

**Review:** The carbon cycle in the Australian Community Climate and Earth System Simulator (ACCESS-ESM1). 2. Historical simulations.

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**General Remarks:**

This paper evaluates the carbon cycle in ACCESS-ESM1, comparing historical simulations against both CMIP-5 model results and observations. The evaluations presented in this paper highlight both strengths and weaknesses in the ACCESS-ESM1 carbon cycle. The comparisons displayed and discussed document the baseline behavior of ACCESS-ESM1, which is essential for model justification needed by future studies utilizing this tool. Overall, this paper accomplishes its goal of showing that the ACCESS-ESM1 is a useful tool in exploring the carbon cycle, including both land and oceanic fluxes and carbon storage.

This paper contributes to modeling science by evaluating and documenting the carbon cycle modeling capabilities in ACCESS-ESM1. In general, this study is structured well and presented in a clear fashion, and I found the description to be sufficient to satisfy the reproducibility requirements. While the scientific approach and applied methods are valid, I found the text to be too subjective rather than quantitative. Instead of using generalized, generic comments on the model performance, I feel that this paper requires more scientific and statistical-based evaluations in many of the presented comparisons. Modifications to include more direct, quantitative and supportable statements would not only improve the scientific quality of the text, but would also strengthen the validity of the model evaluation. Despite this criticism, I feel this study is suitable for publication in Geoscientific Model Development with revisions to address this concern.

**Specific Comments:**

The main focus of these specific comments is to highlight text that could benefit from being more quantitative rather than subjective. In addition, there are some specific suggestions for clarifications aimed at improving the overall flow and clarity of the manuscript. Before starting the specific suggestions, one other general remark I have is that the figure axes and color bar labels are very small and hard to read.

**Abstract**

The abstract provides an overall view of how well ACCESS-ESM1 performs; however, they provide this information using terms such as “good” and “performs well”. How well? Good according to whom? Is your good the same as my good? First off, the authors state ACCESS-ESM1 overestimates the seasonal amplitude of LAI, but do not attach any quantities to this statement. Rather than providing a quantitative assessment of this statement, the reader is left to wonder if this is a substantial bias, perhaps even being prohibitive of using this model in the future, or merely a relatively minor difference that is offset by other positive features. The paper then continues with the statement that the oceanic and land fluxes “show good agreement with the observations”, but again, no metric is used. How big are the differences, what is the error, or how closely correlated, both spatially and temporally, are they to observations and/or other CMIP-5 models? In the last overview comparison, the authors state that the seasonal cycle is “close to the observed seasonal cycle”, but as the reader I have to wonder how close is close? I believe that putting quantifiable

metrics on at least some of these statements will strengthen the concluding remark that ACCESS-ESM1 is indeed a useful tool.

## **Observations**

The model evaluation (later in the text) proceeds through a straightforward succession of comparisons; however, the order of these comparisons is different than the order the data is listed in. For clarity and consistency, it would be an easy fix to reorganize this section to present the data in the order that they are used.

### **Land Temperature and Precipitation**

This section presents a time-series comparison of temperature and precipitation, but contains very little quantitative statements. For example, the temperature anomalies are “close to the observed anomalies through most of the period”. Again I have to wonder, how close is close? Perhaps a difference plot showing the errors would be useful? Or perhaps a correlation coefficient that may or may not be significant? Some sort of metric on this statement would be much more enlightening to the reader. For example, as I look at Fig. 1 myself, I see the author’s point that ACCESS-ESM1 is lower than the observations 1965-2005; however, to my eye I also see the model looks quite a bit lower in the 1940s. I think that a difference plot would help identify these areas, rather than relying on the reader to have to assess the differences between the models and the observations on eyesight alone. Another example later in this section occurs in the precipitation anomaly discussion: the authors state the differences are “generally small,” but provide no values to suggest what is meant by small. This is followed by the statement that “the simulations compare well with observed rainfall anomalies until about 1950,” with no supporting metric such as minimal differences or significant correlation coefficients to back up this statement. Overall, I found that this entire section had very few quantitative comparisons, instead relying heavily on subjective terminology, making me believe that at least some measurable metrics would improve the paper.

The last paragraph in this section discusses the timing of precipitation anomalies versus volcanic eruptions. The authors point out a reduction in precipitation following eruptions, with the one exception of El Chichon; however, when I looked at the time-series, I did not see a decrease after the Santa Maria eruption in addition to El Chichon. In fact, looking at the time-series, the decrease after volcanic eruptions did not stand out to me, especially when the authors note the reduction following the Mt. Pinatubo eruption, which does stand out, is too far away from the eruption date to be related. I think a more quantifiable analysis of the magnitude and the timing of this decrease, in days or weeks or some stated time-scale, would be helpful.

### **Sea Surface Temperature and Mixed Layer Depth**

The second full paragraph of this section discusses spatial patterns of sea surface temperatures, except no time period for the analysis is provided, not even in the caption for Figure 3. Are these differences over the entire simulation or a selected time period? Shifting to the text, the authors state that ACCESS-ESM1 “produces very heterogeneous differences from observations.” Reading this, it was unclear to me what was meant. When I turned to look at the figure I expected to see random errors; however, in my opinion the differences are spatially coherent in latitude bands. Then in this same discussion they state “there do not appear to be strong seasonal biases,” but with this terminology I have to wonder are there or aren’t there? Then they state the exception of the North Atlantic, which has a coherent bias towards cooler temperatures, but to me it looks like this is a year-round bias more than a seasonal bias. It perhaps does vary in magnitude with season, being a larger bias in August, but it looks like the sea surface temperature here is underestimated year round. Further looking at Figure 3, I also see a flip-flop in errors in the southeastern Pacific Ocean,

with positive differences in August and negative differences in February. To me, having differences that vary with time of year makes it seasonal, but this region is not mentioned. I think this section could benefit from more careful wording and analysis.

The ocean mixed layer depth discussion could also benefit from a more quantitative analysis, rather than using statements like “appears to slightly overestimate the depth in winter” and “appears to underestimate the depths in summer,” with no statistical support for these subjective comments. Also, in Figure 4 in this section, the caption states that differences are shown, suggesting a difference plot; however, the figures just show the results from both the model and the observations. I would suggest rewording the caption to avoid confusion.

### **Land Carbon Response**

I think it would be helpful to include the MODIS/AVHRR LAI data in Figure 5b. I realize this would only be for the last few years of the simulation, but the benefit is that it would provide a reference to the simulated LAI values. After reading it through, I discovered there is a section on LAI where it is discussed in more detail, but this figure comes first, and when I read it I was wondering how it compared.

I found the discussion in the last two paragraphs of this section confusing. I loved to see values and uncertainties; however, it was unclear to me what values were comparable. The section starts with a discussion on carbon uptake, which at first I assumed represents total uptake, or GPP. But from what I could tell, the same values provided for ACCESS-ESM1 were then used in the following paragraph, which talks about NEP. I read the section several times, but only found the one value of 154 Pg C for prognostic ACCESS-ESM1. I was then confused when it was stated that this value is “at the low end of the CMIP5 range,” when that range is estimated to be from -59 to 18 PgC according to Shao et al. (when outliers are not included) or from -124 to 50 PgC from Jones et al. Based on these numbers, doesn’t ACCESS-ESM1 take up and store much more carbon (+154 Pg C) than the CMIP5 models? It doesn’t help that the signs make this analysis even more confusing. Since NEP and uptake were discussed, I assumed a negative value was a source of carbon. I know this confusion on signs is difficult to handle, but I just wanted to raise awareness that it contributed to making this section difficult to follow. I apologize if I got these comparisons incorrect, but that indicates that more careful discussion and use of terminology would be helpful.

### **Ocean Response**

I thought the second half of the discussion in this section was clear and informative; however, I had a two questions on Figure 6. First off, the caption states that it’s “Integrated Primary Production,” but doesn’t define what that is. I assume that’s the same as NPP? I’m unfamiliar with that terminology, so it might be worth clarification. Second, the values in the text and the figures don’t match up. In the text the global mean ACCESS-ESM1 NPP is 46 PgC/yr, but from what I can tell this must be for the entire simulation. This is then compared to SeaWiFS value of 52 PgC/yr for 1998-2005. Upon reading this, I expect ACCESS-ESM1 to be lower than SeaWiFS in Figure 6; however, looking at it, ACCESS-ESM1 is higher than SeaWiFS for these years. I personally think it would be better if these comparisons were values representing the same time period, both to have a fair comparison and to match up the values with what is seen in Figure 6.

### **GPP**

I would consider removing the first sentence in the second paragraph, as it is subjective and is not needed with the supporting text. I would then combine the first and second paragraphs to have a complete discussion. I would also remove or modify the final sentence in the second paragraph that states that containing nitrogen and phosphorous “ensures a more realistic simulation.” While I

think there is evidence that including nitrogen and phosphorous is beneficial in many circumstances, that alone does not ensure a model outperforms one where these are not included but instead has more realistic representations of other important processes.

The second section in this section discussing the mean annual cycle of GPP is again quite subjective, and perhaps it wouldn't be too difficult to provide a few quantitative statements.

For the final discussion in the section on IAV, first off I was wondering how IAV is calculated? I can think of a couple of different methods, and it is not defined how they actually calculate the values that are shown. For this section, I suggest a PDF of errors, therefore when you say there is good agreement in the spatial pattern, it can be backed up with "x% of the globe has errors less than x kg C/m<sup>2</sup>." I will also note that the labels in Figure 9 were particularly hard to read.

### **CNP Pool Sizes**

The discussion on the HWSD and soil carbon I found to be quite subjective, again focusing on the comparisons being "good" or "generally good."

For the nitrogen comparison, first off please clarify what the value reported, 85 Pg N, represents (i.e. global over entire simulation?) And just a thought: it might be interesting to show or state how this evolves in time, similar to the time-series shown for carbon.

For the phosphorus discussion, I would suggest either removing the "slightly" modifier used in the discussion on how the model results are lower than the estimated range, or give references on how this is smaller than previous modeling estimates to give a frame of reference for that adverb.

### **Ocean Carbon**

The figures in this section are out of order. I would recommend swapping Figures 12 and 13, rather than discussing Figure 13 first and then going back to 12.

### **Ocean NPP**

This section shows and discusses mean seasonal cycles; however, after discussing seasonal aspects such as shifts in timing, the section ends with the disclaimer that the timing cannot be compared. I'm wondering, if that's the case, might it be better to just look at amplitudes in a different format, such as a table or bar graph? If you leave the figures, I would recommend moving this statement to the beginning of the discussion and making it clear the timing aspects are only being compared between ACCESS-ESM1 and CMIP5. With the figure as it is currently, I again found the accompanying text subjective, which might also support looking into a table or bar graph format for this section to provide more quantitative comparisons.

I also want to note on Figure 13 the Southern Ocean has different values on the x-axis, which was not mentioned. This was confusing because at first it appears the Southern Ocean does not have a seasonal cycle, but from what I can tell (which is difficult given the large range), the amplitude is comparable to the other oceanic regions.

### **Ocean Sea-Air CO<sub>2</sub> Fluxes**

In this discussion, the authors state that the Southern subtropical gyres overestimate the observed sea-air flux; however, when I look at the figure, it looks to me the biggest uptake is occurring on the coastlines. I personally would like more of a mention of this, and maybe a discussion on why coastlines, particularly off the west coast of Australia, are taking up so much carbon compared to the broader ocean gyres.

In the second paragraph of this section, the second sentence beginning with “Furthermore it appears that globally....” doesn’t make sense to me, as I don’t understand what it is that lies outside the range. Usually regional analyses reveal why global results occur, and it looks like the Southern Hemisphere is the main contributor to the ACCESS-ESM1 global seasonal flux anomaly? The text does go on to state this, so you may just consider removing or modifying that sentence. Later in that same paragraph, the text states that the Northern Hemisphere has fluxes larger than observed but within the range of CMIP5; however, to me it looks like there are several months when ACCESS-ESM1 is outside the range (i.e. Jan, Feb, May). Since the same disclaimer was put on this paragraph (which again I would suggest moving to the beginning of the paragraph if these figures are kept), I would consider a different format, such as a table or bar graph, or even keeping the seasonal figures but not showing the seasonal cycle in the observations if you don’t believe them, and instead using a solid line.

### **Anthropogenic Inventory**

The discussion is again quite subjective. Also, in Figure 15, there is a reference to Key et al. (2004) that is confusing and not in the text (I’m unsure if the figure appears in both papers?) Upon finishing the paper, I see the reference in the conclusions, but you might want to somehow clarify or include it in the text during this discussion.

### **Atmospheric CO2**

I think it would be better to combine the first paragraph with the next sentence following it (starting “Therefore, our...”).

### **Conclusions**

I think the conclusions would be stronger if some quantitative assessments were provided. I also noticed the order of the conclusions was not the same as the order presented in the text, but neither was it combined in a more succinct fashion to group conclusions. Simply reordering the paragraphs would make the text more consistent.

In the second-to-last paragraph, I was confused by the statement that “Seasonally the ACCESS-ESM1 appears biased toward the Southern Hemisphere.” I’m not sure how to interpret this statement, as to me it’s using both temporal and spatial references combined in an unclear fashion. This same type of statement occurs later in the sentence “Globally the annual mean is well captured but biased to low latitudes.” I recommend reworking those sentences to clarify the intent of the text. Also, a quantification of how it is “well captured” would strengthen the statement.

### **Technical Comments:**

Overall I have very few technical comments. My main technical comments are on comma usage. I personally find well-placed commas can aid in reading the text, but am not a comma expert, so feel free to take or leave the suggestions.

### **Abstract**

Line 8: “reproduced; however, “  
Line 9: “anthropogenic aerosols, “

### **Introduction**

Line 19: “resolutions, and”  
Line 21: remove comma “processes through”

Page 2, Line 7: "carbon cycle, an overestimation"  
Page 2, Line 18: "soil carbon, especially in the northern latitudes, "  
Page 2, Line 21: "However, "  
Page 2, Line 28: "CMIP5, but in a"  
Page 2, Line 31: "First, we briefly"

### **Model Configuration, Simulations and Comparison Data**

Page 3, Line 12: "Wang et al., 2011), which"  
Page 3, Line 14: "coupled, and"  
Page 3, Line 16: "Consequently, global"  
Page 4, Line 5: "negligible, and"  
Page 4, Line 22: "simulations, we focused"  
Page 4, Line 31: add a space "*precipitation*: Climate Research"

### **ACCESS-ESM1 Climatology**

Page 6, Line 2: add an s "external forcings"  
Page 7, Line 1: "lower albedo, especially"  
Page 7, Line 2: "hemisphere, and consequently"  
Page 7, Line 32: "Interestingly, "

### **ACCESS-ESM1 Carbon Cycle Response**

Page 9, Line 16: check the text surrounding the reference, either putting the reference in parenthesis or modifying the text

### **Evaluation of the Present Day Carbon Cycle**

Page 13, Line 14: remove the s "encouraging result."  
(Just as a note, personally I would change that statement to something less subjective such as it falls within the observed range.)  
Page 13, Line 26: "Table 2, and"  
Page 14, Line 14: "(ACCESS-ESM1), respectively"  
Page 14, Line 17: add a space "2 months."  
Page 16, Line 5: change from is "fluxes are not well captured"  
Page 16, Line 23: "well reproduced, with"  
Page 16, Line 7: "As observed, the amplitude"

### **Conclusions**

Page 16, Line 22: remove comma "hemisphere and underestimates"  
Page 16, Line 29: remove by "is about 20%"  
Page 16, Line 31: "globally, therefore increasing NEE."  
Page 17, Line 4: "into tracers, which"