In blue: reviewer comments
In black: authors reply

General comments

This paper, by Pierre Mathiot and Nicolas Jourdain, uses a ¼° version of NEMO, with various alterations compared with previous versions of the same model to improve the present-day climatology, forced by surface forcing from a high-end scenario in the late 23rd century. The goal of this study is to understand how ocean conditions change under such extreme forcing and the likely impact on ice shelf melt rate, which is likely to ultimately impact ice sheet mass loss and sea level rise. The model and experiments are thoroughly described and the results are interesting, although the authors correctly note that this is a highly idealised scenario and the model lacks key components of the earth system response (principally the lack of interactive ice sheets and ice shelves) that would be expected to alter the ocean conditions. I have the following suggestions to improve the manuscript:

Authors reply: We thank the reviewer for their positive comments on the overall paper. These in-depth comments have allowed us to clarify many aspects of our manuscript and have prompted us to go further into the description of the mechanisms.

Section 2.1: As noted on line 136, other configurations of NEMO and similar models at ¼° are unrealistic. Therefore, it would be useful to have a summary of the key changes made in this configuration that led to the improved climatology. I understand that the authors are unlikely to know the impact of every change, but it would be useful to have some discussion of the changes that are likely to have made a large improvement to the representation of the present-day conditions.

Authors reply: First, we have tried to better explain the motivation for the main changes in our revised section 2.1 (maximum sea ice fraction, lateral and bottom boundary conditions, increased vertical resolution, iceberg line over Bear Ridge, and tidal velocity in the three-equation melt rate). Second, we already had two sentences in the conclusion highlighting what we consider as the most important changes, so we have not added further comments:

> *'Thanks to a preliminary tuning of the sea ice model parameters and of the lateral and bottom boundary conditions at the northern end of the Antarctic Peninsula, we simulate realistic water masses in the Southern Ocean and on the Antarctic continental shelf. This is important as the performance of previous versions of eORCA025 was not good enough to be used in ocean--ice-sheet simulations (Smith et al. 2021).'*

L52-54: Please justify the reasons for thinning Getz ice shelf: why this ice shelf and not others, and how could this approach be justified? How big a difference did this single change make compared with other tunable parameters in the model?

Authors reply: We have added more explanations in section 2.1:

> *'After preliminary tests, the Getz ice shelf draft was artificially thinned by 200 m (keeping the grounding line unchanged) in order to compensate a longstanding bias in the thermocline depth (previously reported by Mathiot et al., 2017). The later was driving very excessive release of meltwater, which was strongly deteriorating the mean state of the Ross Sea (a connection previously described in Nakayama et al., 2020). More details on the impact of such correction are provided in Section 3.3.'*

Then in section 3.3:

> *'The total melt underneath Getz was strongly overestimated in preliminary simulations, reaching 400-500 Gt yr^{-1} (not shown). By reducing the ice shelf draft of Getz (section 2), we have artificially displaced it into the model cold mixed layer, which gives more realistic melt rates. This empirical correction of the ice shelf draft is*

> *nonetheless slightly too strong because it was done prior to the completion of parameter tuning.'*

Then in section 4.3:

> *'The melt increase is particularly strong for Abbot and Getz ice shelves (Fig. 11) because a large portion of the ice draft is currently located in the cold winter mixed layer (Cochran et al., 2014; Wei et al., 2020) and experiences a shift to much warmer conditions in the perturbed experiment. Given that the position of the thermocline with respect to the ice draft largely drives the transition to a high melting regime, we believe that the ice draft correction applied to Getz has made the transition more realistic.'*

It is also worth noting that the shape of Getz ice shelf is not perfectly known, with uncertainties on the ice draft of approximately 100 m in BedMachine-Antarctica, but a part of the issue is also clearly related to a bias in our thermocline depth.

L62: What is the motivation for (and impact of) applying a no-slip condition around the islands near the Antarctic Peninsula?

Authors reply: The text has been slightly changed to address this comment:

- Changes in Section 2: *'A free-slip lateral boundary condition on momentum is applied with no slip condition applied locally at Bering Strait, in the whole Mediterranean sea, along the West Greenland coast and around the south Shetland, Elephant and south Orkney islands (at the Northern end of the Antarctic Peninsula). This technique is a crude method to take into account the locally complex sub-grid scale bathymetry, and it affects water mass properties as explained in section 3.2.'*
- Changes in section 3.2: *'The north end of the Antarctic Peninsula also exhibits a cold bias in REF. Preliminary analyses during the tuning process suggested that this bias was sensitive to the HSSW properties (worse when HSSW was not dense enough), to the treatment of the bathymetry, and to the lateral slip condition and bottom friction at the tip of Peninsula.'*

L82-83: Please clarify this sentence about the calving pattern: I don't understand what this means, nor its significance, and I expect many readers will also be confused here.

Authors reply: The text has been changed to:

> *'The total calving rate of individual ice shelves is derived from Rignot et al. (2013) who assumed steady ice shelf fronts. As we have no information on the geographical distribution of calving for a given ice shelf, the local calving rate of each ocean cell along the front of an ice shelf is defined randomly at the beginning of the simulation preserving the total amount of calving per ice shelf. The calving rate is kept unchanged throughout the simulation.'*

L95-96: Surface runoff from Antarctica could become regionally important in such a high-end future scenario, which should be noted here.

Authors reply: We agree but we have not added anything as this was already mentioned in the next section:

> *'… and that runoff from ice melting at the surface will remain zero. All these assumptions are unrealistic even for projections to 2100 (Seroussi et al., 2020; Kittel et al., 2021).'*

L96-98: Why is this freshwater flux correction needed? What other errors in the model is this compensating for? Might these issues undermine the realism of the future scenario?

Authors reply: We thank the two reviewers for pointing this out and we have added more details in the revised manuscript:

> *'On top of other freshwater fluxes (precipitation, runoff ...), a common practice in forced ocean models is to use some form of sea surface salinity restoring. This restoring is required because of the missing atmospheric feedbacks on humidity in forced models (for more details see Griffies et al., 2016). To make the model sensitivity analysis more robust, this corrective term was diagnosed from sea surface salinity restoring towards WOA2018 over the period 1999-2018 in a former simulation (the "REALISTIC" simulation described in Burgard et al., 2022) and applied as an additional climatological monthly freshwater flux in all our simulations.'*

L111-112: Although the anomaly method will correct a part of the model biases, I find it hard to believe that model biases will not affect the projected changes, especially under such an extreme scenario. Therefore, it would be useful to briefly summarise the performance of the IPSL-CM6A-LR model in this region, and consider whether there are processes that are poorly represented that may affect the realism of the surface forcing projections.
Authors reply: More details on IPSL-CM6 have been added to the text about the performance of IPSL-CM6A-LR:

> *'IPSL-CM6A-LR is one of the few CMIP6 models extending their scenario-based projections to 2300. In present-day conditions, IPSL-CM6-LR is cold biased by a few degrees at the surface of the Antarctic Ice Sheet (Boucher et al., 2020). On the ocean side, bottom water formation on Antarctic shelves is reasonably well represented as well as the presences of the cold and warm shelves in IPSL-CM6 (Heuzé et al. 2020; Purich et al., 2021). Sea ice extent is within the observational uncertainty in summer and slightly overestimated in winter (Boucher et al., 2020). These elements give confidence that the overall atmospheric forcings of IPSL-CM6-LR can be used to drive an ocean model'.*

With regards to the stationarity of model biases, we already had this sentence, in which we have only expanded the scenarios analyzed by Krinner et al.:

> *'Our method is expected to correct a part of the CMIP model biases that are largely stationary even under strong climate changes (as shown by Krinner et al., 2018, from preindustrial to 4xCO2)'.*

L190-191: Clarify this: are you suggesting that the updated Thwaites ice draft should improve the simulated melt rates? Or that it might have inadvertently caused larger biases?
Authors reply: Thwaites area changed in the recent year. Rignot et al. (2013) used a pre-2012 area (5,499 km^2) compared to the Paolo et al. (2023) who used a more recent area (3,116 km^2). We have reformulated this sentence:

> *'For Thwaites, it should be noticed that we use a recent ice shelf draft in NEMO (Morlighem et al. 2020a, 2020b) with a significantly reduced area compared to the period covered by Rignot et al. (2013), which logically decreases the integrated melt'.*

L191-192: Please clarify this statement. The ice shelf draft was reduced by 200 m to counteract a longstanding bias that was producing excessive melt rates (section 2). Here, it would be useful to repeat that the ice shelf draft was reduced by 200 m. Furthermore, it would be useful to qualify the statement that this correction was too strong: I assume that the melt rates agree better with observations than before the correction was made? Is the implication that future studies using this model could apply a similar but smaller change to the Getz ice shelf draft?
Authors reply: See our response to the previous comment on L52-54 and associated modifications in our manuscript. Future studies should indeed probably apply a slightly smaller correction.

Section 4.1: Could you give some indication of the likely reasons why the gyres intensify and increase in extent? Is this consistent with changes in ocean surface stress curl in the projected future forcing, as suggested by Gomez-Valdivia et al. (2023)? Many readers won't have read that reference, so at least discuss this possibility.

Authors reply: Yes, this is consistent, and we have added:

> *'This is consistent with changes in wind stress curl due to changes in the atmospheric circulation (Fig. 1e) and sea ice loss, as previously reported in projections over the 21st century (Gómez-Valdivia et al., 2023).'.*

L230-231: Can the slow trend really be "explained by slow changes in deep ocean properties at the global scale"? 100 years seems like a short time for such global deep ocean changes to be manifest. Instead, it seems more likely that the deep changes in all these locations are generated by changes in deep water source regions driven by changes in the deep circulation around Antarctica. Perhaps just replacing "global" with "circum-Antarctic" would be better?

Authors reply: Kissel et al. (2008) indeed suggested a millennia timescale for NADW to propagate from Arctic to glacial Southern Ocean based on paleo evidence, and Armour et al. (2016) suggested a multi centennial time scale to explain the current delay in Southern Ocean warming. In the CMIP5 outputs, however, Sallée et al. (2013) showed a warming of CDWs in a projection until 2100. They speculated that this was more a result of vertical mixing in the Southern Ocean than a change of the CDW at their source. So, we agree, circum-Antarctic is more adapted for this simulation and we have also added the reference to Sallée et al. (2013).

Armour, K., Marshall, J., Scott, J. *et al.* Southern Ocean warming delayed by circumpolar upwelling and equatorward transport. *Nature Geosci* **9**, 549–554 (2016). https://doi-org.insu.bib.cnrs.fr/10.1038/ngeo2731

Kissel, C., Laj, C., Piotrowski, A. M., Goldstein, S. L., and Hemming, S. R. (2008), Millennial-scale propagation of Atlantic deep waters to the glacial Southern Ocean, *Paleoceanography*, 23, PA2102, doi:10.1029/2008PA001624.

L237-242: It is understandable that you can't diagnose all the mechanisms, but this discussion still feels unsatisfying and like a list of possible mechanisms. Several of these mechanisms are very region-dependent, so it would be good to split this paragraph into coherent groups of regions (as done in the final paragraph of the conclusions). The relatively uniform warming might help to diagnose the most important mechanisms. For example, the currents along the shelf break are likely to be very different, and these currents are strongly linked to the supply or blocking of CDW onto the continental shelf. Figure 8 implies that these currents have changed, but it would be useful to plot the differences. It is not clear to me that the removal of sea ice and the subsequent freshening down to 400 m and deeper would lead to warming except in regions of HSSW production. Another mechanism to consider is whether the increased melt rates directly increase the overturning circulation on the continental shelf and thus help to bring more warm water onto the shelf?

Authors reply: This comment has pushed us to propose a deeper description of the mechanisms at play in the perturbed simulation. We have added a new figure (Fig. 10) that we use to explain how dense and warm shelves both end up as "warm–fresh shelf" with a specific current system at the shelf break (see modified section 4.2).

L249-251: What reasons might explain this difference? Is this just an area of model uncertainty? Similar question for L 274-278

Authors reply: Differences in the model set up may explain these different sensitivities. A paragraph has been added:

> '*Two aspects may explain these different sensitivities. First, we simulate strong increase in melt rates near the ice shelf front because of the disappearance of the cold surface layer in*

> *our simulations. In the two other studies, the surface layer is still cold and the presence of an interactive ice sheet allows the ice shelf to thin and thereby to partly remain in this cold layer. Second, our parameterisation of tide-induced mixing in the three-equation system (Jourdain et al., 2019) may have a significant effect on the Ross and Ronne-Filchner melt rates. While this parameterisation has a weak effect in present-day conditions because all the available heat is consumed anyway (Hutchinson et al., 2023), the abundance of warm water in the future may enhance its role in the largest ice shelf cavities.*'

L261-262: Presumably the increase in melt rate at Getz is over-estimated due to the artificially-thinned ice shelf draft?
Authors reply: See our response to the previous comment on L52-54 and associated modifications in our manuscript. Wei et al. (2020) mentioned that a large portion of Getz is above the thermocline with low melt rates because within the cold surface mixed layer. Because of the thermocline bias, we thinned Getz to reproduce this. So, with our correction, a large part of Getz is somewhat realistic because it is above the model thermocline. Without this depth correction, the change in PERT for Getz could have been much weaker because our modelled Getz in REF without correction would have been warm already.

L295: While this is a very good summary overall, it would be useful to at least speculate on how these results will impact the retreat of the ice sheats and how this might in turn influence the ocean circulation.
Authors reply: We mention the importance of the ice sheet feedback in the conclusion. It is very difficult to compare our melt rates to other ice sheet modelling studies, first because very few provide their basal melt rates in 2300, then because the diminution of the ice shelf area and thickness in ice sheet models alter the integrated values. For example, the 15,700 Gt yr^{-1} reached in our perturbed experiment in conditions of 2300 correspond to the upper end (95th percentile) of the ice shelf basal mass loss in 2200 in Coulon et al. (2023, under review). But their 2200 estimate is for altered ice shelves, and it decreases after 2200 due to the increasing number of collapsed ice shelves. For this reason, we prefer not to speculate on the details of the ice sheet response and its feedback to the ocean.

Coulon, V., Klose, A. K., Kittel, C., Edwards, T., Turner, F., Winkelmann, R. and Pattyn, F. (2023). Disentangling the drivers of future Antarctic ice loss with a historically-calibrated ice-sheet model. *EGUsphere*, 2023, 1-42.

Typos etc:

L35, L154, L197, L234: Check parentheses around citations. In some places they should be added, in others they should be removed.
Authors reply: DONE

L89: “equation” should be plural (equations)
Authors reply: DONE

L163: Fasten -> fastened?
Authors reply: DONE

L176: “to the exception of” -> “with the exception of”
Authors reply: DONE