

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 Referee1 for the constructive feedback. The manuscript is now revised considerably to include all of the Referees' suggestions and comments. Below are more detailed responses to Referee1 comments:

R1: writing is good, though I would encourage the authors to use the past tense when writing, and the active voice where possible. For example, the first two sentences of the abstract would be better written as: "We developed a complex earth system model by coupling terrestrial and oceanic carbon cycle components into the Bergen Climate Model. For this paper, we have generated two model simulations (one with ...) to study

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the large scale climate ... " and so on.

The above suggestions have been implemented in the revised manuscript.

R1: I believe the "E" in "Earth system model" should be capitalized whenever used. The Model Description section seems somewhat brief, given that requirements of this journal. I understand that most model components are described elsewhere – but be sure to fully describe any differences in the implementation of the model components that are not described in these previous publications.

The suggestions has been included in the revised manuscript, and in response to the other referees, we have added further details in the model description subsections: 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, and mixed layer depth computation), HAMOCC (further detail description on processes such as, phytoplankton classes, nitrogen fixation, carbon chemistry, etc.), and Sea ice (The GELATO model description has been shortened).

R1: Additionally, it may be helpful to include a section where you describe how this model differs from (or where it falls in) the suite of coupled climate-carbon cycle models that already exist. To what extent do model components overlap with other models? How "independent" is this model from others? For example, the LPJ model has been implemented in any least one or two other models, as has HAMOCC (this information can all be found I believe in the Friedlingstein 2006 paper). A section like this may help to clarify and understand the results and how they compare to other model (e.g. do other LPJ-based models give similar terrestrial carbon cycle results)?

In the revised manuscript we have added a new section (Section 4.3. Global climate-carbon cycle feedback) comparing the global gamma and beta factors for both the ocean and land simulated by BCM-C and compare them with other Earth system models, in particular we emphasize how they compare to other models that use similar

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carbon cycle components as BCM-C (i.e., HAMOCC and LPJ).

R1: On page 858 lines 16-18, the numbers given here for terrestrial carbon uptake do not seem to match the numbers shown in Figure 3c. Could you clarify?

This is especially because Figure 3c represents a ten-years running-average of the terrestrial carbon uptake. We have fixed these inconsistencies in the revised manuscript.

R1: Be careful of the terminology you use throughout. When referring to positive climate-carbon feedbacks, be sure that you are not including in this also the effect of negative concentration-carbon feedbacks, which do not involve climate change. For example, on page 858, lines 21-24, you infer a conclusion about the relative strength of terrestrial positive climate-carbon feedbacks based on the straight difference between the COU and UNC run. This is not necessarily correct, as the COU-UNC difference includes also different CO₂ concentrations, and hence includes the effect of additional concentration-carbon feedbacks on top of the climate-carbon feedback (e.g. you may have both a very strong concentration-carbon and very strong climate-carbon feedback on the ocean side, which explains the small difference there). To isolate the effect of positive climate-carbon feedbacks, you would need to do the feedback analysis as in Friedlingstein's paper (which is partially done later in the paper). Where you have NOT done this, be sure to acknowledge the COU-UNC difference includes BOTH positive climate-carbon and negative concentration-carbon feedbacks (e.g. also on page 859, lines 18-20; page 863, lines 18-21; page 864, lines 11-12; and page 869, lines 20-26).

The authors agree with the Referee that climate-carbon cycle feedback between the two simulations (COU and UNC) is not justified by simply calculating the difference between the two simulations, due to difference in the simulated atmospheric CO₂ concentration. To avoid inconsistencies when illustrating the climate-carbon cycle feedback (or climate impact on carbon uptake), in the revised paper, we have replaced the previous UNC simulation with a similar simulation but applying the same atmospheric CO₂ concentration as the COU simulation. Hence, the estimated climate-carbon cycle

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feedback is more consistent.

R1: Equation 3: This is not quite the same (I think) as the gamma land calculated by Friedlingstein 2006? Could you clarify to what extent this quantity is or is not different?

This equation is fundamentally similar to that of Equation 2 (in Friedlingstein et al., 2006 paper). In our study, we performed an additional simulation with the land carbon cycle model. The simulation, which we referred in the manuscript as 'UNCb', is done offline and used prescribe climate change (physical) fields from the COU experiment and constant preindustrial atmospheric CO₂ concentration (284.7ppm). Therefore this offline experiment computes only the sensitivity land carbon cycle towards climate change (i.e., temperature).

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