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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.

R. M. Law et al.

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We thank the reviewer for their comments. Each comment is addressed below with the original review in italics and our responses in normal font.

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

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Interactive Discussion

Discussion Paper

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.

The aim of the paper was to provide both a technical description of the model and, alongside the companion paper (Ziehn et al.), to evaluate the model. We apologise for the delay in submitting the Ziehn et al. manuscript and understand that the delay has made the review of this paper more difficult. Ziehn et al. has been submitted so that it should be available, at least on GMDD, by the time this paper is finalised. The final paragraph of the introduction will be divided into two paragraphs with the second paragraph providing more clarity on the scope of this paper (in respect to both the model description and evaluation) and that of the Ziehn et al companion paper. In particular section references will be included.

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..

It is our hope that both papers will be available together as soon as possible, as part 1 and 2 of the same study. While there are many ways that the model evaluation could have been divided across the two papers, we generally tried to match the simulation period with the period of observations. Thus, for land carbon especially, most of the comparison with observations is limited to present-day conditions and hence the historical simulation in the Ziehn et al. paper. Evaluation of the pre-industrial simulation focussed on aspects of the simulation that require many simulation years such as equilibration and interannual to decadal variability. We will try to make this clearer in the introduction (a re-written final paragraph) and in the first paragraph of Section 4.

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.

Since many components of the model had been previously documented, our focus here was on noting the updates/differences from those previously published versions. This was particularly true for the physical climate model. It was well documented in the ACCESS1.3 version and hence our aim was to show only that the physical model (ACCESS1.4) underlying our ESM (Carbon-cycle) version performed closely enough to

ACCESS1.3 that the earlier documentation was still valid for the current model version. In effect, no or little improvement was expected between ACCESS1.3 and ACCESS1.4 and that was the purpose of Fig 2, to confirm that the performance was very similar. Since this has obviously caused some confusion, we propose to move the description of the ACCESS1.4 differences from ACCESS1.3 to an appendix. This will ensure the model evolution is captured while allowing the body of the paper to focus more explicitly on ACCESS-ESM1 including relevant aspects of its physical climate simulation.

For the ocean evaluation, we now show key diagnostics of the ACCESS-ESM simulation (meridional overturning, mixed layer depth, sea-ice) and directly assess them and compare them to other CMIP5 simulations. The assessment is now focused on ACCESS-ESM. We do include a short summary paragraph on how ACCESS-ESM and its simulation differs from previously published ACCESS results.

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).

We just present the ACCESS-ESM results and then compare it to both CMIP5 simulations and ACCESS1.3 We then follow this with a more thorough discussion of some of the key differences between ACCESS-ESM and ACCESS1.3 in regards to AAWB and MLD. Some discussion of the consequences for BGC will be provided later in the paper when we assess the ocean BGC.

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).

When ESMs are run in concentration-driven mode, it is generally assumed that simulating the carbon cycle has no impact on the climate simulation. We chose to present both prescribed and prognostic LAI cases here to note the climate impact of using prognostic LAI as well as to demonstrate the changed variability in land carbon fluxes with prognostic LAI. These impacts can be assessed in a pre-industrial simulation. For comparisons with observations and other CMIP5 models, this is more easily done for present-day conditions and hence is covered by Ziehn et al. (for GPP, LAI, carbon pool size etc).

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.

First, we have dropped all references to nitrate because, as formulated, the model limiting macro nutrient is phosphate. Hence any link to processes like nitrogen fixation and denitrification are not relevant. Second, we now discuss the potential causes of the biases in the surface phosphate and link them to processes like phosphate uptake, POM remineralization and ocean circulation. To do this we have added figures of NPP, surface phosphate, Export Production (through 100m), and air-sea CO_2 fluxes. The figure shows both the ocean-only forced simulation and the subsequent coupled sim-

ulation. Most of the problems with the NPP appear in the coupled simulation, which reflects changes in the tropical circulation in the coupled model. The problem is most apparent in NPP because of excessive recycling of phosphate in the photic zone. Interestingly, by the time one gets to CO2 fluxes the differences between the ocean only and the coupled model are much less.

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.

It is better stated as biases in the salinity, which reflects both freshwater fluxes and the ocean circulation.

Yes, you are correct that bias in the alkalinity fields are partially linked to the export of calcium carbonate from the photic zone. By including a figure of the zonally averaged alkalinity section it is clear that the present simulation overestimates calcium carbonate export and underestimates the vertical gradient of alkalinity. However, the salinity biases are also important because of the high correlation between salinity and alkalinity in the ocean (both observed and modelled). While both DIC and ALK are influenced by the freshwater flux the former tracer can flux out at the surface while the latter one cannot. Therefore low salinity at the surface is also associated with low alkalinity and both biases help to explain the positive trend in the sea-air flux of CO2.

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.

Following the reviewers request we have added an assessment of the interior BGC fields by comparing the zonally averaged sections of DIC, ALK, oxygen and Phosphate with observations. We have also added time series plots of the global averaged DIC, ALK and sediment DIC and ALK values so the reader can assess the model drift. Note the sediment pool of DIC and ALK is small because the sediments are remineralized back into the water column on the annual timescale. This prevents large pools accumulating in the sediments but was done to improve numerical stability, by preventing instantaneous remineralization in the bottom in shallow water causing a large change in the BGC tracers.

The DIC in the ocean is slowly equilibrating with the atmosphere and if we expand the plot of the global net air-sea flux one can see a declining trend in the flux, which will require many thousands of years to reach a net flux of zero. Complicating the trend to a zero net flux is a climate system that is also not equilibrated and is also drifting to its mean state.

Specific and technical comments: Page 8073, Line 28: How is the partial pressure of CO_2 computed? Briefly describe the inorganic carbon chemistry formulation used.

Following the OCMIP3 protocol pCO2 ocean is computed using the simulated T, S, PO4, DIC and Alk.

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

Done.

P8074, L24: remove 'responding to'

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Interactive Discussion

Discussion Paper

Done.

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

Land resolution information will be added in the first paragraph of Section 3 and the second paragraph of Sec 3.1.1 (variable number of vegetation types per grid-cell). The ocean vertical resolution will be added at the end of the first paragraph of Section 3.

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.

This sentence has been modified to note that it is estimated global land carbon uptake and the data sources are given in the Zhang et al., 2014 paper.

P8076, L26: CMIP5 historical and RCP scenarios

Done.

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.

This is anthropogenic fertiliser despite being for the preindustrial period, because the same set-up is also used for the historical and future periods. The set-up is a compromise between using present-day fertiliser application rates but only applying them to the pre-industrial crop area. The last sentence of Section 3.1.1 has been rewritten to make this clearer.

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

'confirms' in this sentence was confusing. It was meant to indicate that an impact was expected, for the reasons stated on p8076, line 7-8, but given the size of the impact was uncertain, 'confirms' was misleading. We propose to change 'confirms' to 'indicates' and allow the paragraph following to elaborate on the difference and the link between

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the LAI and temperature change.

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

We have added in this paragraph an example of how the LAI could impact albedo for a snow-covered surface. We have also added a counter example where a large change in LAI has the opposite impact to a small change in LAI, and now note that there is no simple relationship between LAI and temperature since changes in LAI can potentially change many components of the surface energy balance.

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.

This paragraph will be rewritten as ACCESS-ESM1 results will now be shown rather than those from ACCESS1.4. ACCESS1.3-ACCESS1.4 differences will be noted in an appendix. This will reduce the number of model simulations referred to in the body of the manuscript and hence we do not feel that a table of simulations is necessary.

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

The conservation check was performed before the model simulation had completed. There is no evidence that conservation behaviour has changed significantly over the final centuries of the simulation. This is now noted in the first sentence of Section 4.2.1.

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.

As noted in the text, the distribution of imbalances was highly skewed with most tiles close to zero and a small proportion of tiles with much larger positive imbalances (up to 11000 gC/m2/100y in the ProgLAI case). Given all negative imbalances were small (minimum value -1.88 gC/m2/100y in the ProgLAI case), we took this as indicative of the precision of the carbon balance calculation and thus assumed that +-2gC/m2/100y

Interactive
Comment

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Interactive Discussion

Discussion Paper

indicated carbon conservation within the precision of the calculation. We have modified the first two paragraphs of Section 4.2.1 to explain this choice.

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?

The paragraph has been rewritten to include information from an example transect across India. This example shows that the size of the leaf carbon pool for the crop pft is highly correlated with the amount of rainfall. This supports the statement that plant growth is limited by rainfall.

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).

The carbon pools are compared with observations and other CMIP5 models in Ziehn et al. as it is more appropriate to do this from the historical simulation. Overall ACCESS-ESM1 gives pool sizes that agree reasonably well with observations.

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?

The issue was with the choice of initial pool sizes, which were too far out of balance to allow a sensible equilibrium to be reached. This was resolved by not allowing the inorganic soil mineral nitrogen and soil labile phosphorus pools to go below a minimum value of 0.5 gNm^{-2} and 0.1 gPm^{-2} respectively. Effectively a small source of nitrogen and phosphorus is input to the system predominantly through the model spin-up phase. The frequency with which this fix is required was assessed by checking how many tiles were at the minimum value at different parts of the simulation. Tundra and deciduous needleleaf vegetation types are the most affected vegetation types and more so for

Interactive
Comment

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Interactive Discussion

Discussion Paper

phosphorus than nitrogen, with the fraction of affected tiles dropping from over 40-60% to less than 5% for phosphorus and from 11-14% to less than 8% for nitrogen (nitrogen was incorrectly highlighted in the manuscript but this will be fixed and a more accurate description of the problem will be included).

While clarifying this issue, we found an error in eq 5 in the paper which should include multiplication by 'c' which effectively acts as a tuning parameter. This has been fixed and the values of the extra parameter have been added to the supplementary table.

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

This statement was describing features seen in the figure. We have changed the sentence from 'There is a suggestion of ...' to 'The figure shows ...' to make this clearer.

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

Nitrogen and phophorus timeseries have been added to Figure 6 (as panels c and d) and the description in the text will be expanded to address the 'why' question.

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

The seasonal cycle and spatial distribution of GPP are compared with observations and CMIP5 models in Ziehn et al., Sec 5.1.1.

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.

The response of vegetation, particularly the C4 vegetation type, to low rainfall is now mentioned in the conclusions along with a potential new development which may improve this aspect of the simulation.

P8086, L7-24: It is not clear what is the purpose of assessing the inter-annual vari-

ability (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?

Understanding the sensitivity of land carbon fluxes to climate on interannual timescales can aid in understanding how these fluxes may respond to externally forced climate change, and models can be more easily validated on interannual timescales (e.g. Fig 6.17, Ciais et al., WG1 AR5 Chapter 6, 2013). Observational evidence to support the simulated IAV is only available for the present day but we will check its comparability to our pre-industrial simulation.

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

The ocean-only simulation has quite different dynamics to the coupled simulation which has implications for the biogeochemistry and the ability to use ocean-only simulations to help with the spin-up. This will be addressed more explicitly in our revisions including some figures from an ocean only simulation.

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, ...).

Figures will be added to show the time-series of interior DIC at different depths to provide information on the evolution of DIC.

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

The spatial distribution of NPP will be added to the paper, both for the ACCESS-ESM1 simulation and for an ocean-only case.

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

It is not possible to directly compare to observations since the observation include both the natural and anthropogenic fluxes. But we now show the simulated fluxes and the observed values to enable some comparison of them. Regional and seasonal sea-air CO_2 fluxes are compared with observations in Ziehn et al.

P8092, L2: reducing surface salinity biases

Done

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?

We now only show ACCESS-ESM and then discuss how it compares to other CMIP models and to previous ACCESS versions.

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.

There are two bold green lines at some latitudes because there are two types of evergreen trees: broadleaf and needleleaf. The figure has been redrawn to distinguish these two types. All other types are shown as thin solid lines to demonstrate the statements in the text that in the tropics all the vegetation types have lower prognostic LAI than the prescribed LAI, while in the northern mid-latitudes the opposite is true; all vegetation types give larger prognostic LAI than prescribed. We feel this is an important point to make, but it does not require each vegetation type to be identified in the figure, and hence we prefer to leave the thin green lines unchanged.

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

As discussed in the methods section there were instabilities in the DIC tracer which we remedy by a slight change in the numerics of the ocean BGC equations. The instability causes the fluxes to go off-scale and then slowly recover. We now show this behaviour

rather than mask the anomalous fluxes.

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

Done

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