

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/Amer), 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 ($1,182 \text{ Gt a}^{-1}$) is consistent with observational estimates for Antarctica of $1,325 \pm 235 \text{ Gt a}^{-1}$ for the 2000s (Rignot et al., 2013) and of $965 \pm 265 \text{ Gt a}^{-1}$ 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 ($+1 \text{ Gt a}^{-1}$) 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 deglaciaded 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 were 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 biases 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 known 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 by 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

