

Interactive comment on “Bergen earth system model (BCM-C): model description and regional climate-carbon cycle feedbacks assessment” by J. F. Tjiputra et al.

Anonymous Referee #3

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This submission describes an earth system model that comprises a dynamical climate model (atmosphere, ocean, sea ice) and a biogeochemical model (ocean and land). The submission is appropriate for GMD. Overall the model is described fairly well, although I list below a number of places where the authors should clarify and make improvements. Assuming that these are addressed in a reasonable manner in revision, I would support its publication.

One big-picture question I have is perhaps a bit philosophical and has to do with the purpose of this model. Given the plethora of earth system models available today, what do the authors want to do with this one? Are there certain scenarios for which

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the Bergen model would be better than the others? The Bergen model has reasonably high vertical resolution (over 30 in both atmosphere and ocean) and good horizontal resolution, but it's not quite "high" resolution in today's standard nor is it as coarse and fast as the intermediate complexity model. What's the positioning of the Bergen model? Or does Bergen just want to have a model of its own?

Here is a list of more significant issues followed by minor issues:

1) p 849, line 11: potential density is referenced to 2000 db instead of 0 db in the original MICOM. What is being sacrificed here? Clearly most of the dynamical action occurs near surface, so the original scheme makes sense to me. My question relates to the big picture question – what's the purpose of this change?

2) p 851, line 19: it seems like diatom production is a function of Si only and unrelated to P, N, Fe. Doesn't Si need to be linked to the other nutrients by some stoichiometry? Otherwise, you could end up having diatom production (where there is sufficient Si) even in the absence of P, N, or Fe. Or does this not happen? If so, why?

3) p 851, line 23: how is N fixed?

4) p 853, line 12: explain how the mixed layer depth is determined.

5) p 853, line 25: it seems inadequate to update CO₂ only once a year. Is the model not suitable for seasonality studies then? Given up so early and unnecessarily? Explain.

6) p 856, line 7: describes Figure 1. The match with observation is very good indeed for the annually averaged fields. How about for the different seasons? Is the model appropriate for seasonality studies?

7) Various places (p 858, 865, 866): I am quite unconvinced that the solubility signal of climate change (COU-UNC) with respect to ocean CO₂ uptake is so small. For example, that a 3 deg C warming in equatorial Pacific leads to negligible change in solubility (p 866, line 13) is strange. Even though the authors correctly point out that the sensitivity of CO₂ solubility to temperature becomes lower with higher temperatures,

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there is still sensitivity for CO₂ (unlike some other gases). I am unconvinced in part because Figure 10 can't all the same unit: solubility is not per unit time, so Figure 10a is incorrect. (Actually all panels of Figure 10 need to be fixed) The authors should show a simple back-of-the-envelope calculation of what the CO₂ solubility is for average low latitude surface water (use mean DIC, ALK, temp, sal), then calculate the same solubility with only temperature changed by 3 deg. It should NOT be negligible. It is not clear to me how the authors isolated the solubility (and other) signal using equations 5-7. Do they represent separate model runs or postprocessing? If you want to truly isolate the solubility signal, you have basically use the SST mask from COU onto UNC with respect to gas exchange only; this would ensure that everything else (biology, circulation) remain the same. And you would need to do a prescribed CO₂ run instead of CO₂ emissions run, because $\Delta p\text{CO}_2$ would obviously be different in emissions run if uptake is different.

8) section 4.3 and Figure 6: the temperature dependent carbon uptake by land is entirely clear. For example, in Fig 6 (d? not labeled), the dark blue line (S10-S30) has a peak between 2-3 deg warming. Why is the change not monotonic? Is the temperature dependence of production vs. respiration changing? Also, p 861, line 5, why is carbon uptake given by the difference between NPP and respiration? NPP should already include respiration.

9) p 864, line 18: enhanced carbon uptake is attributed to sea ice retreat and the strong vertical mixing in the newly opened waters. What is happening to biology (POC, CaCO₃ flux) in and winds (piston velocity) over those waters?

10) p 863, line 7-8: it is noted that carbon source regions have switched to carbon sink regions and that equatorial Pacific absorbed 4 kg C/m². These statements are confusing natural carbon, anthropogenic carbon, and total carbon. Remember that anthropogenic carbon flux is pretty much always into to ocean everywhere by the way book keeping works. With regard to the equatorial Pacific, I think the authors want to say that the anthropogenic carbon flux is into the ocean but total carbon flux is still out.

Be very careful with making distinction between natural and anthropogenic carbon.

Minor comments:

- a) Abstract, line 16: replace “improve” with “enhance”
- b) Abstract and elsewhere: use “retreat” instead of “opening” to refer to sea ice change
- c) p 856, line 21: explicitly say COU or UNC experiments instead of “fully coupled”
- d) p 863, line 26: there is no Figure 6b
- e) p 866, line 29: there is no Figure 9c
- f) p 867, line 5: pCO₂ mostly determined by DIC. What’s happening to ALK, which should scale roughly to salinity?
- g) p 868, line 12: how is the Revelle buffer factor calculated? There is no exact formula available.

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