

Interactive comment on “An isopycnic ocean carbon cycle model” by K. M. Assmann et al.

Anonymous Referee #3

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This submission and the earlier Tjiputra et al submission are companion papers. This submission elaborates in greater detail the ocean component, both physical and biogeochemical, of the earth system model described in Tjiputra. A major selling point here is the use of an isopycnal model based on MICOM. In general I am favorable impressed and would recommend publication if the following points can be addressed adequately in revision.

Perhaps the most significant point to be made is that the calibration and assessment of biogeochemical ocean model seem incomplete without simulating the two transient tracers that have come to prove so useful: CFCs and carbon-14. The omission of these two tracers from the Assmann submission is glaring. These two have in some sense become metrics to assess ocean carbon models, so any revision really should include new simulations of these tracers.

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CFCs are short time transient tracers, but C14 requires long spin ups (thousands of years). It would be useful for the community to know how long (clock time) such a simulation would take with this new Bergen model.

Here are other points that I would like the authors to address in their revision:

- 1) P. 1025, line 7: deep ocean circulation does not have time scale of 1000-2000 years. It is shorter and centennial. Assmann should actually read the cited reference.
- 2) P. 1029, line 10: I noted this in the Tjiputra submission as well, but there does not seem to be adequate justification given for referencing potential density to 2000 db. It seems important to get it right the near surface, where dynamics is more variable, than the deep, where things tend to be quieter. What is gained and what is sacrificed by changing the reference from 0 db in MICOM to 2000 db? If AAIW becomes so cold and indeed reaches 3000 m (p 1038, line 18) because of 2000 db referencing, I do not see the benefit of changing the reference point.
- 3) P. 1033, line 3: again I noted this in Tjiputra submission, but should not silica be linked to P, N, and Fe? Is it possible for diatom to be produced in the model where there may be insufficient nutrients? Could this be contributing to the overestimated POC production in the Southern Ocean, for which the authors have looked into IRON, ABS, and DIAPYC (section 5)?
- 4) P. 1034, line 5: does export production include advection of DOM (in addition to POC sinking)?
- 5) P. 1040, line 18: it is noted that temperature and oxygen are the best simulated tracers. Is it because the temperature is done well, and therefore the gas saturation is about right?
- 6) P. 1040, line 21: why is sea ice too large? Have the authors tried to improve this?
- 7) P. 1042, line 2: carbon uptake is 0.05 Gt/yr; is this because the model is not quite at steady state? If the authors do a long spin up for C14, I would think that the carbon

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uptake would really approach 0 Gt/yr.

8) P. 1045, line 14: where do these stoichiometric ratios come from? They are a bit different than Anderson and Sarmiento for example.

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