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Geoscientific Model Development Discussions

Interactive comment on "The carbon cycle in the Australian Community Climate and Earth System Simulator (ACCESS-ESM1). 2. Historical simulations" *by* Tilo Ziehn et al.

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Review of "The carbon cycle in the Australian Community Climate and Earth System Simulator (ACCESS-ESM1). 2. Historical simulations", by T Ziehn et al.

This is a partner manuscript (part 2) which together with Law et al Part-1 document and describe the configuration and performance of the ACCESS Earth System model. My remit clearly is to review this paper, but I make a couple of comments which probably span both papers. This is exactly (in my view) the ideal paper for GMD and the authors deserve credit for a thorough job.

Overall I find them to be a comprehensive and well written pair of papers and that the

model is clearly well formulated and calibrated and performs well in both pre-industrial and historical simulations. There would be no problem at all in including this model in multi-model comparisons with other CMIP5 models. The two main issues I would encourage the authors to address in the run-up to CMIP6 are to do with spin-up and land-use - and I know both of these are in progress. Neither require major changes to the paper but could be mentioned (the latter is covered well actually but the former needs more discussion I think - see detailed comments below).

Otherwise I have only very minor comments that may improve clarity and also a few suggestions for other evaluation activities the authors may consider, but are not required for publication of this paper.

Chris Jones.

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Major comments

1. My main concern with this model is the length of time taken to achieve a spun-up state. Law et al document this nicely, but I think it requires more discussion in this paper too what the implications are. The drifts in carbon stores are still non-negligible even after 800 years of spin-up (your start point here). I think this should be laid out explicitly before the analysis starts. You do, in the land carbon section, acknowledge this and subtract the control run drift. But unless a reader has been through Law et al they would not know how big this drift is. For the ocean it is more important still, and the ocean section does not mention this at all. The drift of circa 0.7 GtC/yr (figure 11a in Law et al) is of similar magnitude to your historical fluxes (I assume these are corrected for the drift). If a reader hadn't seen that figure then they would not realise from this paper the size of drift being subtracted.

In CONCENTRATION-driven runs like this, you can of course force the correct CO2 and correct for the drift after the simulation. But that is not possible in an EMISSIONdriven run, and such a drift would cause a massive drift in atmospheric CO2 rendering an emissions-driven historical run meaningless. This would, at present preclude use of this model in C4MIP for example which would be a great shame. The latest C4MIP protocol (Jones et al 2016, GMDD - CMIP special issue) recommends a maximum acceptable drift of 10 GtC per century. I therefore throughly recommend that ACCESS modellers attempt to find accelerated means to derive a spun-up state in time for CMIP6. There are numerous options, such as running offline (for either land or ocean), or using reduced turn-over time techniques as per Koven et al for CLM (http://www.biogeosciences.net/10/7109/2013/bg-10-7109-2013.pdf).

as a final word on this, lack of carbon conservation would also be more of an issue for E-driven runs.

2. My second concern is the lack of land-use change as a forcing. You already know this, and acknowledge it in the paper, so no revisions to the manuscript are required, but I just take this opportunity to stress that simulations of contemporary and future climate/carbon cycle are very much reduced in usefulness if they lack the very large land-use forcing of the land carbon cycle. Implementing this for CMIP6 is also therefore a priority I would say.

Minor comments

1. Having quickly read Law et al before I reviewed this one I was struck that there was not an evaluation there of (land) carbon stocks. I do feel that the land carbon modelling community have become fixated on evaluation against fluxes to the detriment of stocks and residence times. This is beginning to change and I was pleased to see some discussion of carbon stocks in this paper. It would be nice to see the time changes in these though as well - could table 2 be extended to show pre-industrial and present day stocks? In the discussion on biomass you mention that your results are higher than observational based estimates - but of course you lack land-use change as a driver. So it could easily be expected that your biomass is of the order 100-150 GtC too high

due to this. If you masked for present day agricultural regions you would probably get a much closer fit to expected global totals. So your simulation is actually not bad.

2. Abstract - you can say that aerosol forcing is large or larger than other models. But don't say "over-sensitive" as we simply don't know. Maybe this is correct. . .

3. section 2.3 - I like the comparison vs CMIP5 models. This is a nice way to put model-data discrepancies in context. But why do you only include 5 CMIP5 models (counting the 2 IPSLs as one model). Anav et al used more than twice that - any reason not to use the full set?

4. in a couple of places (e.g. start sec 3.1) you mention the variability of the land sink and/or the atmospheric CO2. You could go further and use this as an evaluation metric. Both Cadule et al (2010, GBC) and Cox et al (2013, Nature) show the power of the C-cycle sensitivity to ENSO on inter-annual timescales as a really strong evaluation metric and constraint on the sensitivity of ESMs.

5. p.7, line 21. I don't understand why you attribute your slower warming to the initial warm bias - how do you know the warm bias causes this? Could just be under-sensitive SSTs not related to a bias.

6. p.8 line 3. I was amused to see that having an error different from other models was "encouraging"! not sure why! Can you say why it is better to have the opposite MLD bias from other models?

7. in general I thought this MLD section (3.2) was a bit superficial - can you be a bit more quantitative in your comparison and description? the figure shows the data but it can be hard to tell from there if the differences you describe are of the order of a few % or 10s of % or factors of 2 or more or what? And can you mention your confidence in the ons? presumably global maps of MLD are not directly observed but must have certain extrapolation uncertainties and so on. Are some areas/seasons of the oceans better sampled than others etc. . .

8. p. 8 line 21. Can you define what you mean by IAV. Interannual Variability I know, but how do you turn this into a number? is it the standard deviation of a time series of annual means? in which case is the time series de-trended first? etc

9. p.9 When discussing historical changes in land carbon can you split into veg and soil changes (e.g. put in table 2). You could compare directly with the 2 models in Jones et al (2013) which also don't have land-use forcing (dashed lines in figure 2). You could probably also compare with model results from detection-and-attribution studies which ran with/without certain forcings. The are probably various no-land-use runs to look at.

10. Sec 4.2. Despite the large drift your historical ocean sink does look a very close match to the obs. Can you also quote a cumulative uptake here?

11. sec 5.1.2. I was curious to see that your prescribed LAI didn't match your observations. Can you explain why not? You illy that this is because there are differences between observed datasets - which is of course true. But then why do you choose one dataset to prescribe LAI to the model, and then a different one to evaluate against? If one is better than the other can you use it for both?

12. p.13 discussion of different carbon stocks for the two configurations. Given both have very similar GPP, the large difference in biomass is presumably due to residence times? Having GPP further north in compared to the tropics means that for the same global GPP you have a higher biomass? You might consider next time an evaluation of turnover times - e.g. as per Carvalhais et al (2014, Nature).

13. section 5.2.1 / 5.2.2. Can you swap the order of figures 12 and 13? you describe 13 first.

14. sec 5.2.3. You compare global totals between model and GLODAP, but the map shows missing areas in the ons dataset. So should you first mask out the model to match the same area before you compare totals? (or quote a full AND a masked number for the model). The values in the Arctic for example are very high but missing

in the obs (although of course the area is smaller than this projection makes it look).

15. sec 5.3. It seems reasonable that the land drives most of the seasonal cycle at your ons stations except the south pole. I like the way you have labelled the land and ocean CO2 separately to be able to diagnose this. But I couldn't work out why the contribution of the ocean at the south pole looks different for your two LAI configurations. If I read the figure correctly then the role of the ocean is given by the blue solid minus the blue dashed line and the red solid minus the red dashed line. These are quite different by eye - e.g. the blue lines are quite far apart in December and the red lines quite close. So why does your LAI treatment affect your December ocean fluxes so much?

16. Table 2. Can you add a final row at the bottom of "total"

17. general comment on figures - maybe a personal preference but please can you add a legend so that I can easily read which line is which. The text in the captions is very good - but I have to read a whole paragraph to spot what the red/blue lines are. Would be great to simply see a legend with this in as well as the detail in the caption text.

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