

Interactive comment on “Strength and limits of transient mid to late Holocene simulations with dynamical vegetation” by Pascale Braconnot et al.

Anonymous Referee #2

Received and published: 6 December 2018

Summary:

Braconnot et al. use the IPSL Earth system model with dynamic vegetation to perform a transient simulation from the mid-Holocene to present-day. The authors begin by testing how their changes to the default IPSL model impact the mean climate state. Modifications are made to the land surface model, aerosol emissions, soil evaporation, and vegetation. Sensitivity tests show some significant changes in the mean temperature and precipitation associated with their model modifications. Surprisingly, overall biases do not change much from the default IPSL model configuration. The authors then perform different vegetation initialization experiments and discover a single equilibrium state (more or less) for the mid-Holocene but multiple equilibrium states for the preindustrial. Finally, they perform a transient simulation from the mid-Holocene to

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present-day and compare model output with vegetation records.

Although there was clearly a lot of effort put into these simulations, many of the results are muddled by poor structuring and limited analysis. In its current form, the paper is not especially satisfying from a model development perspective or a paleoclimate perspective. Given the choice of journal, I recommend modifying the structure and results to better reflect the target audience. You can leave the model development and testing sections, but you need to provide additional context and analyses.

General Comments:

The results and analyses left me unsatisfied, especially given the amount of time spent on model testing. Many findings are dismissed as beyond the scope of this paper or for future work. However, without in-depth exploration of at least some of the interesting results of the simulations, the paper feels more like a data description, which is fine, but not especially appropriate for Climate of the Past. I recommend expanding the transient simulation results and analysis, since it is the novel part of this study. There are several topics that could be explored further, such as the importance of dynamic vegetation in the transient climate response (needed for the title), the mechanisms driving multiple equilibria, and comparison with proxy reconstructions. The authors might also want to consider how do these results compare with other transient model simulation? I have not edited this manuscript for spelling and grammar. There are many instances of incorrect spelling. Please proof-read the text and figure labels before resubmission. Also, I encourage the authors to seek assistance from a very proficient or native English speaker.

Specific Comments:

Line 123: Can sea level actually change in the model?

Line 143: I do not think that this is very good justification for not thoroughly testing modifications against preindustrial climate.

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Lines 152-153: Given the importance of the aerosol responses, why do you prescribe aerosols here? Are dust and sea-salt prescribed to PI? How might this impact climate? I do not find "...we also plan to run simulations with fully interactive dust and sea-salt" good justification.

Line 163: Some of these modifications do not feel robust (e.g. the soil evaporation factor).

Lines 175-176: What is the TOA energy imbalance for these runs? This could be important since different simulations are run for different amounts of time. 0.4 Wm^{-2} is far from zero. . .

Line 181: Why does this modification impact the ocean response so dramatically? I thought the hydrologic modification should only impact the land surface. Am I missing something?

Lines 203-205: This sentence is ambiguous. What does "...might be small. . ." mean?

Line 213: "et al." for a personal communication?

Line 259: Is the carbon really in equilibrium?

Line 310: how well does 50% compare with other models?

Lines 325-326: "Since surface variables adjust rapidly, this is a way to compare the rapid adjustment to insolation

326 and the additional effect due to the dynamical vegetation (not discussed here)." Why say this then? It feels like an advertisement. . .

Line 367: Why not cite Joos?

Line 380: Do you mean JJAS? How do you account for the calendar changes?

Line 398: Interesting. . .Worth performing spectral analysis on the variability?

Lines 418-419: Why would this lead to an underestimate?

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Line 445: "...provides the feeling. . ." is not scientific language.

Line 462-463: Is this sentence necessary?

Line 476-477: Could the difference relate to spin-up procedure?

Line 480: This kind of "additional analysis" is what is necessary to make this paper valuable to the community.

Figures:

For the difference maps, please center the contour ranges on 0 and add white in the middle to more easily distinguish differences. Change months on the X-axes to years.

Figure 1: Add the PI runs to the comparison. Figure 6: Is it 1 m or 2 m air temperature?

Figure 9: Needed? Could be supplemental.

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