Clim. Past Discuss., 8, C681–C689, 2012
www.clim-past-discuss.net/8/C681/2012/

Climate of the Past Discussions

Interactive comment on "Glacial-interglacial variability in Tropical Pangaean Precipitation during the Late Paleozoic Ice Age: simulations with the Community Climate System Model" *by* N. G. Heavens et al.

Anonymous Referee #2

Received and published: 28 June 2012

General Comments:

This manuscript presents an ambitious climate modeling study that runs a series of sensitivity experiments focused on the Late Palaeozoic ice age interval. It attempts to determine the reaction of tropical precipitation to a number of different internal and external climate forcing mechanisms. The study's stated purpose is to "simulate the glacial-interglacial climate variability evident in the geologic record of the LPIA in order to understand its underlying mechanisms." While I find the goals of the study to be scientifically admirable and relevant to LPIA study, the manuscript suffers from poor or-

ganization, presents some questionable methodology, and ultimately does not present a coherent message. The significant contribution of this study to the LPIA field lie in its use of a climate model with dynamic ocean and sea level change capabilities. In my estimation, publication of the modeling results in this manuscript is possible and would benefit the community, but it will require significant framing, organizational, and editorial revisions.

By and large, the greatest flaw of this study in its current form is that it attempts to do too much. Sensitivity experiments are run to determine the climatic change induced by differences in ice sheets, alpine glaciation, sea-level change, vegetation, greenhouse gases, and orbital configurations (in addition to multiple control runs of preindustrial climate). While each of these factors may play a role in tropical precipitation in the LPIA, in the manuscript's current state their presentation and relative relevance is incoherent.

Methodologically there are issues with using prescribed ice sheets and vegetation that are out of equilibrium with the simulated climate. These issues need a better explanation, but are not fatal to the manuscript's publication. The notable exception is the ICEH experiment that places alpine glaciers in hot equatorial latitudes. I do not see the benefit of presenting results of a climate system that cannot physically exist. I recommend that the ICEH experiments and their discussion be excised from the study.

Specific Comments:

Introduction:

The introduction presents pieces of the LPIA background, but provides little in the way of motivation for the sensitivity studies that comprise the manuscript. There is a minimal amount of background information, an attempt to link the Late Palaeozoic and Cenozoic periods, and a discussion of previous Late Palaeozoic modeling efforts. Considering the focus of the manuscript, the reaction of tropical precipitation to various forcing agents, one would expect that the justification for the various forcing agents would be presented. The introduction does not accomplish this.

CPD

8, C681–C689, 2012

- Diamictites are presented as the only line of evidence for LPIA ice sheets but were there others?

- The attempt to draw parallels between the Late Palaeozoic and Cenozoic is unconvincing. The existence of ice sheets in the LPIA and Cenozoic does indeed link them as icehouse periods, but what was the nature of the ice sheets; i.e., were they of similar volumes, similar latitudes, etc.? Likewise, it is mentioned that each period has cyclic deposition, but Milankovitch frequencies are found in deposits throughout geologic time. Are the coal-rich cyclothems typical of the LPIA also found in the Cenozoic? The main point here is: how similar/different are these two periods climatically and why is it important to the model results you are about to present?

- The relevance of the "trend of aridification" paragraph is not clear.

- Three paragraphs in the introduction are spent discussing previous modeling studies. This is significant (>50%) and suggests that the motivation for this manuscript will be model-model comparison. Based on the remainder of the manuscript, this is not true. My suggestion would be to recast the introduction to reflect the true focus of the manuscript and minimize discussion of other's modeling efforts here, saving it for the Discussion section. By recasting in this manner, you can explain to your readership the geological and climatological basis for the series of sensitivity studies you are about to present, and perhaps briefly mention what makes them new and unique; e.g., the inclusion of a dynamic ocean and quasi-realistic sea level changes via the inclusion of lakes.

Methods:

- Are these biases discussed in the Yeager et al (2006) citation important to this study? If so, how do they influence your results?

- Why was the paleogeography discussion relegated to an appendix? If it must be relegated, it would be good to include the citation for the paleogeographic basis here

CPD

8, C681–C689, 2012

(modified Blakely).

- The justification for modifying the Blakely and Rowley paleogeographies is unclear. Why are elevations of 0, 200, and 1000 m increased to 100, 200, and 1500 m? Why are some aspects of bathymetry from some reconstructions used but not others?

- The use of lakes at low elevations is novel and warrants greater description in the methods, due to its outsized effects in the final results. This could be set up nicely if a previous discussion of glacioeustatic change as a modifier of tropical precipitation in the Introduction section has been made.

- The IPCC radiative forcing paragraph is superfluous.

- Description of the vegetation modeling is confusing. It is my understanding that the goal was to create a greenhouse and icehouse vegetation distribution that could be prescribed as a surface condition within the model simulations. To do this, the CCSM3 base simulations were used to force the BIOME4 model. The results of BIOME4 were then converted to CCSM3 vegetation types to use in further CCSM3 simulations. Why not use CCSM3 vegetation in the base simulations, thereby eliminating the use of another model (BIOME4)?

- The vegetation simulated in the base simulations would likely be different than the vegetation simulated in the other sensitivity experiments. That is, the vegetation used in most experiments is out of equilibrium with the climate state. For example, is the biome distribution of the icehouse base simulation still realistic in the huge ice simulation? I would imagine this would influence your results and should be discussed.

- It is mentioned that BIOME4 is insensitive to soil properties. It would be good to either cite a study here or explain this (perhaps you did offline testing?).

- The discussion of “sunshine” and BIOME4 is problematic. Based on my reading of this section one method was used to determine the biome distribution in the greenhouse simulation while a different methodology was used in the icehouse simulation.

CPD
8, C681–C689, 2012

The justification for using differing methods is based on the notion that a particular biome type, xerophytic shrubs, had not yet evolved. Instead, a biome type in disequilibrium with the dry glacial climate, tropical forest is used. The justification for this change is based on the palaeobotantical record. Preservational biases of wet and dry deposition aside, this rationale is antithetical to the argument presented in the previous paragraph in the discussion of grass. "Because non-grass plants that were adapted to similar climatic conditions to present-day grasses likely occupied those biomes." If this is the case, wouldn't something similar to xerophytic shrubs occupy the glacial tropics rather than tropical forests? What effect does the tropical forest vice the xerophytic shrubs have on the tropical climate? Based on these considerations, I recommend an additional simulation that quantifies the effect of the tropical forest v. xerophytic shrub on tropical precipitation.

- The method of creation of the prescribed ice sheets is unclear. The "mean daily liquid equivalent snow depth" is used. Is this the annual average? Summer average?

- Though it is mentioned in the discussion for the ICEH case, it should be mentioned in the methods and in the discussion that prescribed ice sheets are not in equilibrium with the climates being simulated (ICEB and ICES included).

- I'm not sure the error in the ice height algorithm needs to be discussed. It appears to have had no influence on your results and is in line with LGM observations. If the only issue is that it was contrary to your original intentions, why discuss it?

- The ICEH experiments make little sense to me. Why was 25.6 C chosen? Is there a modern analog that supports this number? If the alpine ice is completely out of equilibrium with the climate system, what are we learning from these highly unrealistic simulations?

- Section 2.7 suggests 'various simulations' and refers the reader to a chart. The orbital configurations need to be presented and discussed. They play a significant role in your discussion of monsoonal variability (tropical precipitation) and are central to the

CPD

8, C681–C689, 2012

message of the manuscript. Where did the chosen orbital values come from?

- Section 2.7 also mentions preindustrial simulations. The incorporation of these simulations (and the comparisons intra-text) seems superfluous. If they are essential to model validation (which is the context in which they are mentioned) they need to be discussed in full and not in the same section as the orbital variability. Likewise, caveats of this comparison need to be discussed; ocean gateways, continental configurations, topographies, etc.

Results:

The results are presented in a confusing manner. There are two main problems; a lack of organization and too many sensitivity experiments being discussed simultaneously. The presentation of results needs to be organized by sensitivity variable. For instance, different sections and figures should be dedicated to (a) ice sheet size, (b) orbital configuration, (c) sea level, (d) greenhouse gas concentration, etc. Each sensitivity variable should discuss the resulting changes to tropical climate. Discussing things in this manner may preclude the examination of all sensitivity variables, but this would aid in producing a more coherent message. Once the effects of different sensitivity variables have been explored, transition to monsoonal variability.

- Page 1927, Line 12-17: I read this as an attempt at model validation. Is this correct? Perhaps this should lead the results section?

- As it is one of the major advances of this study, the results would benefit from a greater discussion of the dynamic ocean and its effect on the climate system. How does this added component improve upon simulations with mixed layer oceans? Are their major/minor climatic differences?

- Discussion of the streamfunctions with relation to monsoonal variability is a good idea, but the current description is unclear. What does “seasonally varying cross-equatorial meridional cell” mean in relation to the figures, and does it occur in 10a-d, or only when

CPD
8, C681–C689, 2012

the a hemisphere's summer season is in perihelion? More explanatory figure captions and more descriptive labeling of the figure would assist in comprehension.

Discussion:

- Section 4.3 Glacial aridity or glacial humidity: It is unclear that the comparisons made in this section are robust. It is clear that the intention is to test individual sensitivity variables, but the experiments were not designed in a manner conducive to such comparisons. For example, the base simulations test both GHG concentrations and different sea level configurations. In an attempt to isolate the singular effect of GHGs, other simulations with the same GHG concentration but different sea level configurations are added. This implies that changes within the climate system due to changes in sea level are linear. It is not apparent that this assumption is robust. It is also unclear why these calculations are normalized and what they are normalized with. This needs a better/clearer explanation and/or citations to defend the method. One means to verify/prove the chosen methodology would be to carry out example 'clean' sensitivity experiments in which only one knob is turned at a time, thereby isolating the effects of the sensitivity variable in question.

- Based on the Figure 16 caption it is apparent that these precipitation values are land-based and equatorial, but Section 4.3 does not make this clear. It is also unclear in the Figure 16 caption what the statement ". . .are estimated changes between the LGM and the present day" means.

- It should also be made clear that what this data represents is not glacial-interglacial precipitation change, but how precipitation responds to various forcing agents that are assumed to accompany glacial and interglacial conditions. That is, glacial and interglacial conditions are not simulated in these experiments. Instead, climates are simulated with prescribed ice sheets and vegetation, though the ice sheets and vegetation are not in equilibrium with the climates.

- Pages 1939-1940 present arguments that are not substantiated by the modeling re-

CPD

8, C681–C689, 2012

sults. Temporal arguments of ice sheet growth and decay, changing tropical precipitation regimes, and regional climate differences are discussed that cannot be substantiated by the model results. These arguments should be scaled back. Discussion of results from the physically unrealistic ICEH experiments, particularly when used to explain geologic observations should be avoided. The paper would greatly benefit from a description of the more concrete results, such as changes in precipitation due to dynamic ocean currents and sea level change.

General Corrections:

- There are many instances in which the language is not specific; specificity of language would go a long way toward improving the presentation of results. For example, use of modifiers such as high magnitude or high frequency should be accompanied by parenthetical approximations suggesting the order of magnitude you are referencing. In addition, the tone of some of the language used is not professional, e.g., “None of this complexity is surprising”, “consistent with expected patterns”, “and other aspects of climate”, etc. While statements such as these may be correct, they often lack specificity. The manuscript would benefit from a more rigorous use of language.

- The naming convention of individual sensitivity experiments is a source of confusion. Creating names with better descriptive qualities would be helpful, particularly with the orbital simulations.

- Figures 9 & 11 are too small to read.

- The figure captions are generally not very useful in deciphering what is presented. A more thorough description would be beneficial.

Some additional manuscripts that might be of use to the paper’s content:

Chiang CH & Friedman AR (2012) Extratropical Cooling, Interhemispheric Thermal Gradients, and Tropical Climate Change, Annu. Rev. Earth Planet. Sci.

Heckel PH (1995) Glacial-eustatic base-level-Climatic model for late middle to late

CPD

8, C681–C689, 2012

Pennsylvanian coal-bed formation in the Appalachian basin, Journal of Sedimentary Research B65.

Heckel PH (2008) Pennsylvanian cyclothems in Midcontinent North America as far-field effects of waxing and waning of Gondwana ice sheets. In: Fielding, C.R., Frank, T.D., Isbell, J.L. (Eds.), Resolving the late Paleozoic ice age in time and space: GSA Special Paper, 441.

Horton DE, Poulsen CJ, Montanez IP, DiMichelle WA (2012) Eccentricity-paced late Paleozoic climate change, Palaeogeography, Palaeoclimatology, Palaeoecology.

Rankey EC (1997) Relations between relative changes in sea level and climate shifts: Pennsylvanian-Permian mixed carbonate-siliciclastic strata, western United States. GSA Bulletin.

Interactive comment on Clim. Past Discuss., 8, 1915, 2012.