Comments on "Evaluating an accelerated forcing approach for improving computational efficiency in coupled ice sheet-ocean modelling" by Qin Zhou and colleagues.

This study proposes and evaluates a method to accelerate ocean–ice-sheet coupled simulations by considering that ocean simulations represent longer time periods than the mode time and by providing accelerated changes in ice geometry to the ocean model. Computational cost is a strong limitation of ocean–ice-sheet coupled models for sea level projections, so it is an important investigation. However, I am not convinced that "this approach could be applicable in modelling studies related to Antarctica's contribution to sea level rise projections" for the reasons below. This is a very important aspect that should be clarified before modelling groups start implementing this approach.

Major comments

I have two important concerns with the applicability to real world simulations, which should be discussed and probably reflected in the abstract:

1- Numerous studies have highlighted the significant impact of ice-shelf and iceberg meltwater on the ocean stratification, with important consequences for the evolution of sea ice (Bintanja et al, 2013; Swart and Fyfe 2013; Merino et al., 2018), Antarctic bottom water formation (Li et al., 2023), ocean currents around Antarctica (Moorman et al., 2020) and global climate (Bronselaer et al., 2018; Purich and England, 2023). If a global ocean model representing ice-shelf cavities is run with the accelerated approach over something like a (real) century, the total freshwater flux into the Southern Ocean won't be the same as in the regular simulation, which may significantly affect the climate system. Similarly, in some coupled ocean-ice sheet models like in Smith et al. (2021), the ice-sheet model sends its calving flux to the ocean model; how could this work with the accelerated approach? I guess that all these fluxes could be multiplied by alpha, but this would change the ocean dynamics. I am also unsure how it would work with an atmospheric forcing (which is absent from the idealised configurations presented here).

2- This work evaluates the accelerated forcing approach with two periods of variability: 0.6 year and 30 years (in real years). It is clearly shown that the accelerated method does not well capture the changes in response to the 30-year forcing (Fig.11). How about periods of 2-7 years that correspond both to ENSO (which significantly influences regions like the Amundsen Sea) and is closer to the residence time? Isn't it an important issue that this range is poorly represented by the accelerated method.

Minor comments and edits:

- L. 22: this is not only a carbon emission scenario, there are other anthropogenic emissions.

- L. 24: a better or complementary reference on the uncertainty is Seroussi et al. (2023).

- L. 30: "local" (instead of "regional") would be more in line with the results cited here (the increase is relatively small at the scale of an ice shelf).

- L. 40: “primarily in testing phases or for sensitivity studies (Muntjewerf et al., 2021)” is not so relevant for UKESM which has been used for scenario-based projections by Siahaan et al. (2022) even if there are important model biases. Furthermore, I don’t understand the reference to Muntjewerf’s paper which is about the Greenland ice sheet.

- L. 57: replace “Specifically” with something like “In this case” or “Under this assumption”.

- L. 59-62: the formulations $\overline{\dot{z}_d(t)}$ and $\overline{\dot{z}_d(t/\alpha)}$ are not clear to me as the bar indicates a time average. Wouldn’t $\overline{\dot{z}_d}^T$ and $\overline{\dot{z}_d}^{T/\alpha}$ be clearer?

- L. 66-84: at this stage, the reader does not know that you are using the ISOMIP+/MISOMIP1 configurations, so “boundary conditions” may refer to the surface boundary conditions (especially for a global ocean model) as well as the ocean lateral boundary conditions. Similarly, “far field” is not so clear at this stage.

- L. 99 & L. 104: these equations are not so clear to me. Why not using two variables for the model time (t_M) and the representated time (t_R).

- Table 4: I am not sure that averaging the barotropic stream function is the most accurate way to calculate the residence time because this function is defined in a relative way (only its gradients are physical). Taking the maximum minus the minimum seems more relevant. I am also wondering whether the relevant time in the ISOMIP+ case is the residence time in the entire rectangular domain.

- L. 216: correct “Notably,Although”.

- L. 219: another very ood reference for this is Jenkins et al. (2018).

- Fig. 6 is interesting. Do the authors have an explanation for the weaker melt at the frequency of the barotropic circulation? On the left of the plot, the ocean temperature does not have time to adjust in the water entering the cavity ends up at a temperature of $0.5(T_C+T_W)$. Towards the right of the plot (and beyond), the temperatures tend to follow the oscillatory forcing (equation 7 of the manuscript). If you assume a melt dependency to the quadratic thermal forcing and average the melt rate over time, you can probably explain the left-right asymmetry. My guess for the low central value is that the melt-induced circulation starts to increase in response to thermal forcing just when the forcing switches back to cold condition, which quickly cools the cavity, while the return to a warm phase is slower due to the low melt-induced circulation in cold conditions. In this case, the mean temperature in the cavity is closer to T_C, so melting is at its weakest value.

- Fig. 10, panel a: explain PFast1-mm in the caption.

- Fig. 10, panel b: the yellow red curves seem to show the relative difference (in %), not the absolute difference as indicated in the caption. Showing ΔV for the three experiments as in Fig. 11 (not the relative difference) would probably be easier to read. I also don’t understand the values: why don’t PFast3 and PFast10 start with 0% difference at month zero.

- L. 392: “Here exists a few locations” -> exist ?

- L. 401-404: I find this sentence hard to follow.

- L. 476: I do see reasons, see my main comments.

L. 455-468: Ok but the real ocean has a lot of variability associated with periods between 1 year and 30 years (e.g., El Niño Southern Oscillation; Holland et al., 2019). For this reason, Fig. 11 is quite concerning for an application to a real ocean.

The method should be compared to Lofverstrom et al. (2020) who present an approach for the atmosphere forcing of Greenland, but has some similarities with the method presented here.

References

Jenkins, A., Shoosmith, D., Dutrieux, P., Jacobs, S., Kim, T. W., Lee, S. H. and others (2018). West Antarctic Ice Sheet retreat in the Amundsen Sea driven by decadal oceanic variability. *Nature Geoscience*, *11*(10), 733-738.

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