

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#2 for the critical and constructive comments. Two of the three Referees pointed out that the current model simulations' set up doesn't represent the effects of climate change on terrestrial and oceanic carbon uptake accurately because both COU and UNC simulation have different atmospheric CO₂ concentration. In light to these critical comments, we have decided to replace the UNC experiment with one where the atmospheric CO₂ is prescribed from the COU, but still without climate change. All the results, analysis, discussions, and figures in the revised manuscript have been updated accordingly. Below are more detailed responses to Referee#2

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comments:

R2: The main major comment I have is that the analysis somewhat mixes geochemical and climate effects on the carbon cycle. Already in the abstract, the fact that the ocean carbon fluxes are similar in the COU and UNC simulations does not tell anything about the climate-carbon feedback, contrary to what is wrongly implied by Tjiputra et al. Indeed, the 2 simulations have very different atmospheric CO₂ and one would have to correct for this if looking for climate effects on the carbon fluxes.

As mentioned above, we agree that the previous model set up does not represent the climate impact on carbon uptake correctly (or inconsistent). To avoid inconsistencies when illustrating the climate-carbon cycle feedback (or climate impact on carbon uptake), we carried out a new experiment UNC. Here, the new UNC simulation applied the same atmospheric CO₂ concentration as the COU simulation, but remain without the radiative effect on atmospheric CO₂ change. The new analysis in the revised manuscript now yields more consistent estimates of climate-carbon cycle feedback.

R2: Later, in the land analysis, the authors have followed the same analysis than in Friedlingstein et al. (2006), introducing beta and gamma factors. Such analysis is not performed for the ocean part and I found section 4.4 very confusing.

In the revised manuscript, we have added a new subsection (Section 4.3.) where we compute the gamma and beta factor for both the ocean and land reservoirs. More analysis and discussions has been added describing how the value from BCM-C compare to the other models, especially those applying the same components. As suggested, section 4.4 has been revised substantially as well (see below).

R2: Even if all components are described elsewhere, I found the model and simulations descriptions not very precise.

The reason for the relatively short model description is because all of the model components used in the BCM-C has been tested and validated separately and many publi-

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cations has described and discussed the model individually. Therefore, the authors try to focus more on the updates and essential model parameterization that affect the coupling and carbon climate feedback related issues. As suggested, the revised manuscript has been improved to include more details updates with respect to previous publications. These include updates to: ARPEGE (more detail on the revised orographic gravity wave drag scheme and vertical diffusion scheme), MICOM (clarification and motivation to the chosen pressure reference levels for the vertical coordinate), HAMOCC (further detail description on processes such as, phytoplankton classes, nitrogen fixation, updated carbon chemistry, etc.), and Sea ice (The GELATO model description has been shorten up).

R2: Why is the GELATO model described if not used here?

The GELATO model description has been removed in the revised manuscript. We only include a sentence describing the potential alternative option for the sea-ice model in the BCM-C (see revised Section 2.3).

R2: How is the model initialized? Is the model still drifting after 600 yrs?

The model experiment (COU and UNC) and initialized from the final state acquired from the REF experiment. This information is now noted on Section 3.2 of the revised paper. In the 'Preindustrial reference run' section (3.1), we have included a sentence stating that the model reached a reasonable steady state after 600 years of spin up. The readers are also referred to supplemental figure 2.

R2: The analysis of the land carbon cycle follows partly the method proposed by Friedlingstein et al; 2006. But I found the discussion of the gamma term too short. How are precipitation changing in the COU simulation? How does this affect land carbon uptake and the gamma term?

As suggested by two of three referees, we have looked at the precipitation field in more detail, particularly in respect to the gamma term. In the revised manuscript, we have

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shown why, for some regions, the change in carbon uptake to changes in temperature is very non-linear (e.g., S10-S30). We show that this is partly due to changes in regional precipitation is non-linear with respect to changes in surface temperature. In addition to more discussion, we have also included supplemental figure 4, showing regional variation in precipitation as compared to changes in temperature. It shows that for certain region changes is temperature correlates reasonably with changes in precipitation, which consistent with linear gamma effect (see also Section 4.4, in the revised manuscript).

R2: Again, I found section 4.4 very confusing. What are the beta and gamma values for the ocean component? I think this section needs careful re-organization.

We agree with the comment, and we have carefully reorganized this section. In the revised manuscript, we have improved the overall discussions with regards to the regional ocean climate-carbon cycle feedback analysis (Section 4.5 now in the revised paper). In addition, we have reorganized the analysis into three subsections (4.5.1, 4.5.2, and 4.5.3) describing how climate change affect: the temporal and regional oceanic carbon uptake, air-sea CO₂-flux determining properties in the model, and the regional Revelle factor.

R2: This model yields one the largest climate-carbon feedback value when compared to Friedlingstein et al. 2006 11 models. Why is it so?

The model actually yields relatively average carbon-climate feedback as compared to the other models (Section 4.3). See also Fig. 5 in the revised paper. For the year 2099, the computed beta factor (for both land and ocean) for BCM-C is well within the standard deviation from other models. Only the sensitivity of the land carbon uptake to temperature lies in the extreme range of other model, but it is still within the standard deviation range.

R2: How do the model results compare to other models that use one or several of the components used here (LPJ, HAMOCC, ARPEGE. . .)? What do we learn from that

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comparison?

A new section (Section 4.3) has been added to the revised paper, comparing how the BCM-C compare to the other Earth system models, particularly with other models using the similar carbon cycle components: MPI (HAMOCC), CLIMBER (LPJ), and BERN-CC (LPJ). The section also discusses the comparison between the BCM-C simulated carbon uptake sensitivity to changes in temperature and atmospheric CO₂ concentration, as compared to the other models.

R2: One big novelty here is that the ocean component is an isopycnic model. What are the implications (is the impact of stratification on air-sea carbon fluxes somewhat different to what other models predict?).

The referee is invited to read the study by Assmann et al. (2009, also submitted to the same journal), where they focus more in evaluating the HAMOCC-MICOM coupling performance as compared to other model or observation. In general, they show that there is no major discrepancy between the spatial carbon fluxes distribution simulated in the isopycnic model as compared to other z-coordinate models and observation. We also pointed out in part of the manuscript that, in certain regions, the use of bulk MLD (formulated in MICOM) resulted in slightly stronger carbon uptakes.

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