 2023). A complete collapse of the West Antarctic ice sheet may be committed if global warming reaches 2°C above pre-industrial conditions (Garbe et al., 2020) or if the Amundsen sea warms by more than 1.2°C above present-day conditions (Rosier et al., 2021) or even less (Chandler et al., 2025). However, confidence in the parameterised ice-shelf melt rates under future conditions remains low (Burgard et al., 2022, 2023) *and not representing the ocean feedbacks is equivalent to assuming that there is an unbounded amount of heat available for ice-shelf melting, which is wrong (Sergienko et al., 2026).*

C3: Sections 4.1 and 4.2, e.g. on L260 "... are showing signs of instability". Please clarify whether your simulations can explicitly distinguish between a dynamic instability and a lagged, transient response (i.e. committed retreat / inertia). If it's not possible, I would suggest a revision of how you refer to grounding line retreat in sections 4.1 and 4.2. If it's sustained, unforced grounding line retreat, I would recommend referring to it as such. If you'd prefer to stick with the instability language, then it would be helpful to be clear (perhaps in the introduction) how you define instability. E.g. see Box 1 in Sergienko et al. (2026).

R3: As described in our response to C2, we now explicitly define what we mean by "instability". The two occurrences of "MISI" in Section 4.1 have been revised. The first has been removed, and the second has been replaced by "*self-sustained grounding-line retreat*". Concerning the use of "tipping point", we use this term only in the context of bistability. We reviewed its usage throughout the manuscript and did not identify any instances where it was incorrectly used to refer to an instability.

C4: With the new mention of MICI (R2 comment), also include mention of studies that dispute it as an instability mechanism, e.g. Morligem et al. (2024).

R4: We have slightly revised the text discussing MICI to acknowledge that its relevance remains debated: *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), potentially triggering Marine Ice Cliff Instability (MICI; Pollard et al., 2015), a proposed mechanism whose relevance for Antarctic ice-sheet evolution remains under debate (e.g. Morlighem et al., 2024).*

Sergienko et al., 2026: https://www.nature.com/articles/s41561-026-01941-2
Morlighem et al., 2024: https://www.science.org/doi/full/10.1126/sciadv.ado7794