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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 #1

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Synopsis

The manuscript evaluates the carbon cycle components of the NorESM Earth system model in simulations performed within the 5th Phase of the Coupled Model Intercomparison Project. The results of this (and similar) model(s) will have implications for the IPCC's AR5 processes and thus a thorough model evaluation is necessary. The authors provide a detailed analysis of the performances of the two carbon reservoirs of NorESM jointly. While documenting recent improvements in the model, the manuscript goes beyond a technical report format: it evaluates not only the distribution of model parameters but also describes the behavior of the processes that lead to these distributions. Such study will be of interest to a broad scientific community of the developers and users of CMIP5 experiments. The topic, in my opinion, also fits the journal's scope.

I would recommend minor revisions following the comments below.

Comments

- p. 3037 line 23: the ocean model in NorESM is indeed unique among other CMIP5 models in using isopycnic coordinate system. The authors explain the advantages and issues of this algorithm later in the manuscript. However, it would be very useful for the users of CMIP5 data to learn about them earlier.

- on p. 3038 lines 4-5 it is stated that 'biogeochemical states . . . strongly depend on the quality of the physical fields in the model'. While providing a rather detailed analysis of the ocean's physical state (including ocean temperature, salinity and MLD), the paper omits analogous evaluation of the physical fields which influence the terrestrial carbon cycle (for instance air temperature, precipitation, surface radiation). This omission leaves the land carbon cycle evaluation incomplete.

- p. 3039, line 5: Here and in several other locations in the paper an improvement of one or another model component is mentioned. However, the implications of this (and further) improvement(s) are not discussed. Hence, it sounds rather unjustified whether these improvements were really necessary.

- p. 3040, lines 5-10: Have these recent model developments been documented and evaluated elsewhere? More details on these model improvements would be helpful.

- p. 3040, ocean carbon cycle model description: It would be helpful to see all major features of the model summarized in a table. Such a table could include a list of model tracers treated prognositcally in the ocean.

- p. 3041, lines 1-2: Please provide a citation for the sediment model. Also mention here if sediments were included in your CMIP5 experiments.

- p. 3041, line 3: Mention that the NPZD model in HAMOCC is extended by DOC.

- p. 3041, line 16: add 'constant' before Redfield.

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- p. 3041, line 24: add ‘the upper’ before 100 m.

- p. 3041-3042: Description of ocean carbon cycle model misses information on which light scheme was used in the experiments.

- p. 3041-3042: Please add a description of how weathering fluxes were treated in your simulations.

- p. 3043-3044: It would be useful to see a summary of all land carbon cycle compartments/pools (dead and live) in the model listed in a table.

- p. 3044: Specify how autotrophic and heterotrophic respirations are calculated in the model as these processes are critical for NPP variations on land.

- p. 3045, line 15: It takes several tens of thousands of years for deep-sea sediments to reach an equilibrium state if you initialize them from zero values. Give more details with regard to sediments initialization and spin-up.

- p. 3046, line 17: Also discuss if the phasing of the simulated variability is in line with observations.

- p. 3046-3047: Section ‘Ocean biogeochemistry’ starts with an extended comparison of the physical fields (T, S, MLD) with observations. Consider to separate this very useful evaluation of physical parameters in a new subsection.

- p. 3048-3053: The globally integrated values (for instance, primary production, export production, etc) and budgets for the ocean are now discussed and spread over several locations in the text. It would be very helpful if they were summarized in a separate table, perhaps similar to your Table 1 for terrestrial parameters.

- p. 3048, lines 18-19: specify what this is improvement is due to.

- p. 3049, line 9: again, how this improvement was achieved?

- p. 3049-3050: In your discussion on ocean biogeochemical tracers, you attribute their

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distributions to water masses properties and other physical properties in the model. This is alright, but what is the role of biogeochemical parameterizations of for instance, the sinking fluxes, remineralization (particularly specific to the model HAMOCC) in the distributions of phosphate and oxygen?

- p. 3049, line 25: How well does the model perform with regard to iron?

- p. 3050, lines 24-25: An explanation why is it not easy to simulate the correct TA would be helpful for the readers without background in running global ocean biogeochemical models.

- p. 3050, line 28: add a ‘d’ in ‘compare’

- p. 3051, lines 3-6: Discuss what implications does an overestimated TA have on the ocean buffer capacity.

- p. 3051, lines 7-8: It is unclear why do you need to show Alk minus DIC.

- p. 3052, lines 21-22: Model performance with regard to silicate distributions not shown/discussed. Hence, while maybe true, it sounds rather speculative to attribute the discrepancy in PIC export to surface silicate concentrations. You either need to include silicate in your evaluation or cite previous studies.

- p. 3053, line 9: Explain why the Southern Ocean is a region of increasing interest.

- p. 3053, lines 19-20: Why is this relevant here (the three regions playing a key role in the future)?

- p. 3054, lines 5-8: You are aware of course that the DIC anomaly is not the same as anthropogenic carbon. It may be a suitable approximation though. This has to be explicitly mentioned.

- p. 3054, line 17: Explain why the model estimates of CO2 uptake are lower than observed.

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- Section 4.3: Is another paper focused on the assessment of the terrestrial biogeochemistry in NorESM planned? If not, this section needs a somewhat more detailed analysis, i.e. starting with the evaluation of physical model parameters that are critical for terrestrial biogeochemistry. Evaluation of the terrestrial carbon cycle component could be expanded by discussing model performance with respect to reproducing radiation / surface albedo.

- p. 3055, lines 1-3: Explain why the terrestrial carbon uptake is lower.

- p. 3055, line 7: A reference to precipitation and temperature distributions is unfounded without showing them.

- p. 3069, lines 1-5: I am not sure this outlook is really relevant for the paper.

- Figure 13: The colorbar is extremely confusing: some colors (e.g. blue, red) are repeated several times. Does blue in continents shading stand for no data or values close to zero, or close to 100?

- Figures 14-16: Likewise, the colorbar does not provide necessary details. For instance, it is unclear if the dark blue in Antarctica for GPP values in Fig. 15 is zero or not.

- Figure 16, legend: Clarify which respiration is shown here.

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