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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of the carbon cycle components in the Norwegian Earth System Model (NorESM)" *by* J. F. Tjiputra et al.

Anonymous Referee #2

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Review of Tjiputra et al. GMD

In this manuscript, Tjiputra et al. focus on evaluating the land and ocean carbon components of the newly developed Norwgian Earth System Model (NorESM). This model has been used to run several simulations (including future scenarios) contributing to the Coupled Model Intercomparison Project phase 5 (CMIP5), and is (or will be) used in many inter-model comparison studies. It is thus timely to propose such an evaluation.

In the evaluation, the authors first describe the spatial distribution of simulated marine biogeochemical tracers, such as phosphate, oxygen , dissolved inorganic carbon, alkanity, and compare those fields to available climatologies. They also present how simulated ocean NPP and air-sea carbon fluxes compare to available observation-based climatologies. On the land side, the authors focus on GPP and compare the simulated

values to values derived from the FLUXNET network. .

This is a useful study, and it fits very well the scope of Geophysical Model Developments, and as such, should be published. But I think the manuscript could be improved, as it suffers now from several major problems that I list below. If the authors address my concerns, I would support publication in Geophysical Model Developments.

Specific comments / Major issues:

1). Land / Ocean balance. I acknowledge that fact that the authors have tried to combine the land and the ocean carbon cycle analysis in the same manuscript. But, as a reader, you still very clearly see that the ocean and land parts have been written by different people. The abstract is a good example of that, listing a few global numbers for the land, and focusing on deficiencies in the spatial representation of some marine tracers. Again, in the result section, the ocean analysis is based on 13 figures, whereas the land one only uses 4 of them. The ocean section is structured with sub-sections – it is not the case for the land one. . . Of course, a perfect balance is not possible (and not wished), but more integration / balance is needed.

2). Improvements from a previous version. A general goal in model evaluation is, to my view, to show how a newer version of a model is better / worse than the previous one – and for what reasons. This is only touched upon in the present manuscript and should be better discussed here. I would integrate in the main article some of the figures that are now only in the supplementary material. Is this possible to draw conclusions for the land part as well?

3). Links between physical biases and biogeochemistry / carbon cycle. This point is illustrated for the ocean part - and could be even better discussed with the inclusion of figures showing previous modelling results. But it is clearly lacking on the land side. I would strongly recommend some additional analysis to strengthen the manuscript.

4). Ocean analysis. I feel that the evaluation of 3D variables first (nutrient, dic, alkalin-

ity) and then NPP and air-sea carbon fluxes are a bit dis-connected. They should be better integrated. For example, could the authors show the different nutrients and the most important limiting nutrient spatially, and how it relates to NPP ?

Interactive comment on Geosci. Model Dev. Discuss., 5, 3035, 2012.