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Interactive comment on "Simulating Marine Isotope Stage 7 with a coupled climate-ice sheet model" *by* Dipayan Choudhury et al.

Anonymous Referee #1

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General comments

Choudhury et al. present simulations of the glacial inception MIS7 (240-170 ka) using a climate model coupled to an ice sheet model (North and South). They discuss the simulated global sea level evolution during MIS7 and in particular the two step glaciation. While coupled ice sheet – climate simulations spanning several thousand years are still scarce, the paper shows very interesting results. I have listed below a few comments / suggestions that could be considered before resubmission.

Specific comments

1- Methods

- I found the coupling strategy not very clear. If I understand correctly, the surface mass

balance model is a submodel of PSUISM. From Fig. 2 it seems that PSUISM only takes SAT, solar radiation, precipitation and Tocean. How these fields are downscaled onto the higher resolution ice sheet model grid? Then, what is the "solar radiation" (not explained in Sec. 2.3)? I find it a bit strange that surface albedo is not part of the fields that are given to PSUISM. Eq.1 and Eq.2 suggest that PSUISM compute its own albedo from rs and rl. Where do these rs and rl come from? Also, you do not explain how you compute the sub-shelf melt from Tocean. Is this a simple scaling after a bilinear spatial interpolation? What about ice shelves developing where there is no oceanic grid points (e.g. Kara and Barents seas)? I am sure that we can find all this information in the different papers that use a similar LOVECLIP framework but it would facilitate the reading to have a synthetic and integrated view in this paper as well.

- On the coupling strategy again, but from the ice sheet to the climate model this time. I understand the use of the acceleration factor. However, I do not understand how you can ensure water conservation between the ice sheet and the rest of the climate system with this acceleration factor. It seems to me that when we use an acceleration factor we can only conserve the volume or the flux, but not the two quantities at the same time (already reported by Heinemann et al., 2014). For example, let's assume that the ice sheet model runs 10 years for each year computed by the rest of the climate model. Let's say that the ice sheet model produces 10 km3 of volume loss integrated over the 10 years (1km3/yr). What do you give to the ocean model? 1 km3 or 10 km3 ? The second option conserves the volume but the fluxes that arrive to the ocean are overestimated which can ultimately result in unrealistic oceanic evolution. In any case, I think it needs a bit more of description in the paper. Also, what is the routing strategy to transfer the ice loss to the ocean? Do you use the atmospheric model runoff model? In case of an ice sheet inception, do you have a negative flux at the surface of the ocean that increases the salinity? Is this spatially resolved?

- Have you run a pre-industrial control run (for example a 10-kyr long simulation with the LGM bathymetry under pre-industrial orbital and GHG forcing)? Such simulation could be nice to validate your model setup, or at least to quantify the bias in the coupled model trajectory.

- By starting at 240 ka, you start towards the end of a deglaciation. Do you think that the long-term climate trajectory has an impact on your results? For example do you think that a restart from a full glacial state at 250 ka would result in a similar global ice volume evolution across MIS7?

- You use a simple bias correction for surface temperature and precipitation. Why not use a similar technique for the radiative inputs of the ice sheet model and for the oceanic temperature as well?

- The hydrofracturing and cliff collapse parametrisation embedded in PSUISM is controversial (e.g. Edwards et al., 2019). Do you think that you would end up with different ice volume trajectories using a model that does not account for the MICI?

2- Results

- From my understanding of your model results, the respective role of orbital configuration with respect to CO_2 is pretty much linked to the choice of the alpha / m combination. From Fig. 3 it seems that you cannot guarantee the uniqueness of your calibrated alpha / m (higher m but lower alpha might works equally well than lower m and higher alpha). As a result how robust is your conclusion on the respective role of orbital versus CO_2?

- You justify the use of the alpha parameter to correct for the lack of sensitivity of the climate model. There are alternative way to modify the climate sensitivity in the model. For example Loutre et al. (2011) modify a set of model parameters in order to have a similar pre-industrial climate but different sensitivity to the change in CO_2. With your approach you might give too much importance to the radiative effect of CO_2.

- The Eurasian ice sheet in your simulation does not grow at all which seems in contradiction with palaeo data (e.g. Batchelor et al., 2019). Even in the run-away glaciation

presented in Fig. S6, it seems that there is only very little ice developing in Eurasia. I know that it is not trivial to simulate satisfactorily the Eurasian ice sheet inception, particularly the Kara-Barents sector. Do you have any idea on how to improve on this? Do you think that it is an oceanic problem, e.g. too warm waters in the Kara and Barents seas? Or an atmospheric problem, e.g. shift in storm tracks?

- For the two step inception (Sec. 3.4), you claim that CO_2 explains the later full inception. If the insolation signal is indeed similar for both periods, it shows nonetheless a greater amplitude in MIS 7e-7d-7c with respect to MIS 7a-6e-6d. The insolation maxima is thus slightly smaller for the later period. This slight difference in the insolation maxima could explain the ice retreat in the older inception, with the full glaciation being the result of an insolation threshold? Of course CO_2 should help but it is hard to distinguish the role of the two forcings (especially also because the results depend on the value of the chosen alpha parameter).

- I wonder how robust are the atmospheric circulation changes discussed in Sec. 3.6. For example, don't you think that the atmospheric circulation might be potentially largely affected by the presence of an ice sheet in Eurasia?

3- Miscellaneous

- l64-78 You could refer to your figure 1 somewhere around here to facilitate the reading.

- towards l194 and Fig. S1 From my understanding of your methods your reference climate model uses a last glacial maximum bathymetry (since bathymetry is fixed to a LGM value). Would it have been appropriate to compute the bias for the modern climate using a LGM bathymetry? More generally it would be very interesting to see the effect of the sole bathymetry on the simulated climate (under PI forcing for example).

- l252 The Filchner-Ronne ice shelf is advancing but there is almost no change for the Ross ice shelf. Any idea why?

- l306-312 You show in the figure the mean SMB/ablation/accumulation over the ice sheet. In doing the spatial average, we can end up with very similar values for very different ice sheet extent. In addition, we cannot quantify the importance of the different processes to explain the total mass change (for example the sub-shelf melt is much more negative than SMB but it may concern only a very small fraction of the ice sheet). Instead, it could be interesting to have the integrated value over the ice sheet but not divided by the extent (Gt or km3 per year), to have a better idea of the respective role of SMB and sub-shelf melting to explain the total ice volume change.

- l319-322 The spike in SMB is very impressive but I am even more surprised by the spike in shelf melting. I think it could be useful to show maps of surface mass balance and sub-shelf melting for temporal snapshots in the vicinity of this event (circa 210 ka?). In doing so, it will illustrate the saddle-collapse as well as the spike in sub-shelf melt. Again, maybe the spike in sub-shelf melt is affecting a tiny area and is not representative of the total mass loss (previous comment)?

- l345-346 Do you know the reason for this? It would be nice to understand this circulation change (which produce the spike in sub-shelf melt?). It happens during the deglaciation and thus, in the meantime, you probably have a lot of freshwater flux that is discharged to the ocean. Does the two processes (increase in sub-shelf melt and freshwater flux) are related in some way? If yes, how realistic are your freshwater fluxes (see previous comment on the acceleration factor).

- Fig. 2 You could also mention on the figure how the interpolation / downscaling to the different grids is done. Also, by “landmask” you mean “ice mask” (albedo)?

Technical comments

- Fig. 4 The grounding line is hardly distinguishable.

- Fig. 4 (and Fig. S2, S3, S4 and S5) I find the maps of mixed information thickness / ice velocity hard to read. Maybe using the contours for thickness and the solid colours for velocity would look nicer?

- Fig. 7 I am sure that you can use a better colour bar for panel f to i. At least it can be white where there are small changes instead of yellow.

- Fig. 7 l675 purple contours are for which run (blue or black in panel a)?

- Fig. S1 Panel b: since a multiplicative correction of 1 means no correction maybe it could have been better to have a neutral colour such as white around the value of 1.

References

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