Reply to Anonymous Referee #2

The comments of Anonymous Referee #2 are re-printed in regular text. Our responses are given in bold.

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 organization, 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.

We thank Referee #2 for a thorough review of this manuscript. We appreciate the Referee's interest in improving the framing, organization, and editorial presentation of the manuscript.

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.

We respectfully disagree with the statement that "the presentation and relative relevance" of the forcings is incoherent. Since there are few observational constraints on the variability on less than 400,000 year timescales, of some forcings, we chose to explore the phase space of possible variability as far as we could.

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.

We respectfully disagree with the Referee's suggestion to excise simulations that use ICEH. As documented in several papers in the peer-reviewed literature peri- and proglacial sediments and landforms have been hypothesized in late Paleozoic deposits from the eastern end of the Central Pangaean Mountains (e.g., Becq-Giraudon et al., 1996) and the western end (Sweet and Soreghan, 2008; Soreghan et al., 2007, 2008a, b, 2009). The Becq-Giraudon et al. (1996) study posits that these deposits were high elevation(>4500 m). The preservation of deposits from that altitude for 300 million years is highly unlikely, suggesting that deposits probably were from lower elevations. Estimates of the paleo-altitude of periglacial deposits on the western end are 500-1500 m. Furthermore, a recent paper on brachiopod isotopic compositions by Giles et al. (2012) suggests that Late Paleozoic tropical oceans were cold relative to modern interstadial tropical oceans, results that are potentially consistent with the cold tropical scenario that ICEH produces. Anomalously low tropical water temperatures were also suggested (with some caveats) by Powell et al. (2009). Very low (<30 degree) latitude tropical glaciation has also been recently documented for the Devonian (e.g., Brezinski et al., 2010)

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.

In the revised manuscript, we will adjust the focus of the manuscript to justify the sensitivity experiments.

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

See Wanless and Cannon (1966). There are examples of striated pavement, striations on cobbles, and erratics etc. We will mention those in the revised manuscript.

- 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 literature puts forth little consensus about the details (on issues as wide-ranging as Pennsylvanian glacial extent and Pleistocene cyclothems) that would allow the detailed comparison contemplated by the Referee. Nevertheless, many authors have drawn this comparison. This discussion was meant to be a hook to draw the wider readership of the journal into the late Paleozoic. We certainly can de-emphasize these points in the revised manuscript if needed.

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

The clarity of this paragraph will surely be improved if we rewrite the Introduction to emphasize the points of interest to the Referee.

- 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.

We will adopt the Referee's suggestion to restructure the Introduction in the revised manuscript, though we will discuss the potential for low-latitude glaciation as well.

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

We will address this in the revised manuscript. The most relevant issue is that the sea ice cover in some simulations may be unrealistically high. A bias of this type would primarily affect icehouse.glaciation.huge and its orbital sensitivity simulations.

- 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 (modified Blakely).

We relegated the paleogeography discussion to an appendix for the sake of brevity and readability.

We will include a citation to the two major sources in the text.

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

The elevations refer to colorbar labels on the Rowley map, which we interpret to mean, "Above this altitude but not above the next altitude."

- 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.

We hope that the revised manuscript will contain an Introduction section that sets up the discussion of the lakes as well as the Referee hopes.

- The IPCC radiative forcing paragraph is superfluous.

We respectfully disagree-- This discussion is quite relevant to evaluating the CO_2 sensitivity.

- 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)?

We do not understand the Referee's comment. CCSM3 in a certain configuration can simulate vegetation dynamically, but doing so was computationally prohibitive for this study. Therefore, we needed some way to turn climate into biomes outside of CCSM3.

- 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.

Anonymous Referee #1 made this point also, but more forcefully. In the case of the circular orbit simulation that uses ICEH (icehouse.glaciation.huge), we have found that adjusting the vegetation strengthens the pseudo-monsoon, as areas with less transpirative biomes become drier. We are open to adjusting vegetation in the sensitivity experiments by another iteration of the procedure the Referee has described.

- 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?).

We did do what we think the Referee means by offline testing. We will describe the offline testing in the revised manuscript.

- 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. 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 Referee is mistaken. The sunshine was parameterized for both V2500 and V250 in the exact same way. All we note here is that a different sunshine parameterization would have a significant effect on V250 but not V2500. If we adjust the vegetation, we will use this different sunshine parameterization, since it is the one more widely used in the literature. If we do not adjust the vegetation, we will clarify this discussion, as both Anonymous Referees misinterpreted what we wrote on this subject, implying our discussion was unclear.

The critique concerning the shift of tropical vegetation with climate is broadly correct. In the extreme glacials we produce using ICEH, the equilibrium vegetation does change somewhat. Western equatorial Pangaea gains forests,

while the rest of equatorial Pangaea gains more deserts. We have investigated the effect of this vegetation change and find it mostly reinforces the precipitation changes that drive it. In other words, equatorial Pangaea becomes even drier, and more precipitation falls at ~20 degrees North and South.

- 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?

We will provide more details about this procedure in the revised manuscript. A key element is how many days in the year that the snow liquid equivalent depth is greater than 1 cm.

- 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).

We will note this in the revised manuscript, though we should point out that this is true of any simulation with an imposed land ice configuration, including ones that use historical reconstructions of the Laurentide ice sheet etc.

- 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?

We respectfully disagree with the Referee. The deep interest of the Anonymous Referees in repeatability and methodological details suggests to us that the error in the ice height algorithm should be mentioned.

- 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?

The principle of this simulation is that 25.6° C is the warmest seasonal mean temperature experienced by the point corresponding to the area where periglacial deposits have been identified on the western end of the CPM. The paleo-altitudes are approximately the same, too. Our argument is that areas simulated to be colder than this in the simulation using ICEB were likely glaciated.

This extrapolation technique may not be radical enough. In our simulations, the tropics cool less precipitously than the rest of the Earth. Or it could 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). Moreover, desert areas may be cold but not receive enough precipitation for glaciers to form. Even more remarkable, tropical ocean temperatures only drop ~4° C between icehouse.glaciation.big and icehouse.glaciation.huge, suggesting that matching the variability proposed by Giles (2012) would be difficult. We will address these issues in greater detail in the revised manuscript.

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

We will explain the choices of the orbital parameters in the revised manuscript. They primarily come from previous modeling papers, but we did check the range of Laskar's models for Cenozoic orbital parameters. (Paleozoic orbital parameters cannot be reconstructed.)

- 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.

We disagree with the Referee in part. It is quite standard practice to use pre-industrial controls as benchmarks for more experimental simulations. The purpose of citing these simulations is to show what the model does with well-known input conditions. In the revised manuscript, we will separate out the pre-industrial control simulations from the orbital variability simulations in Section 2. We are unsure how the caveats mentioned by the Referee are relevant to our discussion of these simulations.

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.

The results are presented in a manner that corresponds to our confidence in how well our experiments can be applied to the exceptionally uncertain state of climate during the Late Paleozoic Ice Age. We considered presenting the results in the way suggested by the Referee but found that such a presentation tended to emphasize minor experimental results that might not be robust.

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

This is a good suggestion.

- 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 [sic] major/minor climatic differences?

Mixed-layer oceans are more highly parameterized than dynamic oceans. The standard practice for setting those parameters is to tune them so that the simulations match the observed, present-day behavior of the Earth's oceans. In periods during which such observations are unavailable, the results of a dynamic ocean model sometimes are used to set the parameters. In many cases, however, modelers simply use a mixed-layer ocean tuned to modern ocean heat transport (see discussion in Section 5 in Peyser and Poulsen (2008)).

Which is not to say that a dynamic ocean model is perfect. Dynamic ocean models are somewhat parameterized as well. We could certainly test the effects of a mixed-layer ocean tuned to modern conditions on some of the simulations we present. However, we would prefer to focus the paper on sensitivity to forcings rather than on sensitivity to unphysical approximations.

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

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We agree with the Referee. Example cross-equatorial cells are shown in Figures 10b-c, not all four panels. We will highlight this feature better in the revised manuscript.

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.

Both Anonymous Referees wanted a cleaner pCO_2 sensitivity experiment. We would have thought that showing that the global surface temperature response due to doubling CO_2 was in line with the previously reported behavior of the model would have eliminated the need to do so. However, we are open to performing such an experiment.

- Based on the Figure 16 caption it is apparent that these precipitation values are landbased 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.

We will make sure that the same information appears in the text of the manuscript as in Figure 16. We will be specific in the revised manuscript what we mean "estimated changes between the Last Glacial Maximum (21,000 years ago) and pre-industrial conditions (c. 1850 CE)."

- 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.

We will clarify this point in the revised manuscript.

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

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We will scale back these arguments in the revised manuscript where they are unsubstantiated by the model results. We object to the characterization of the ICEH experiment as physically unrealistic, since this experiment is a fair representation of highland equatorial glaciation, for which there is hypothesized geological evidence published in several peer-reviewed journals. Its plausibility is supported by recent geological and geochemical studies. Even if the ice cover in the experiment is transient, its effects on the hydrological cycle and regional circulation are physically realistic.

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.

There is no acceptable way to attribute precipitation to the use of a dynamic ocean model. Figures 15 and 16 are quite clear about the changes in tropical Pangaean precipitation due to 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.

We will make the language more specific in the way the Referee suggests.

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.

Some of the language is vague or perhaps overly informal rather than “not professional.” We will pay attention to this issue while revising the manuscript.

- 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.

We will make the names we use even more descriptive in the revised

manuscript.

- Figures 9 & 11 are too small to read.

These figures are quite easy to read when using a Zoom function on a PDF reader. If *Climate of the Past* were a print journal, we would be more concerned.

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

The careful work of both Referees has identified some good places for the captions to be improved, and we will heed these suggestions.

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 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.

We thank the Referee for the interesting suggestions.

References Cited Here But Not In the Discussion Paper

Brezinski, D.K., Cecil, C.B., and Skema, V.W.: Late Devonian glacigenic and associated facies from the central Appalachian Basin, eastern United States: Geol. Soc. Amer. Bull., 122 (1-2), 265–281, doi: 10.1130/B26556.1, 2010.

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

Powell, M.G., Schöne, B.R., and Jacob, D.E.:Tropical marine climate during the late Paleozoic ice age using trace element analyses of brachiopods: Palaeogeogr. Palaeoclimat. Palaeoecol., 280 (1-2), 143–149, doi: 10.1016/j.palaeo.2009.06.003, 2009.

Wanless, H. R. and Cannon, J.R.: Late Paleozoic glaciation, Earth-Sci. Rev, 1, 247-286, 1966.