Reply to Anonymous Referee #1

The comments of Anonymous Referee #1 are re-printed in regular text. Our responses are given in bold.

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.

We thank Anonymous Referee #1 for a thorough review of this manuscript. We agree with some of the comments, but disagree with the Referee about the merit of the economies we have adopted in methodology and analysis. Given the large number of experiments, we have a responsibility to present the more salient points of the simulations. Moreover, we feel the demands for precision are unwarranted, given the poor state of proxy data for validation.

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.

We respectfully disagree with the Referee. The level of detail we provided may have been confusing, but it was in the interest of repeatability.

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);

Here, the Referee is mistaken. We did not change the sunshine formulation between runs. In fact, we maintained the same sunshine formulation precisely to avoid the critique made by the Referee. Since both Anonymous Referees made the same mistake, we will pay special attention to clarifying this discussion in the revised manuscript.

(ii) the use of only two vegetation reconstructions for the branch simulations;

We think the Referee overstates the importance of this issue to the simulations. First, our results indicate that vegetation makes fairly minor differences to global surface temperatures (cf. Cases 1 and 3 and Cases 2 and 8 in Table 3). We consider them minor, since they accompany radical global changes to vegetation (conversion of temperate forests to a diversity of other types). In addition, work on a parallel project suggests that vegetation changes have a rather predictable effect on precipitation regionally. Less precipitation

leads to less transpirative biomes and thus less precipitation (and vice versa).

(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 CO2 sensitivity experiment (without changes in sea-level) should have been run.

Except for the orbital sensitivity simulations, the manuscript provides a ceiling for the disequilibrium of the simulations.

For the purpose of this reply, we also have calculated the mean radiative imbalance of the orbital sensitivity simulations in years 71-100:

Simulation	**Radiative Imbalance (W m^{-2})**
gh.v.ecc	**-0.46**
gh.v.ecc4	**-0.31**
ih.v.ecc	**-0.16**
ih.v.ecc4	**-0.26**
ih.g.b.ecc	**-0.46**
ih.g.b.ecc2	**-0.87**
ih.g.b.ecc3	**-0.09**
ih.g.b.ecc4	**-0.47**
ih.g.b.ecc	**-0.59**
ih.g.b.ecc4	**-0.17**

We mistakenly omitted a caveat about these simulations not being as close to equilibrium as the circular orbit simulations but will correct this oversight in the revised manuscript.

Changing CO_2 and sea level at the same time is entirely defensible. We have a clean sea level change experiment. The effect of changing pCO_2 by a factor of 10 is very large. And when we use the clean sea level change experiment to estimate the much smaller correction (~10%) for sea level change, we get the published CO_2 sensitivity for the model in this configuration. We see no reason to invoke non-linearities.

We are, however, open to running a CO_2 sensitivity experiment without sea level change.

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 lowlatitude 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.

We disagree with the Referee about the defensibility of our approach, as we have stated above. It is technically possible to re-adjust the vegetation (using BIOME4) to the climate of the simulation and re-run the simulation to approximate equilibrium once more. We are open to running such experiments if they will improve the Referee's confidence in the results.

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.

How about: "Simulating Late Paleozoic Glacial-Interglacial Climate Variability in the Community Climate System Model"?

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.

We disagree with the Referee's characterization of the connection between the Introduction and the Discussion. We address the controversy to the extent allowed by the information available to us. We even explain how glacial humidity might be a reasonable observation in some tropical areas or in certain eras. At the level of uncertainty in the problem, the proper framework for model inter-comparison would require identically forced simulations as far as is possible. We simply could not obtain the necessary forcing files to repeat past sensitivity experiments. We certainly are willing to extend the discussion of model uncertainties, but to some extent, the integrity of the model is less at issue than the mechanisms identified by the model, such as the relative importance of orbital forcing and low-latitude glaciation in setting the tropical precipitation regime.

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.

We agree that this idea is overly simplistic, but the paradigm of uniformly dry and/or wet glacials or interglacials remained prevalent in the Late Paleozoic literature until very recently (Horton et al., 2012, in press just after this manuscript was submitted). When the first author presented this work at the last AGU Fall Meeting, everyone with whom he spoke agreed it was the dominant paradigm.

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.

We would be glad to do so in the revised manuscript.

p. 1921, l. 11. "pN2O 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.

We will adopt the usage of the Referee.

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 0Wm^{-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.

We will include such a table in the revised manuscript.

p. 1922, l. 10. The authors report based on data from Kiegl [sic] 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 same in all experiments.

The Referee misunderstands our protocol here. We run experiments until the absolute value of the radiative imbalance is less than 0.5 W m^{-2} and the absolute value of the global surface temperature changes no more than 0.05 °C. We investigated what this protocol would mean for Kiehl and Shields' simulation. In other words, we downloaded the model output, determined where we would have stopped the experiment, and compared it with the last 30 years of the "equilibrium run." This information was an earlier draft of the manuscript. We will include it as an appendix in the revised manuscript.

Additionally, there is good reason to think that the spatial biases in temperature and precipitation will be similar between simulations. The principal inertia in the system is the deep ocean, which is regulated by the thermohaline circulation, which primarily interacts with the atmosphere at higher latitudes. Using Kiehl and Shields' results allows us to see what these theoretical high latitude biases look like in a similar paleogeography to the one we use.

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

We separately halved and doubled the two soil parameters, ran BIOME4 with the new input, and then evaluated the changes by inspection.

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.

We will include such a reference in the revised manuscript.

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.

We will be glad to describe this procedure in more detail in the revised manuscript.

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

We refer here to consistency with the way the land ice configuration was set up in the Southern Hemisphere.

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

We agree with the Referee that the punctuation in this sentence is rather haphazard.

How about: "The huge glacial configuration (ICEH) uses the results of an icehouse simulation forced by the big polar glacial configuration (icehouse.glaciation.big) (Table 3) to calculate the maximum seasonal mean temperature at the paleo-location of the proglacial/periglacial facies identified by Soreghan et al. (2009). (Seasonal means are denoted December-January-February (DJF), March-April-May (MAM), June-July-August (JJA), September-October-November (SON).)"?

We also will add a reference to Figure 1 in the revised manuscript.

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?

Soreghan et al. have so far reconstructed mountain glaciation at one point. The rationale for ICEH was that if ice was stable at that point, it should be stable in areas colder than this area in a warmer climate. This extrapolation technique may not be radical enough. In our simulations, tropical climate cools less precipitously than the rest of the Earth. The Referee, however, is right to suggest it may be too radical. Low-altitude equatorial land ice could be kept stable by some negative radiative forcing preferential to the region (or the whole of equatorial Pangaea). We will discuss this point in somewhat more detail in the revised manuscript.

We already have discussed the evidence for extensive ice cover in the Northern Hemisphere (Shi and Waterhouse, 2010). The deeper problem is many of the relevant basins to look for such glaciation (the Barents Sea, for instance) have been poorly explored until very recently.

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?

There is no material difference. We will clarify this in the revised manuscript. We used "brought to equilibrium" as shorthand for the equilibrium constraints we used. Hopefully, enhancing discussion of the analysis of Kiehl and Shields' run, as suggested by the Referee, will clarify the meaning of this shorthand.

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.

We respectfully disagree with the Referee. There is no reason why a dynamic ocean model should be necessary to drive a Paleo-Tethys monsoon. If the monsoon is an atmospheric circulation, why would anything in the ocean but the energy budget of the surface waters matter to its dynamics? We should point out that Shields and Kiehl (2007) found that the location of the warm pool in Paleo-Tethys (and some details of the monsoon over Pangaea) was sensitive to whether the atmosphere and ocean were coupled. Yet the fundamental point remains, the monsoon is dependent on the SST distribution, not how the SST distribution comes to be.

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".

We will make the deletion in the revised manuscript. We respectfully disagree with the Referee on the second point. The use of "characteristics divide" is fairly widespread in published English usage, and we would be able to cite some potent examples in support of this usage.

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.)

We agree with the Referee about the editing insofar as our attention to detail here led to obfuscation. We will be clearer about the linear approximations that we make when interpolating in the phase space of the simulations and the uncertainties these linear approximations introduce. We are open to running a clean CO_2 experiment, but we disagree with the Referee that such an experiment was objectively necessary when varying pCO_2 by an order of magnitude concurrently with a sea level change. Note that the correction applied is ~10%..

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

We disagree with the Referee in part. A phenomenon that has a teleconnective effect at some critical level has "more global effects." Here, "global" is used antonymously with "local." We, however, do think that "more extensive" still might read better.

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

We refer to the replacement of any biome by one with a higher albedo. We disagree that further specificity is needed.

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

We disagree with the Referee that this sentence is superfluous, since it maintains continuity with the beginning of the next paragraph.

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.

We will do so in the revised manuscript.

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

We disagree with the Referee about the impossibility of reading the wind vectors. Zooming to 300-600% makes them quite clear. In a journal with a print edition, we would be more concerned. We will note in the revised manuscript that the white shading refers to precipitation less than 1 mm per year.

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

Monsoon intensity beyond a certain threshold. If the monsoon rains only reach Goa but not Delhi, the monsoon has some intensity but is considered a failure. We agree with the Referee that this usage is overly informal and will change it to intensity in the revised manuscript.

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

We will rephrase this to, ”not bringing the simulation as close to equilibrium as Kiehl and Shields (2005)”

p. 1937, l. 11. “Therefore” Delete.
We will remove “Therefore” in the revised manuscript.

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.

We agree with the Referee that the linear sensitivities are an approximation. We will underline this point in the revised manuscript.

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.

We will do so.

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.

The Referee is mistaken. Sub-labels appear on Figure 15.

p. 1938. The justification for the sensitivity analysis is questionable. What does it mean to indicate the precipitation sensitivity to a 106km2 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 CO2 to a 1m change in sea level?

We disagree with the Referee. We must make some choice of normalization. Figure 15 re-normalizes to units that allow multiplication in the range of 0.01-100. The point of Figure 16 is to re-scale the quantities to make a realistic comparison of plausible glacial-interglacial changes. But then again, this re-scaling is plausible if you believe glacial-interglacial cycling during the late Paleozoic is similar to the late Cenozoic.

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?

We are quite willing to point out that this discussion is speculative. High sensitivity of alpine glaciers to small changes is not a radical suggestion. . Significant alpine glacier changes are recorded from the Little Ice Age (Le Roy Ladurie, 1983). Moreover, concurrent pCO_2 changes could be more important for melting the CPM glaciers than the orbital forcing itself.

The Referee makes reference to Horton et al. (2012), a manuscript that was available online less than a month before we submitted this manuscript. The

fundamental argument of this paper is that pCO_2 is the prime control on low-latitude precipitation. If we restricted our phase space to the relatively high-latitude glacial configurations that are generated by Horton et al. (2012)'s simulations, we would obtain the same result. Our simulations provide enough information to compare the amplitude of orbital-driven low-latitude precipitation variability in our simulations and those in Horton et al. (2012). We will address this in the revised manuscript. The interpretation of Horton et al. (2012) suggests there was a large amount of carbon burial immediately before and during deglaciation (Figure 9), which is consistent with pCO_2 peaking in early glacial phases. This concept, however, seems counter-intuitive.

References Cited Here But Not In the Discussion Paper

Giles, P.S.: Low-latitude Ordovicia to Triassic brachiopod habitat temperatures (BHTs) determined from $\delta^{18}O_{[brachiopod\ calcite]}$: A cold hard look at ice-house tropical oceans, Palaeogeogr. Palaeoclimat. Palaeoecol., 317-318, 134-152, 2012.

Le Roy Ladurie, E.: Histoire du Climat depuis l'An Mil, Champs Flammarion, Paris, France, 1983.

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