**Geoscientific
Model Development
Discussions**

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

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The authors thank Referee#3 for the detail comments and very constructive feedbacks. We tried to answer to all raised questions, and implement most of the suggestions in the revised manuscript. As a result, we believe the revised paper is more clear and much improved. Below are more detailed responses to Referee#3 comments:

R3: 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 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?

In addition of having its own model in Bergen, we think that despite its current resolution, it is very much applicable for future scenario studies up to year 2100 or 2200, given its reasonable computational time ($\sim$7 model years/day). The BCM-C will also be used for studies related to future changes in high latitude biological production, acidification, as well as interannual variability study between climate (ENSO, NAO) and regional carbon cycle. In addition to the BCM-C we are currently developing a new earth system model using improved components with higher resolution and faster integration time. For this purpose, BCM-C provide excellent tool for sensitivity testing, especially with respect to MICOM and HAMOCC5, both of which are part of the new model.

R3: p 849, line 11: potential density is referenced to 2000 db instead of 0 db in the original MICOM. What is being sacriïňА̧ced 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?

The selection of 2000 db as the reference pressure is motivated by the study of Mc-Dougall and Jackett (2005), who argue that these isopycnals are a reasonable compromise in approximating neutral surfaces of the World Ocean. In contrast, isopycnals referenced to 0 db generally become significantly non-neutral in the deep ocean. The pressure gradient force (PGF) is estimated using in situ density, which reduces problems with the dynamics away from the reference pressure compared to the traditional approach in isopycnic models where potential density replaces in situ density in the PGF calculation. The main drawback of using 2000 db reference pressure is a poor representation of stratification in some areas, mainly in high latitude haloclines, especially in parts of the Southern Ocean. The above statement has been included in the revised manuscript (Section 2.2. Ocean circulation model description).

R3: 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?

The model computes a bulk phytoplankton growth, which is based on multi-nutrient limitation (P, N, and Fe). The modeled bulk phytoplankton concentration is then divided into diatoms and coccolithophores, based on silicate concentration. The formulation leads to higher diatom fraction when the prognostic silicate concentration is high. We have clarified in the revised manuscript (Section 2.4) that the model computes the 'phytoplankton' (and not diatom) growth rate. The silicate function is only used to determine the diatom fraction out of the whole phytoplankton concentration.

R3: p 851, line 23: how is N fixed?

Nitrogen fixation is parameterized as the relaxation of surface layer deviation of the N:P ratio of nutrients. Thus, whenever there is more phosphate than nitrate (i.e. based on the Redfield ratio) in the surface layer, phytoplankton fixes atmospheric nitrogen and immediately recycled to nitrate. This description has been added to the HAMOCC description section 2.4.

R3: p 853, line 12: explain how the mixed layer depth is determined.

The mixed layer depth in the model depends on entrainment and detrainment processes, such as wind steering, current sheer, and density stratification. It is parameterized based on study by Gaspar (1988). More information on MLD is included in the revised manuscript (Section 2.2).

R3: 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.

The reason that the CO2 is only updated once a year is because of the nature of restart-

step of the ARPEGE model, which is 30-days. Since the model run on calendar year (i.e., 365 days/year), the restart period can stop in the middle of a month, which conflict with the LPJ model that restarts every month. It is still to be explored whether or not the model will be suitable for seasonal studies. In terms of the physical variability, the model produces well the expected seasonal climate variations (also see supplemental figure 1). However, the concerns would be from the carbon cycle side.

R3: p 856, line 7: describes Figure 1. The match with observation is very good indeed for the annually averaged ïňĄelds. How about for the different seasons? Is the model appropriate for seasonality studies?

In the revised manuscript (Section 4.1., we have include reference to supplemental figure 1, describing similar seasonal model-data evaluation using the Taylor diagram. In general, for different seasons, the model also performs relatively well compare to the observation. Main model strength remains in simulating the seasonal and spatial variability in surface air temperature 'sat' and sea surface temperature 'sst'. Given the reasonably good fit in simulating the seasonal climate variables, the authors believe that the BCM-C has potential for seasonal-to-interannual climate-carbon cycle interaction study. Although, as mentioned above, the carbon cycle part need to be first validated rigorously.

R3: 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 PaciïňĄc 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, 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 ïňĄxed) 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.

In HAMOCC5, the solubility of CO2 (as well as O2 and N2) gas in seawater follows the generalized formulation of Weiss (1974). In the revised manuscript, we have also included 'back-of-the-envelope' calculation of CO2 gas solubility for the future scenario (end of 21st century) as compared to the preindustrial period (1850s). The change (of -6%), while not negligible, is relatively small compare to the change in high latitude (close to -20%). We have also included additional figure (i.e., Figure 11a) showing the relative change in surface water solubility. In the revised manuscript, we follow the referee's comment, and replace the word 'negligible' with 'smaller'.

R3: 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?

They represent postprocessing (we noted this in the revised manuscript). Section 4.5.2 is further clarified. In addition of including a plot of relative change in CO2 gas solubility and gas transfer rate, we also improve the text in explaining how each of these signals variation affect the regional oceanic carbon uptake.

R3: 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 pre-scribed CO2 run instead of CO2 emissions run, because delta_pCO2 would obviously be different in emissions run if uptake is different.

This is, of course, the better method to separate the solubility signal from the gas exchange, but it would require more computational power and time to generate such simulation. Nevertheless, in the revised manuscript, we managed to replace the 'previous UNC' with the new UNC simulation where the atmospheric CO2 concentration is prescribed (from the COU), hence reduce the inconsistencies in the delta_pCO2 variation.

R3: 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?

This is mainly due to the fact that climate change also affects other physical properties such as precipitation. These properties also affect the NPP and respiration in the model. Fig. 6 only shows the sensitivity of NEP, NPP and Rh due to change in T, where the background change in precipitation is not shown. In certain regions (e.g., S10-S30), change in surface temperature is also associated to non-linear change in precipitation. On the other hand, in northern hemisphere, N90-N60, change in temperature has relatively linear correlation to change in precipitation, hence changes in carbon cycle (in Fig 6d, 6e, and 6f) is more monotonic. This has been added into the discussion section as well. We have also added supplemental Fig. 4 showing how the simulated changes in surface temperature relate to change in precipitation for different regions. Figure 6 has been revised and labeled accordingly.

R3: Also, p 861, line 5, why is carbon uptake given by the difference between NPP and respiration? NPP should already include respiration.

In the LPJ model, the actual carbon uptake is called Net Ecosystem Production (NEP), which is depends on NPP, respiration or remineralization (Rh), and fire-lost terms. NEP=NPP-Rh-fire. The term fire is generally smaller than NPP and respiration.

R3: 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?

In general, the retreat of sea ice increases marine net primary production significantly, hence the export flux (both POC and CaCO3) out of the mixed layer depth. On average, the net primary production in the newly opened water (COU experiment) regions increase by approximately a factor of six as compared to that with sea ice (UNC ex-

periment). More detail analysis of future climate impact on high latitude biological production is currently underway.

R3: 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.

We agree with the Referee's comment and have revised the sentences. In the revised manuscript, we make it clear that it is the 'anthropogenic' carbon uptake that has keep increasing in different regions, as a results of continuous increase in atmospheric CO2.

R3: 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"

The above suggestions have been implemented in the revised manuscript.

R3: d) p 863, line 26: there is no Figure 6b e) p 866, line 29: there is no Figure 9c

The above errors are fixed accordingly.

R3: f) p 867, line 5: pCO2 mostly determined by DIC. What's happening to ALK, which should scale roughly to salinity?

While ALK plays important role in certain surface regions where the fresh water sources and sinks are non-negligible. However, surface ocean grid cell in the model represent large volume, where 'ALK-effect' on pCO2 due to changes in fresh water fluxes is relatively small compared to the 'DIC-effect'. Earlier study (Tjiputra and Winguth, 2008) using similar ocean carbon cycle model shows how DIC variability control mostly pCO2 variability.

R3: g) p 868, line 12: how is the Revelle buffer factor calculated? There is no exact formula available.

The buffer factor is estimated using the formula from Maier-Reimer and Hasselmann (1987). This information is now included in the revised manuscript (Section 4.5.3).

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