Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-129-AC2, 2017

Geoscientific Model Development Discussions
Open Access
EGU

Interactive comment on "The Path to CAM6: Coupled Simulations with CAM5.4 and CAM5.5" *by* Peter A. Bogenschutz et al.

Peter A. Bogenschutz et al.

bogenschutz1@llnl.gov

Received and published: 31 October 2017

We would like to thank anonymous referee #1 for volunteering their time to review this manuscript and for offering suggestions to make it better. Please see responses to each specific comment below:

1) Only the atmosphere model is described in the section 2 for model description. Given that this work is to document the coupled simulations, it is useful to also briefly describe other model components used, assuming the same are used for all the CAM configurations in this work.

Author response (AR): A new sub-section has been added entitled "Component Models" to address this.

Printer-friendly version

Discussion paper

2) Figure 1 on the preindustrial runs: CESM-CAM5.3 at year 402 is assumed to have a globally average surface temperature near the stable equilibrium of 287.0K. Why CESM-CAM5.4 and CESM-CAM5.5, which were initialized with CESM-CAM5.3 at year 402, have substantially higher global mean surface temperature? This appears inconsistent given the description in the text. Is there something missing?

AR: The initialization of CESM-CAM5.4 and CESM-CAM5.5 means the ocean state is the same as CESM-CAM5.3 as they start. Since CESM-CAM5.4 and CESM-CAM5.5 have a different atmosphere, and a slightly different energy budget, they will reach a different equilibrium temperature.

3) Page 8 line 4, it is not an accurate statement suggesting that "improvements stem from reduction in magnitude of the errors", given pattern correlation coefficient remain unchanged. From Figure 3, it can also be seen that both error magnitudes and patters change; and there exist quite regions with error magnitudes become larger.

AR: This statement has been removed and the discussion modified accordingly.

4) Figure 10 on relative AMOC strength between CESM-CAM5.3 and CESM-CAM5.5: the authors speculated that the difference in simulated surface wind stress in the north Atlantic could be the likely cause. Large difference in southern mid-latitude surface wind stress between them could be an even larger factor (e.g., Delworth and Zeng 2008). Suggest to review and revise this speculation attribution.

AR: The Delworth and Zeng 2008 paper has been cited and this has been mentioned as a possible reason for the differences in the simulated AMOC between the model configurations.

5) Figure 6 includes the diurnal composite of precipitation for the tropical Africa, but essential no description in text. Suggest to add some description for it, though the points to make can largely be reflected in the Amazon composites.

AR: Thanks for pointing out this oversight. A short discussion has been added for the

Printer-friendly version

Discussion paper

African diurnal cycle of precipitation.

6) Page 5 line 21, redundant word “that” is used.

AR: Fixed

7) Page 11, last line, given the context, “inter-annual seasonal tropical variability should be “intra-seasonal . . .”.

AR: Fixed

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-129, 2017.