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Climate of the Past Discussions

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Interactive Comment

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 #1

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This study describes the results of a suite of climate model experiments that were conducted to examine the sensitivity of low-latitude Pangaean precipitation to greenhouse gas levels, orbit, ice sheet extent, and sea level. The study is impressive for the large number of CCSM experiments conducted, but is ultimately frustrating for its lack of detail and precision. The manuscript may eventually be published, but requires substantial revision and additional experiments.

My main criticism of the study is the vagary of the climate model experiments. Science should be repeatable. There is no way that the simulations described here could be repeated based on the descriptions in the text. In addition, the decisions made for individual experiments are questionable. Examples include: (i) the decision to alter the possible sunshine used in BIOME4 for glacial versus nonglacial runs (p. 1923); (ii) the use of only two vegetation reconstructions for the branch simulations; (iii) the fact that the base experiments were not run to equilibrium. The authors also don't indicate the degree to which the branch experiments are in energy balance. In addition, several simulations are missing. For example, a CO_2 sensitivity experiment (without changes in sea-level) should have been run.

The authors use the same two vegetation reconstructions for all of the branch runs (p. 1922, l. 22). This is indefensible. The vegetation distribution would almost certainly be different between base and branch runs and could have an enormous influence on low-latitude precipitation. Furthermore, the authors don't address the role of vegetation on their simulations, or discuss this limitation. At a minimum, they should run an additional experiment with the same forcing but different vegetation prescriptions. The authors also don't describe changes in ocean dynamics and sea-surface temperatures between runs. These could be very important, particularly when comparing the results in this study from previous studies.

The title is misleading. It gives the impression that concrete results are offered, which is not the case. A more appropriate title would emphasize the model sensitivity to glacial-interglacial changes.

The Discussion is inadequate. There is no discussion of model limitations, and comparisons with other modeling studies are limited. The introduction emphasizes a controversy in glacial-interglacial climate variability, but the Discussion never really addresses this. Why do previous simulations support glacial humidity, but this study does not? The Introduction suggests that the reason might be related to CCSM having a dynamic ocean, but this is never mentioned again in the manuscript. Is it even possible to address these differences since the ice-sheet extents differ between studies? There seems to be a disconnect between the Introduction and Discussion/Conclusions. The comparison with model results and geological studies is very limited, giving me the impression that the geological studies that are mentioned were cherry-picked to match the experimental results.

The writing is inadequate throughout manuscript. The writing is not precise, is open to many interpretations, and includes grammatical errors. I have noted some, but not all, cases below.

p. 1918, l. 9-15. The idea of wet or dry conditions in the tropics associated with glacial or interglacial climates is too simplistic and too generalized. Pleistocene paleoclimate records indicate a more nuanced spatial response to glaciation (e.g. Wang et al., Nature, 2004 show that northeast Brazil was wet when Northern Hemisphere was cold). The authors refer to this complexity in their Discussion (p. 1940, l. 18-21), so it's surprising that they perpetuate this idea in the introduction.

p. 1920, l. 8-9. The authors report that "Yeager et al. (2006) has characterized the biases of this configuration..." It would be helpful to the reader to summarize what these biases are.

p. 1921, l. 11. "pN_2O was left at modern levels" "left" should be "specified" or "set". The authors are incorrectly assuming that readers have advanced knowledge of the base model configuration.

p. 1922, l. 8. "run until the trend...was greater than -0.5 Wm^{-2}" I assume that the trends were then between -0.5 and 0 Wm^{-2}? The writing here, as in many other places, is not very precise. It would be worthwhile including a table of how out-of-balance each experiment was, including the branch experiments.

p. 1922, l. 10. The authors report based on data from Kiegl and Shields (2005) that a bias of 0.5 C is introduced by running experiments for 500 yrs rather than to equilibrium. (i) It is unclear from the text how the authors made this estimate. (ii) There is no reason to think that the spatial biases in temperature and precipitation will be the

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same in all experiments.

p. 1922, l. 21. “The BIOME4 output was mostly insensitive to the assumed soil properties.” Please explain. What experiments were performed to test this?

p. 1923, l. 6. The authors should refer to the Supplementary Material with Horton et al. (2010) in which grasslands are included in the simulation.

p. 1924, l. 13-14. “a network algorithm” Please describe in more detail how this works. “the algorithm was tuned” Again, please describe in more detail.

p. 1925, l. 20 “included for consistency” Consistency with what?

p. 1926, l. 2. Improper use of a semi-colon. Also, add reference to Fig. 1 to show the ICEH glacial extent.

p. 1926, l. 3. “Land ice was then imposed. . .lower than this temperature.” What is the justification for this? Presumably this contour roughly approximates the Soreghan reconstruction? In this case, why use a pseudo-temperature cutoff? It would be clearer to simply state that a low-latitude ice sheet/glacier was imposed, rather than give the impression that there is some climatic basis for it. Is there geological evidence for this extensive ice cover in the Northern Hemisphere?

p. 1926, l. 8. “were performed, brought to equilibrium” What is the difference between performing and bringing the run to equilibrium? The description makes it sounds as though these are two different steps? Are the simulations “run to equilibrium”? Is it fair to say they were “brought to equilibrium” if there is still a substantial energy balance?

p. 1927, l. 9-11. It is unlikely that the sea-surface patterns described here are apparent in either Kutzbach and Ziegler (1993) or Peyser and Poulsen (2008). Neither study included a model with a dynamic ocean model.

p. 1927, l. 16-19. “. . .a good estimate of. . .” Delete. “. . .characteristics divide” The simulations can be divided based on their characteristics, but characteristics can not “divide”.

p. 1927, l. 22-27. This section needs heavy editing. “When this correction is applied. . .” What correct? What’s the magnitude? These effects are not necessarily additive. With all the experiments that have been conducted, why not run a clean CO2 experiment? (Same comment applies to discussion of sensitivities in Section 4.3.)

p. 1928, l. 6. “more global” This is incorrect usage. How can something be “more global”? “more extensive” would be appropriate.

p. 1928, l. 17. “other differential changes” What are these? Be specific.

p. 1929, l. 1-3. This sentence, which is conjecture, is superfluous, since the next sentence provides the model evidence. Delete please.

p. 1931, l. 24. “simple monsoon circulation index. . .was defined” The authors should explicitly state what this index is here. I did find a short description in the figure caption to Fig. 9. It would be clearer to state in the text: “A monsoon index, defined as the difference in area-average (146-191 longitude) 850-mb winds between 9N and 24N, was calculated.” Or something along these lines.

Fig. 12. It’s impossible to read the wind vectors. They are too small. What does the white shading represent?

p. 1934, l. 20 “monsoon’s success” Monsoon development, intensity??? What is monsoon success?

p. 1934, l. 23. “limited equilibration” I don’t think there is such a thing. How do you define it?

p. 1937, l. 11. “Therefore” Delete.

p. 1937-1938. Sensitivities are not necessarily linear, and thus can’t simply be added and subtracted. It should be made clear that this is a gross approximation.

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p. 1937-1938. Please write the description of the denominator rather than the number (e.g. ice area or difference in ice area) to make it easier to understand. The reader can get the value from Table 2.

Fig. 15. Sub-labels (i.e. a,b,c,...) are referred to on p. 1939, l. 21 but are not indicated on the figure. Please add.

p. 1938. The justification for the sensitivity analysis is questionable. What does it mean to indicate the precipitation sensitivity to a $10^6 km^2$ of polar ice or 1m of sea level? The sensitivity is almost certainly not linear. Moreover, the magnitudes are difficult to compare between plots. What does it mean to compare the sensitivity of a doubling of CO_2 to a 1m change in sea level?

p. 1939, l. 11-29. "melt the CPM" Wow! That's one hot summer orbit that could melt a mountain range. On a more serious note, these paragraphs are pure speculation and should be labeled as such. Since the conditions for widespread glaciation of the CPM are uncertain and may be quite severe, it is not clear that Earth's orbital changes would cause glacial waxing, waning. Furthermore, the evidence for CPM glaciation (particularly as prescribed in the model) is extremely speculative. To use this mechanism as the main control on low-latitude precipitation seems unwarranted.

Horton et al. (2012, Palaeo3) recently published a paper that describes the effect of orbital variations on low-latitude climate. How do their results compare with the ones described here?

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