

Interactive comment on “A fully coupled Arctic sea ice-ocean-atmosphere model (ArcIOAM v1.0) based on C-Coupler2: model description and preliminary results” by Shihe Ren et al.

Anonymous Referee #1

Received and published: 1 August 2020

This manuscript describes a coupled via C-Coupler2 Arctic ocean sea ice configuration of MITgcm and Polar WRF atmosphere. The model is intended for high quality Arctic sea ice seasonal predictions. There is large demand for high quality regional climate models of the Arctic basin and such activity must be strongly appreciated. In the manuscript the setup has been validated for year 2012 because of a strong storm formed off the coast of Alaska on 5 August 2012. The role of sea ice-ocean-atmosphere interaction has been addressed. Although the authors demonstrate good modelling skills, good knowledge of the Arctic Ocean system and impressive level of model validation the paper in its present form failed to convince me that the new climate model is ready to use and that is better than any existing global climate model.

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My main criticism is that (1) the performance of the model on different HPC systems, regarding scalability and the costs of all individual components is not addressed, (2) the presented configuration is not properly tuned. I encourage this paper for resubmission after these weaknesses have been fixed.

In the model description, there need to be discussion on why fields and not fluxes are coupled. Do the authors guarantee the same bulk formulas are employed on the atmospheric and ocean sides? Which difference is expected if the fluxes are coupled? I guess that COSMO-CLM/NEMO group has some experience with it although not with the Arctic region. I find this aspect is more important than describing the computation of the corner geographic information for MITgcm. The latter piece I would even omit due to its simplicity. A following chapter after the model description, which gives more information about the model scalability and cost is required.

I believe that C-Coupler is a good tool to use but the statement that a model produces bitwise identical results with a different coupler means that the coupler just works. Is it better in terms of performance? Which interpolation option do you use (question is more relevant for the wind stress)? As an illustration it would be good to see the curl of the wind stress on the atmospheric and oceanic meshes (instead of Fig.3).

In the OCNDYN run is it just setting alpha to 1.0? I believe that one year of coupled simulation is too short to validate the model. Either ensemble of runs or a longer simulation is desired here. I don't want to force the authors to do much of additional work but the cheapest way would be to add the stand-alone MITgcm run for the comparison here. The spin up with JRA55 has been already computed.

Section 4.1, which introduces Fig. 5 illustrates that the model has not been tuned properly yet. Although the authors (line 254) claim that OCECPL is closer to observations, which might be true, but I see that both runs failed. Here it is important to give (at least visually) the measure of the error. A stand-alone MITgcm run, hence, would be a good choice. Provided the high skill of validation made in the following chapters I assume

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that the model has to be better tuned first.

Minor things: line 181: "...without any data assimilation...". There was nothing said about new model is within an assimilation framework before or after. Why to mention this? line 182: "...the coupled model free simulations..." what do you mean? line 188: indeed, nothing about atmosphere. Patterns of SLP although would be of interest. line 230: too speculative. did you do another run with different albedo? line 290: I would elaborate more on this if possible. line 366: not really or show that it is better than in other (global) models line 372: is it due to the albedo feedback? In OCECPL it is computed on the MITgcm side. What happens in OCEDYN? Again, figures 6c, 7 would have more value if the model is tuned. In the present form the model is not ready for this validation.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-95>, 2020.