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Interactive comment on "Coupling framework (1.0) for the ice sheet model PISM (1.1.1) and the ocean model MOM5 (5.1.0) via the ice-shelf cavity module PICO" *by* Moritz Kreuzer et al.

Anonymous Referee #1

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"Coupling framework (1.0) for the ice sheet model PISM (1.1.1) and the ocean model MOM5 (5.1.0) via the ice-shelf cavity module PICO" by Kreuzer et al. describes the software implementation of an ice-sheet/ocean coupler, designed for ocean models that do not resolve ice shelf cavities. This manuscript details the algorithm by which data is exchanged between the two models on a basin-averaged basis, while ensuring conservation and a reasonable computational overhead. The scientific validation of the coupled system is left for another paper, so the realism of the model is not discussed.

General comments:

Coupling ocean and ice-sheet models via PICO is a worthwhile idea, as it would fill a

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gap in coupled modelling. If successful, it would enable CMIP-style models to include evolving ice sheets without having to explicitly resolve ice shelf cavities, which would open a lot of doors for long-timescale simulations. The coupling algorithm described here is well explained and logically designed. I have a few questions about the coupler which should be addressed for clarity (see my specific comments below).

However, I am concerned at how this paper does not show whether the coupled system can actually produce reasonable results, which are acceptably realistic and stable for the present-day climate. I find it acceptable to separate the software description from the model validation, but only if the model validation is submitted as a companion paper at the same time. Without this assurance, I worry that the authors have not yet tested the realism of the coupled system.

What if there are insurmountable challenges which make the premise of the coupling unusable? For example, Figure 5 suggests that the ocean temperatures being passed to PICO are far too warm, with no continental shelf temperatures below about 0.25C. In reality, we know that large regions of Antarctica have inflow into ice shelf cavities consistently around -1.9C, as a result of sea ice formation. Effectively, coupling with this MOM configuration will turn all ice shelf cavities into warm cavities like the Amundsen Sea, but we know that many/most cavities are cold cavities. Surely this would lead to unacceptably high basal melt rates and grounding line retreat.

What if these challenges can only be overcome by substantially changing the coupling design, at which point this paper becomes out of date? While I applaud the authors for their worthwhile efforts in this model development, I worry it is premature to publish before we know whether this approach will work. It might be best to save this paper up until the scientific validation has been completed and written up. At that point, it will be a very nice submission to GMD.

My other major comment is regarding the introduction. I feel that a stronger and clearer case could be made for why this coupling advance is needed. I would love to see a

revised and reorganised introduction, which more clearly lists the major categories of models which already exist (global coupled models with fixed ice sheets; standalone ice sheet models with highly simplified ocean forcing; high-resolution ocean models with static or dynamic ice shelf cavities) and why they are not suitable for ice-sheet/ocean coupling on millennial timescales. This would make it more clear to the non-expert reader why PICO coupling is a major advance.

Specific comments:

Title: Is it necessary to include the version numbers for PISM and MOM5, as well as the coupler? This detracts from readability, and it seems like the coupling should be more or less independent of the specific model versions. Also, the title should specify that the coupling is specific to the Antarctic Ice Sheet.

Abstract, line 8: “Earth system” should be changed to “ocean”, since this manuscript doesn’t discuss coupling between the ice sheet and other climate model components.

Line 24: The discussion of ocean forcing in paleoclimate should be expanded, as many of the readers may be unfamiliar with this.

Line 26: “prescribed ice-sheet configurations” is not clear - does this mean there is a fixed ice surface topography and no simulation of ice dynamics?

Line 30: I suggest changing “circulation in ice-shelf cavities” to “ice pump”, as PICO doesn’t explicitly simulate cavity circulation either.

Line 38: Note that Scenario A1B is from CMIP3, not CMIP5/6.

Line 40: It is not clear that “ice-sheet/ocean interactions” here refers to fully coupled ice-sheet/ocean models including ice dynamics. There are many ocean models which simulate ice shelf thermodynamics alone, with static cavities, which is an important distinction from the types of models I think you are referring to.

Line 46: Donat-Magnin et al. (2017) is not a coupled ice sheet-ocean model, but rather

has static cavities as above, and prescribed a different grounding line for a sensitivity experiment. I would suggest just taking this reference out.

Line 60: Surely it is only 2-dimensional ice sheet models, with SSA or similar? I equate 3-dimensional with full Stokes, which I don't think PICO has been coupled to.

Line 60: Change "approximates" to "parameterises"

Line 100: I suspect "poles" and "equator" are the wrong way round - usually meridional resolution is finer at the poles, to follow the convergence of zonal resolution. Coarser meridional resolution at the poles, as the text says, would lead to a strange cell aspect ratio.

Line 101: Is p* the same as z* (which is a more common vernacular for ocean modellers)?

Line 102: What is the vertical resolution of the thickest layers?

Line 113: The text describes the models as run in alternating order. Is it possible to run them simultaneously on different processors? This would save on walltime, although the CPU hours wouldn't change.

Line 114: Figure 4 suggests that MOM is the first model to run. Is this always the case, or can PISM start the chain?

Figure 4: It might be helpful to align the time axes. It's difficult to tell which simulation segments are supposed to coincide with which.

Line 147: Why not extrapolate the values from the centres to the cell boundaries where needed, to minimise the regions of missing data? Would this change anything, or is it implicit in your next point about filling the missing regions?

Line 148: It is unclear whether the missing values are filled as a single block of adjacent missing values, or cell by cell. This would lead to slightly different behaviour in the averaging routine.

Line 154: What happens to the vertical interpolation if the ocean bathymetry is shallower than the continental shelf depth as seen by PISM?

Line 163: If I understand correctly, every coastal ocean cell in a given basin receives the same mass of freshwater from PISM (and similarly for energy). Why is this not scaled by area, so that larger ocean cells receive more freshwater? This would be equivalent to distributing the total mass flux evenly over the coastline in the given region, regardless of the details of the ocean grid.

Figure 7: It's confusing to compare the different units in (a) and (b). Over what time interval are the fluxes in (a) integrated?

Line 171: What happens if the energy flux causes supercooling of the ocean waters? Is this supercooling automatically removed by the sea ice model?

Line 175: Doesn't ignoring these heat fluxes mean the coupled model does not conserve energy? I understand the argument later that the coupler itself conserves energy, during the regridding process, but it appears false to claim that the entire system is conservative (as is implied in line 317).

Line 188: Explain why the coupling time step is an important parameter. What do you expect would happen if it were too long or too short?

Line 200: Clarify that MOM5 runtimes are slightly longer due to a greater fraction of time spent initialising, as with PISM (I assume this is the case).

Line 222: Does this statement mean that MOM5 does not conserve mass? That is concerning, and should be explained further and referenced.

Line 250: As the Galton-Fenzi reference only refers to an EGU presentation, a better reference for online coupling would be Jordan et al. 2018 (doi:10.1002/2017JC013251) which is already published in JGR.

Line 257: I'm not sure it's essential for online coupling to have the same timestep for ice

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and ocean, as ocean models often subcycle different timesteps for different processes (eg barotropic vs baroclinic modes).

Line 273: The ocean mixed layer will rarely extend as deep as the ice shelf front. Ice shelf meltwater entering the ocean at depth would therefore destabilise the water column and deepen the mixed layer, whereas applying this flux at the surface would have the opposite effect. I appreciate that applying meltwater at depth is not trivial in MOM5, but more attention should be given to the possible negative impacts of this design choice on the ocean simulation.

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