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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.

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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.

This paper describes the coupled Earth System Model (ESM) ACCESS-ESM1, integrating components for the atmosphere, land, sea ice, and the ocean. The innovation that goes into the model presented here is the coupling of carbon (C) cycle models on land and in the ocean, that (potentially) interact with climate. These climate-carbon cy-

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cle feedbacks arise due to the fact that all C (CO2) exchange fluxes between reservoirs on land and in the ocean are sensitive to climate and because climate itself is sensitive to atmospheric CO2, affected by these exchange fluxes. Tremendous amounts of work goes into the development of each of these components, and the coupling of these components itself is a major step forward and and will provide a highly desired addition to the relatively small ensemble of ESMs available already today. The components integrated into ACCESS-ESM1 themselves are not new and are used in other ESM in different constellations. Hence, the characteristics of the ESM presented here should not deviate substantially from other ESM predictions. Nevertheless, developing a new coupled model setup is all but trivial, it's like taking the training-wheels off the uncoupled components, and the coupled system should satisfy a set of key checks and benchmarks.

'Taking off the training wheels' is a good analogy. There can be unanticipated consequences when running the coupled system, even under prescribed atmospheric CO2, and we felt it was important that these be documented. Our challenges with land carbon conservation had not been encountered in standalone simulations run with observed meteorology. Likewise, there has been a noticeable degradation of some aspects of our ocean carbon climatology compared to an ocean only simulation (which we will show more explicitly in our revised paper). The choice we made to focus our land carbon analysis on prescribed vs prognostic LAI was, in a sense, taking one training wheel off at a time. It might be assumed in a prescribed atmospheric CO2 simulation that the carbon cycle has no impact on the climate simulation. We wanted to test that assumption by comparing the prescribed LAI case which has no climate interaction with the prognostic LAI case where some modification to climate occurs. When computing resources are limited, this is useful information for determining if we need to run both climate-only and earth-system model versions in the future.

Considering the size of the type of models presented here (in terms of number of processes represented, amount of code, computational resources required, etc.), a *comprehensive description of such a model is impossible and model testing is a major task that can fill a books. From the perspective of a reviewer, it is thus impossible to really judge on the science that goes into this model, let alone to reproduce results (although GMD requires open-access and the possibility for reviewers to replicate results). Nevertheless, an transparent overview should be provided and key tests and benchmarks should be satisfied. As noted by the authors, this model should be used for future model intercomparison projects, like CMIP6, and the present paper should demonstrate that the model is up to this task.*

We agree that it has been a challenging task to provide sufficient model description and evaluation to adequately document the model. For this reason we chose to split our model evaluation across ‘Part 1’ and ‘Part 2’ papers, focussing on the pre-industrial and historical period respectively. We apologise that delays in getting Part 2 submitted may have made this paper more difficult to assess.

First, I would like to define the challenge better. What does a coupled ESM model have to satisfy and be able to predict? The setup chosen here is to simulate the coupled Earth System, with a focus on climate, ocean circulation, and the C cycle, under constant preindustrial conditions. Given this, the system should equilibrate, i.e. gross C exchange fluxes between atmosphere, ocean, and land may persist, but the net fluxes should attain zero (no model drift). However, a non-zero net flux e.g. into ocean sediments may persist over longer time scales. It has to be acknowledged that limiting computational resources may inhibit a perfect equilibration, this is common also for other ESMs. But even more crucially, mass should be conserved within the system. E.g., the total amount of C present in ocean, plus atmosphere, plus land should be constant.

We deliberately chose this ‘simplest’ case to present here in order to document how our model behaved in respect of carbon conservation and equilibration, alongside describing our carbon climatology. We agree that C conservation and equilibration to zero are important tests but they are not necessarily easy to ensure in the coupled system.

I have major concerns about whether the ACCESS-ESM1 is ready for publication regarding these aspects. Both equilibration and mass conservation are not satisfied here, as the authors note on several occasions. Even after 1000 yr, land still emits 0.4 PgC/yr (ProgLAI case). Similarly, the ocean outgassing after 1000 yr is 0.6 PgC/yr. This is on the order of one fourth of the present-day global net flux in from these respective reservoirs.

Actually the land emission is only 0.14 PgC/yr in the ProgLAI case (but 0.4 PgC/yr in the PresLAI case). As we note in the manuscript, the non-zero land emission is largely accounted for (0.33 out of 0.40 PgC/y for PresLAI, 0.09 out of 0.14 PgC/y for ProgLAI) by tiles that are non-conserving. These are restricted to relatively small geographical regions (Figure 1), in India and in eastern tropical South America (and wetland tiles for PresLAI). This figure will be made available as supplementary information. In a prescribed atmospheric CO2 simulation, as used here, land carbon fluxes are diagnostic only, and have no impact on the simulation of carbon fluxes from any other location. Hence there is no reason for the simulation as a whole to be significantly degraded by these regional errors. It is also worth noting that even if carbon had been conserved in these regions, the low rainfall available to support plant growth would still result in the gross carbon fluxes tending towards zero as in our current work. Thus these regions are unlikely to provide good estimates of land carbon fluxes, in part due to rainfall biases in the physical model over which we have no direct control.

The net air-sea flux is slowly decreasing as shown by the expanded plot of the last 100 years of the simulation (Figure 2). To reach steady-state will take many thousand of years. The large bias in the simulation reflects the low surface alkalinity of the ocean model, which is partially attributed to a low surface salinity (a common feature of coupled models, see Fig 12: Taylor diagram in the paper) and to excessive export of calcium carbonate. While we could increase the surface alkalinity to make the net flux nearly zero the model will continue to drift. Note the magnitude of the outgassing is comparable to other CMIP5 simulations of the pre-industrial.

The other major concerns I have is whether the model is even tested for what it is supposed to provide. Coupled ESMs are used to quantify climate-carbon cycle feedbacks and predict atmospheric CO2 under future CO2 emission (and climate) trajectories. This is a notable difference to providing climate projections given atmospheric CO2. A coupled model should be able, after equilibration and under constant boundary conditions (radiative forcing from other agents, solar radiation), to simulate constant atmospheric CO2 concentrations, with gross exchange fluxes between the atmosphere land and ocean (GPP on land and in the ocean) being broadly consistent with observations. This is not tested here. Atmospheric CO2 concentrations are prescribed in both simulations. I argue that this is not a sufficient setup for a test of a coupled ESM.

Coupled ESMs are being routinely used (e.g. CMIP5) in both concentration-driven and emissions-driven configurations, the former focussed towards the diagnosis of carbon fluxes and the latter to carbon-climate feedbacks. Both uses are valid and we will now note this in our introduction. The analysis presented here of the simpler concentration-driven case has shown that our model would not perform realistically in an emissions-driven configuration without the application of a carbon flux correction. We will make this clear in the conclusions of our paper. The inability of the ACCESS-ESM1 model to perform emissions-driven simulations without carbon flux correction, does not negate the model being useful for carbon flux diagnosis in concentration-driven cases across historical and future periods. For this reason, documentation of ACCESS-ESM1 in its current form remains important.

I acknowledge that a balance has to be found between depth and conciseness in the assessment that can be handled under limited resources and published as a GMD paper. I also acknowledge that such a model development is always work-in-progress but should still be publishable. However, I am not convinced that the work in progress presented here has yet reached a state from where it can be taken further (e.g., by adding complexity, as noted by the authors).

We will rewrite our conclusions to be clearer about what the model is suitable for in

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its current form, what needs to be improved to allow for a wider range of studies and where we are limited by our underlying physical model. We agree that there is little value in adding complexity to the model until we have dealt with some of the more basic issues that this study has uncovered.

*I am listing a set of variables/aspects that may be addressed by a coupled model under constant boundary conditions and assessed *by comparison agains observations*. This is a suggestion for a next round of review. Some of these are already addressed (e.g. meridional overturning) but in many instances only as a comparison to a previous model version is given and not to observations.*

We acknowledge that our manuscript was potentially confusing because different comparisons were made for different parts of the simulation: the physical climate was compared to a previous model version, land carbon was compared between two different ACCESS-ESM1 configurations while ocean carbon was compared with observations and other CMIP5 models. In our revised manuscript we will be clearer about the scope of this paper and that of Part 2; Ziehn et al. (where land carbon is assessed against observations and other CMIP5 models). We will also move material related to different versions of the physical model (ACCESS1.3/ACCESS1.4) to an appendix so that the main body of the text is focussed more clearly on ACCESS-ESM1. For the ocean, we have added a number of additional figures (as suggested) to enable a better assessment of the simulation by comparing to observations.

- mass conservation (C in different components, salinity, alkalinity, other ocean tracers)

The model is mass conserving in the ocean for alkalinity and phosphate. There is a net loss of carbon from the ocean, accounted for by the carbon flux into the atmosphere, but the magnitude is similar in magnitude to other CMIP5 models.

- equilibration of pools in an emission-driven simulation

We have performed tests of emission-driven simulations by including a flux correction to account for the non-zero fluxes diagnosed from the concentration-driven simulation and can achieve relatively stable integrations. We have not performed an emissions-driven case without flux correction because we know this would give unrealistic drifts in atmospheric CO2 and consequent trends in carbon fluxes.

- gross CO2 exchange fluxes, predicted vs. observed/estimated

For land, different aspects of the gross CO2 exchange fluxes are presented in both parts of the study, with comparison against observations and other CMIP5 models predominantly in Part 2 since this evaluates the model behaviour under present-day conditions for which observations (or observation-based products) are available. Different aspects of the sea-air CO2 fluxes are assessed in both this and the companion paper.

- temperature fields (land surface and SST)

A summary statistic for SST is already available in Fig 12. Figure 2 will be replaced by the spatial distribution of land surface temperature difference between the progLAI and presLAI simulations with the original Figure 2 being moved to an appendix.

- surface albedo

We do not think there is a clear need to show surface albedo in this paper, given other priorities.

- sea ice cover - meridional overturning

For assessing the model we have added figures for sea-ice area cover, mixed layer depth, and zonal averaged ocean sections of DIC, ALK, phosphate and oxygen.

- vegetation and soil C distribution and total pool sizes

These are compared with observations in Part 2 (Ziehn et al.).

- CO2 seasonality at different locations where observations are available (This is kind of

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an ultimate test, and is technically possible given that the UM model, in emission-driven setup, transports CO2 through the atmosphere and that the land model simulates NEE across space.)

We have modified the atmospheric model so that even in a concentration-driven case we simulate the contribution of the land and ocean carbon fluxes to atmospheric CO2 as separate passive tracers. We present the resulting atmospheric CO2 seasonality in Part 2 (Ziehn et al.) and show that it compares reasonably well to observations.

- other "standard" benchmarks (Benchmarking of coupled models has been a high priority for years now and helpful tools are freely available. See e.g., ESMValTool by Eyring et al., 2015, GMD).

We are certainly interested in making use of ESMValTool and/or iLamb in future but currently do not have sufficient time to explore those options.

- Open access: Not satisfied, in that the model code used to produce the results presented here is not available. Code for individual components may be accessed, but not for all components. Some links provided are inactive.

The inactive link is for the MOM code. This will be corrected.

- Provide a very general description of some model characteristics, deficiencies and limitations (e.g. prescribed phenology, fixed N fixation and P weathering), to give at least a feeling for what the model can and cannot do. The balance between generality and detail in section 2 is not appropriate. E.g., Eq. 1 and 2 are unnecessary. It is ok to refer to other publications where these components are described.

We will revise section 2, taking these comments into account. Most of the physical model detail (Section 2.1), which is not directly relevant to the carbon simulation, will be moved to the appendix. Equations 1 and 2 will be removed. We will revise the conclusions to try to provide a clearer summary of the model characteristics, deficiencies and limitations.

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- It’s ok not to evaluate individual components in depth, but then the coupled system must be working ok (see above).

Given some of the issues identified in e.g. ocean NPP, we now present some ocean only simulations as a means of assessing how aspects of the simulation are degraded in the coupled system.

I hope my critical review helps to improve this manuscript. In many instances, the material and results are already available and a presentation with a focus on the most important aspects of what a coupled ESM should be able to simulate would much improve the present manuscript. This would lead to a convincing and transparent presentation of key features and variables, e.g. some of the ones I have listed above.

Thanks for the review and we have tried to fully address your comments.

SPECIFIC POINTS

- In abstract, it needs to be made clear what type of simulations are used for evaluation (forcing? emission/concentration-driven?) and against what it is evaluated.

This information will be added to the abstract.

- In abstract, refer to model components presented in Fig 1.

It is not clear to us whether the reviewer would like the model components explicitly mentioned in the abstract (i.e. UM7.3/CABLE/MOM4p1/WOMBAT etc) or whether the reviewer wishes Fig 1 to be referenced from the abstract. Given that it would be unusual to refer to a figure from the abstract, we will add the component model names.

- Introduction puts strong emphasis on climate-carbon cycle feedbacks. However, the paper does not address feedbacks (constant atmospheric CO2).

Agree, and we will re-write the introduction to highlight the usefulness of both concentration-driven and emissions-driven simulations. We will also be clearer about why the concentration driven case is presented here.

- Many different setups may be chosen for comparison of effects. It remains unclear why prescribed and interactive LAI are given such an emphasis.

As noted above, this case was chosen because one configuration does not change the climate and the other does. This would be clearer if we had described our reasoning earlier in the paper. It will now be included towards the end of the introduction.

- Description of model configuration not sufficient: concentration of GHGs, albedo, areosol, solar radiation, other radiative forcing to drive simulation?

This information will be added to the model configuration section and if required a new subsection describing input files for the atmosphere will be added. For the physical model, the configuration generally follows that used for the ACCESS1.3 CMIP5 submission (except background stratospheric volcanic forcing) and it may be appropriate to explicitly reference relevant documentation of those simulations.

- To initialise the model prescribed observational DIC and Alk are used or variable values are "taken from identical test simulations", and no spin-up is done âĂŤ> how does this work? Identical test simulations with 100% identical setup?

Ocean Oxygen, Phosphate, DIC and Alk fields were initialized based on observations and a control run was run for 1000 years before starting the historical simulation. This will be clarified.

- C conservation in land C cycle: Why not 100bug in the code? Numerical precision not sufficient? Or is this linked with the fact that CABLE does not simulate land C loss from disturbance (see p. 8071, l.1)? (this confused me anyway...)

It is an inconsistency in the code in circumstances where the leaf carbon pool gets smaller than a minimum LAI value prescribed for each pft. This allows leaf respiration to exceed GPP which is unrealistic. Further explanation will be provided in the manuscript.

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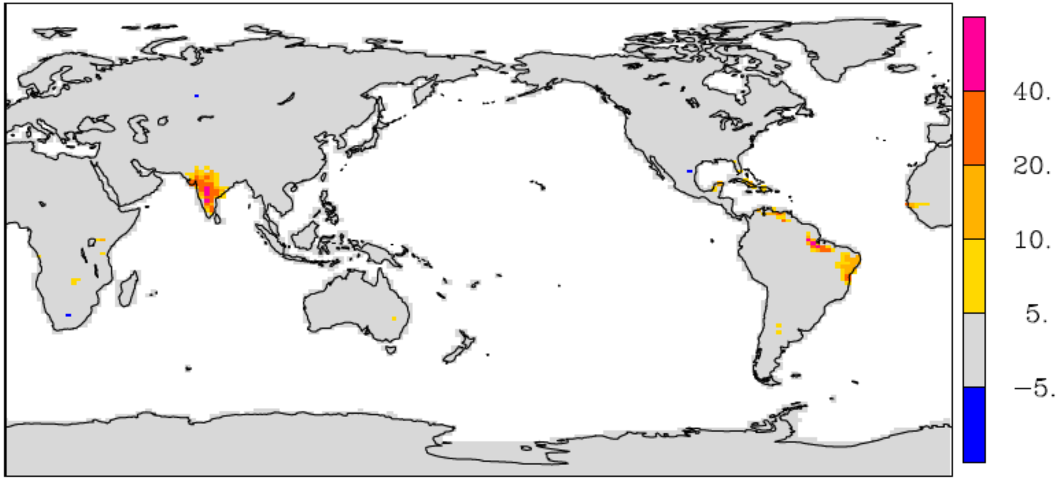

Fig. 1. Year 501-1000 mean net ecosystem exchange (NEE) in gC/m2/yr

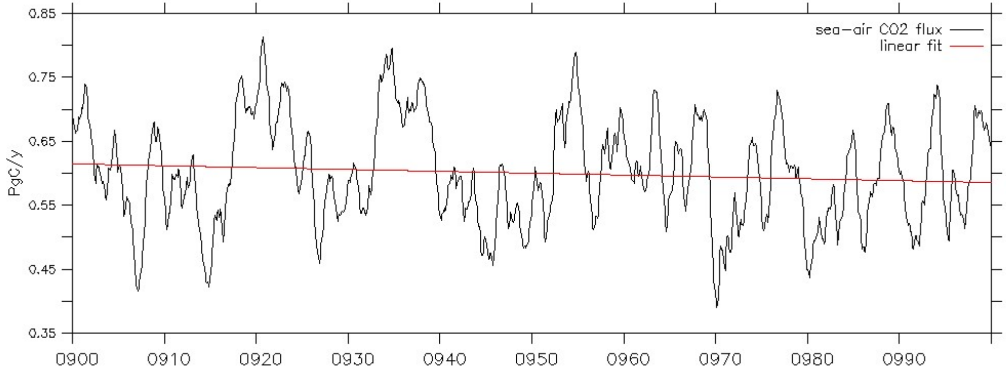

Fig. 2. Sea-air CO2 flux from year 900-1000 (black) and linear fit (red)