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Interactive comment on "Insights into the early Eocene hydrological cycle from an ensemble of atmosphere–ocean GCM simulations" *by* M. J. Carmichael et al.

Anonymous Referee #1

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This study compares aspects of the hydrological cycle and hydrological response among five general circulation models of the early Eocene and numerous experiments, and briefly compares simulated mean annual precipitation with proxy estimates. The major conclusions of the paper are that (i) a warmer Eocene supports higher precipitation/evaporation rates, (ii) climate models differ slightly in their precipitation sensitivity and more seriously in regional precipitation, (iii) models underestimate Eocene polar temperatures and precipitation rates. In general, the study is fine, if pedestrian. It provides a useful but not very creative summary of the hydrological differences between models. The disappointment of the paper is that it offers very few "insights" as advertised by the title. Most (all?) of the conclusions could have been found in previous studies. The study probably tries to do too much. A more detailed examination of specific hydrological features (precipitation sensitivity or monsoons, for example) would be more interesting and valuable.

General Comments

Model intercomparisons can be very useful, and are not done often enough in paleoclimate studies; for good reason, the number of paleoclimate modelers is relatively small and the geological record long. The problem with this model intercomparison is that it is completely descriptive: Model A has a greater global precipitation rate than model B. The utility of models is that they can be probed to understand the reason for the solutions that they give. The solution itself is not that interesting. This study provides very little insight into the mechanisms responsible for the model differences it describes. The model analyses are generally simple and descriptive, and don't explore the dynamics or physics behind the model differences.

The model boundary conditions are not described in sufficient detail. Greenhouse gases other than CO2 are generally not stated, nor are orbital parameters. These would best be summarized in a table. Differences in geography are also not described, which may be appropriate, since the paleographies appear similar (based on Fig. 3). Topography may be a different issue, though, and should be shown. What is the mean continental land elevation? To what degree can differences in topography account for regional hydrological changes? The role of topography (and other surface characteristics) on the hydrological cycle in the Paleogene has previously been considered by Sewall et al. (2000, Global and Planetary Change), Sewall and Sloan (2006, Geology) and Feng et al. (2013, American Journal of Science). Some discussion of these effects is merited, particularly in the context of model-data comparisons. The models also have very coarse resolution, which is well known to degrade the simulation of precipitation. Some discussion of this is warranted as well. Is there any connection between atmospheric model resolution and precipitation sensitivity or response?

Specific Comments/Edits

In general, the paper could use editing and streamlining. It's too long and rambles in parts.

Title. Change the title. I suggest "Comparison of the early Eocene hydrological. . ."

Abstract, line 15. "This is primarily due to elevated atmospheric paleo-CO2." As shown in the manuscript, this is due to elevated temperatures.

p. 3281, lines 1, 6, etc. Surface surface temperature and mean annual temperature are not proper names and should be not capitalized.

p. 3281, line 26-28. "This has resulted in suggestions. . ." This is an incomplete sentence. Please fix.

p. 3289, lines 12-14. What are the sources of these biases? Are they a concern for simulating the Eocene?

p. 3289. Fig. S1. This figure should be moved to the main text, as it more clearly shows extratropical model skill in simulating precipitation than absolute precipitation anomalies. Also, please include the relative anomalies for the ocean as well. Some explanation should be added to explain the very large relative biases that show up over the continents. The multimodel mean is surprisingly good, but individual models (e.g. FAMOUS) have severe problems that should be discussed.

p. 3290, lines 3-5. "EoMIP models simulate a global precipitation rate which agrees fairly well with observational data sets" I agree that this is the case. But, mean annual precipitation is not a good measure of model skill for the obvious reason that cancellation of large biases can make a poor model appear skillful (e.g Fig. 1 of this paper). Mean annual precipitation shouldn't be the determinant for whether to use a model or not.

p. 3290, lines 23-25. ". . .consider mean annual precipitation to be a robust estimate of

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the overall sensitivity. . .” This may well be the case, however the authors’ justification for this, that the interannual variability in global average precipitation is small, is not evidence that this is the case.

p. 3290, lines 24-27. This sentence indicates that the authors determined that the observational datasets could be used because they compare well with the model results. I assume that the authors did not intend this meaning.

p. 3291, lines 6-10. The authors promise here to discuss four reasons for differences in global precipitation rates between models. There is considerable discussion of the role of temperature, but almost none of these other factors. Please add this discussion.

p. 3292, lines 20-24. “may relate to more fundamental differences in model physics” Okay. Please explain and demonstrate what these are.

p. 3293, lines 2-3. The authors need to demonstrate that moisture availability is the reason for the reduced sensitivity. They could do this in a number of ways, e.g. by quantifying changes in continental moist availability or continental relative humidity.

p. 3294, lines 5-6. “Paleogene boundary conditions other than CO2 are crucial in elevation precipitation rate in this model.” Why? How exactly? This point is interesting because it contradicts the conclusions from the HadCM3L model (p. 3293, lines 18-20). Why are these models responding differently to Paleogene boundary conditions?

p. 3295, lines 8-9. “The SPCZ is CCSM is also far weaker in the Eocene simulations. . .” Why?

p. 3295, lines 9-11. “. . .CCSM and HadCM3L strongly diverge in the Eocene. . .” Why?

p. 3295, lines 19-20, Fig. 5. It’s not clear why the authors have selected the models in the way that they have, on the basis of a “common global precipitation rate”. Results from all the models could be shown by using anomalies from the global average rather than absolute values.

p. 3297, lines 10-11. “HadCM3L displays far greater spatial contrasts in net precipitation change..” Why?

Section 3.4. The authors should omit this section on monsoons. The topic is not given sufficient attention. The monsoon results are summarized in a single figure without any real explanation of the results.

p. 3304, lines 1-4. The explanation given doesn't make sense. The failure of the models to predict enough precipitation isn't a result of too much rainout. The precipitation is too low either because there is not sufficient vapor (the saturation pressure is too low) or because not enough of the vapor is undergoing condensation (the lapse rate is too low).

p. 3304, line 6. “anomalies” should be “differences”

p. 3304, lines 23-24. I don't understand the point being made here. Please explain more fully.

p. 3336, Fig. 9. The differences in implied latent heat fluxes between models are quite large. In some models, the latent heat flux is symmetrical between hemispheres and in others it is not. Some discussion of these results, and the dynamics behind them, are required.

Refs. Gasson et al. (2013) is cited but is missing from the references.

Interactive comment on Clim. Past Discuss., 11, 3277, 2015.