Geosci. Model Dev. Discuss., 8, C2505–C2511, 2015
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Geoscientific Model Development Discussions
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Interactive comment on "The carbon cycle in the Australian Community Climate and Earth System Simulator (ACCESS-ESM1) – Part 1: Model description and pre-industrial simulation" *by* R. M. Law et al.

Anonymous Referee #1

Received and published: 16 October 2015

Review for the manuscript "The carbon cycle in the Australian Community Climate and Earth System Simulator (ACCESS-ESM1)-Part 1: Model description and preindustrial simulation" by Law et al.

In this manuscript, the authors describe and evaluate the land and ocean carbon cycle components coupled to the ACCESS-ESM1 model. For the land model, they focus on comparing the significance of having prognostic versus prescribed LAI values. The former is found to produce higher temporal variability in globally averaged GPP and respiration. They show that biases in the vegetation carbon simulated in the model is

related to the physical model that supplies insufficient precipitation in certain regions. The evaluation of the ocean carbon cycle is done through comparing ACCESS-ESM1 to a subset of CMIP5 models and with observations, focusing on the surface tracers and carbon flux and NPP processes. Following a 1000 yrs of preindustrial run, the WOMBAT is a source of carbon to the atmosphere, and the authors attribute this to the bias in the surface alkalinity.

The study fits well within the scope of GMD, but it is my opinion that the manuscript is too brief with many missing elements essential for a carbon cycle model evaluation manuscript. The introduction should be extended to elaborate better the motivations and justifications for the need of such documentation. As it is, it is unclear if the purpose is simply to produce a technical description of the model or to evaluate the model performance, or both. Below I have some general comments and suggestions to improve the manuscript, followed by more specific comments.

General comments: The authors often refer to an accompanying paper by Ziehn et al., which appears to analyze the same model for historical simulation. Given the limited evaluation that can be done for the preindustrial simulation, it may be worthwhile to combine them into one study. Otherwise, both papers should be submitted and available at the same time in GMDD for the reviewers. For instance, on page 8079, lines 6-7, the reader is referred to a different publication for information regarding impact on the atmospheric CO2. I found this difficult to comprehend since this impact on atm. CO2 should be seen in the preindustrial simulation as well. At the least, the authors have to provide some statements summarizing the finding in Ziehn et al. whether or not the impact is significant, why, etc..

The motivation for evaluating the current model against ACCESS1.3 on page 8079 is also unclear. Why not compare against observations? If there is a strong motivation to understand the improvement in the physical model, than this needs to be stated up front. In this case, more details on the physical improvements should be provided in the model description section. Are these improvements expected and why? As it is, section 4.1 and Fig. 2 appear to be unnecessary and disconnected from the rest of the manuscript. Consider to add more details in the simulated bias or improvement in the spatial precipitation pattern here as the authors pointed out that precipitation bias in the model leads to bias in land vegetation.

For the Ocean physics, page 8081, the first paragraph essentially can be summarized into the last sentence, which makes the paragraph appear unnecessary. But I think there are many details being left out here. E.g., why lower AABW strength lead to warmer deep ocean? Why MLD in the two simulations differ in the Ross and Weddell Seas? Is there any new physical parameterization that would lead to this differences? How these changes impact the distribution of biogeochemical tracers (see also additional comment below).

For the land model, the comparison between prescribed vs prognostic LAI is certainly interesting, but there is also limited actual evaluation for its performance compare observational estimates or other CMIP5 models (some suggestions are provided in the specific comments below).

There are many hand-waving statements throughout the manuscript, which can relatively easy to confirm with more detailed assessments. For example, it is stated in the abstract (and P8089) that the "model overestimates surface nitrate values", but this is based on the relative difference in the globally averaged values between model and observations (WOA). And the authors attribute this bias to the export of particulate organic carbon (POC). How so? The model computes nitrate based on stoichiometric ratio to phosphate, with no explicit nitrogen cycle and nitrogen fixation, so it is not directly obvious that this bias is due to POC. A spatial surface nitrate map compare to the WOA and its difference would be more helpful in identifying the mechanism responsible for the bias. Is there similar bias with phosphate? Other source of bias could also be attributed to the parameterization of the ecosystem model (e.g., phytoplankton growth, zooplankton grazing rates, etc.), circulation, etc.

P8081, end of last paragraph: The authors indicate and later state that the bias in the freshwater fluxes leads to bias in alkalinity, pCO2, and finally air-sea CO2 fluxes. Again, this statement is not confirmed by the quantitative analysis available in the manuscript. Wouldn't alkalinity bias due to freshwater fluxes, be cancelled out by the respective DIC-bias? I think how the model formulate the inorganic carbon formation in the surface and fluxes throughout the water column also plays a major role and should be tested before the above statement can be made.

For the ocean carbon cycle performance, the authors focus on the surface sea-air carbon fluxes and NPP. There is no discussions on the interior biogeochemistry. Given that the paper evaluates the deep water ventilation (Section 4.1), it is necessary to also discuss how large scale ocean circulation (together with vertical particle fluxes and remineralization) alter the biogeochemical tracers distribution in the interior ocean. If parallel BGC simulations with different physical are not available, some assessment on the tracer budgets within the available long simulations would be useful to assess the stability of the model. Mean state of vertical section in different basin compare to observation can also be helpful.

Specific and technical comments: Page 8073, Line 28: How is the partial pressure of CO2 computed? Briefly describe the inorganic carbon chemistry formulation used.

P8074, L21: consider replacing 'increasing' with 'changing'

P8074, L24: remove 'responding to'

P8075, L15: What is the spatial resolution of the land? Vertical resolution of the ocean?

P8075, L26: Describe the values of the 'observed land carbon uptake'. Which data set? Global or regional? To my knowledge, there is no directly observed land carbon uptake.

P8076, L26: CMIP5 historical and RCP scenarios

P8077, L23: It is not clear if the fertilizer application here represents anthropogenic or not. I would assume this is natural because of the preindustrial period. Please clarify.

P8080, L15-16: Add a brief statement and reference to why we expect such small impact?

P8080, last paragraph: For non specialist readers, it would be useful to include some statements describing how LAI relates or impacts surface temperature.

P8081, L3: 500 year control, but Fig 14 shows 1000 years model run. Are these two different runs? Would be useful to provide a table list of all performed simulations.

P8082, L6: 601-700. Why not years 901-1000?

P8082, L12: Why choose this number: "2gC/m2"? Is there observational evidence to suggest this as indicative of a steady state? Some explanation/references would be useful.

P8082, L21-23: This statement needs to be better supported by additional, relatively straight forward, analysis. For instance, is it possible to find other regions with similar LAI/PFT characteristic (to this region) but with contrasting precipitation pattern? If so, do they show the expected plant growth?

Section 4.2.1: What are the budgets of the land carbon pools (vegetation/soil/litter/etc.)? How do they compare spatially with observational estimates or other CMIP5 models (Lifeng et al., 2015; Todd-Brown et al., 2013, and references therein).

P8084, L2-3: "Early test simulations . . . getting too low, . . ." More explanation is needed here. What mechanism causes the nitrogen drift? How strong is the drift?

P8084, L12-14: Cite reference for this statement.

P8084, last paragraph: please add some statements describing why the nitrogen and phosphorus pools behave differently? Some illustrative time series would be useful.

P8085, 1st paragraph: How this spatial pattern compares to other CMIP5 models and observational estimates (e.g., Fluxnet, Jung et al., 2011)?

P8085, L19-22: I consider this as one of the key findings of this study and should be highlighted more in the abstract or elaborated better in the conclusions as how to remedy this caveat.

P8086, L7-24: It is not clear what is the purpose of assessing the inter-annual variability (IAV) of the simulated GPP, NEE, etc. Is it critical for specific climate/carbon cycle projection? This motivation can be added into the introduction section. Is there observational evidence that support the simulated IAV?

P8087, L19-22: What are the differences? Is it possible to assess the reason behind these differences?

P8088, L2-3: This statement would be better supported with figures showing time series of DIC budget at different depth intervals (surface, intermediate depth, interior, . . .).

P8088, L4-5: How does the simulated spatial pattern compare to observation, consider add maps of NPP and its difference with the observation.

P8090, L16-17: Consider adding a similar figure as Fig 7 for sea-air CO2 fluxes together with observations.

P8092, L2: reducing surface salinity biases

Figs 3 and 4 captions: why not show results from ACCESS-ESM1 model (instead of ACCESS1.4) to be consistent with the title of the paper?

Fig 7b: Very difficult to distinguish the green lines. Why are there two solid green lines on certain latitudes? For the 'all other types' (solid thin green lines), are these relevant for your discussions? If not, I suggest to remove these lines to make the figure clearer, or use different colors.

Fig 11a: Why are there some discontinuities in the time series?

Fig 14: Consider replacing the colormap for the top panel with that used in Fig 13

References Jung, M., et al. (2011), Global patterns of land-atmosphere fluxes of car- bon dioxide, latent heat, and sensible heat derived from eddy co- variance, satellite, and meteorological observations, J. Geophys. Res., 116, G00J07, doi:10.1029/2010JG001566.

Lifen, J. et al. (2015), Scale-dependent performance of CMIP5 Earth System Models in simulating terrestrial vegetation carbon, J. Climate, 28, 13, 5217-5232.

Todd-Brown,ÂăK.ÂăE.ÂăO., et al. (2013), Causes of variation in soil carbon simulations from CMIP5 Earth system models and comparison with observations, Biogeosciences, 10, 1717-1736, doi:10.5194/bg-10-1717-2013.

Interactive comment on Geosci. Model Dev. Discuss., 8, 8063, 2015.