**Geoscientific
Model Development
Discussions**

Interactive comment on "Development of a system emulating the global carbon cycle in Earth system models" *by* K. Tachiiri et al.

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[Sorry I uploaded an old version. This is the correct version.]

We thank the three reviewers for their helpful comments. Following their comments, we will revise our manuscript.

Our responses to the reviewer's comments are as follows.

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[To Reviewer 1]

(General comments)

We will check the whole manuscript and reorganize it if needed. Relatively less im-

portant figures will be deleted or moved to a supplement, and more description will be added to other figures. For confirmation, not CO2 concentration but global mean surface air temperature is calculated by MIROC-lite and used to select the GCM data to drive the vegetation model.

(Major comments)

1.We will revise all figures as pointed out. For Fig. 1 we are thinking of using parentheses for "atmospheric carbon calculation", as this part is not used for concentration scenarios but is needed to use emission scenarios. The figures pointed out to be redundant are deleted, moved to a supplement, or explained more carefully. The number 1.61, very close to the range (1.27 +/- 0.27) mentioned in Dufresne and Bon (2008)* for recent AOGCMs, in Fig 5 is an experimentally estimated value of the MIROC-lite's original 2xCO2 equilibrium climate sensitivity based on its underlying parameterisations.

*Dufresne, J.-L., and S. Bony, 2008: An assessment of the primary sources of spread of global warming estimates from coupled atmosphere–ocean models. J. Climate, 21, 5135–5144.

2.We will check explanation and discussion for figures and results, and make them better organized.

3.As MIROC-lite has a marine carbon cycle component, we think it is possible to call it an EMICâĂŤin the revised manuscript we will clarify our definition for EMICs.

(Minor comments)

We basically revise following the comments (1-2,4,6,8,10,12,15,17-20). Our responses to other comments are as follows.

3 and 5. Here "original model" means the physical part which is common for Oka et al. (2001) and Oka et al. (2010). We will put clearer description.

7. As mentioned above, we think Fig 5 is important. Let us put more explanation for

the figure, rather than deleting it.

9. We will delete the figure, presenting an off-line spin-up, or move it to supplement.

11. Yes, it is the 2xCO2 equilibrium climate sensitivity.

13. The GCM's dataset has daily climate. So if we can determine the year (from the change in global mean temperature), by solving a quadratic equation approximating the GCM's temperature increase, it is straight forward to get daily climatic fields. Please note that not CO2 level but temperature is used to determine the year to be referred.

14. We meant "ensemble experiments" in which the model is run for a stabilization scenario with various parameter sets.

16. The formulation is SC=2073.1-125.62*SST+3.6276*SST2-0.043219*SST3, following the OCMIP's protocol. www.cgd.ucar.edu/oce/klindsay/ocmip/HOWTO-Abiotic.pdf, with original reference to Wanninkhof (1992)**.

**Wanninkhof, R., 1992. Relationship between wind speed and gas exchange over the ocean, J. Geophys. Res., 97, 7373-7382.

21. We will try to rewrite in more intelligible way.

[To Reviewer 2]

We will correct all of the three parts as pointed out.

[To Reviewer3]

(numbers are corresponding to paragraphs)

1-3. Thanks for these comments.

4. The low resolution 3D OGCM including an ocean carbon cycle is named iced-COCO (Hasumi, 2006)***. We will put this in the Abstract.

***Hasumi, H. CCSR ocean component model (COCO) version 4.0. Report No.25,

Center for Climate System Research, The University of Tokyo, March 2006.

5. The model is originally developed for studying uncertainty in global climate change, but also maybe useful for impact assessments.

6, 8. We will revise following the comments.

7. Evaporation is considered before runoff process, although interception by vegetation is not explicitly considered.

9. Probably you are right –we are checking carefully.

10. We will revise as you pointed out.

11. Respecting each model included, currently we are calling the system as the loosely coupled model (LCM).

12. We do not mean that representing the quasi-equilibrium period is not important. Our point is that the LCM works best for a period of increasing CO2 concentration, and that this is the most important, especially for simulations over the coming 50-100 years when radiative forcing seems likely to increase.

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In addition, we are now redoing our experiments using the MIROC-LS's 2xCO2 radiative forcing (3.09W/m2) which is somewhat lower than what used in the current version of the manuscript (3.71W/m2). It has been confirmed that by this modification the results are not so significantly changed for all experiments.

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