Review#1

The authors conducted two sets of experiments: one for the pre-industrial (PI) period serving as a control, and another for the Middle Miocene. Within each set, they examined two orbital configurations (orbmin and orbmax), analyzing seasonal and spatial patterns of air temperature across these different configurations. While the modeling results are interesting, the study's motivations remain somewhat unclear. I recommend major revisions to ensure the model results are discussed more appropriately and the research objectives are better articulated.

We thank the reviewer for the constructive comment. We have substantially revised the introduction to (1) characterise the distinctive Miocene background climate, (2) summarise the orbital-scale variability recorded in proxies, and (3) clearly state that the aim of this study is to examine how the Miocene climate responds to seasonal–latitudinal insolation anomalies. The revised structure ensures that the motivations align directly with the analyses presented in the manuscript.

In the introduction, the authors review a range of literature documenting global mean climate changes across G-IG cycles during the (Late) Pleistocene. Similarly, in the third paragraph, they cite several benthic $\delta^{18}O$ reconstructions (Holbourn et al., 2007; Tian et al., 2013; Westerhold et al., 2020, etc.) to highlight how global climate also oscillated in response to orbital forcing during the Miocene. It is important to note that these studies primarily examine how global mean climate varies in relation to boreal summer insolation (i.e., changes in the seasonal distribution of insolation). However, the analyses presented in this manuscript do not address the mean and magnitude of global climate variability; instead, the discussions focus exclusively on seasonal and spatial patterns. In fact, the manuscript does not provide any estimate of global mean surface temperature under different orbital configurations. As a result, the motivations outlined in the introduction appear largely disconnected from the rest of the paper.

Many thanks for this insightful comment. We agree that the previous introduction unintentionally suggested a focus on global mean climate changes. We now clarify explicitly that our objective is not to reconstruct glacial–interglacial-scale global-mean variation, but rather to examine how a warm, low-ice Miocene climate responds to seasonal–latitudinal insolation anomalies.

Regarding global mean temperature, the values are reported as global mean air temperature (GMAT) in Table 1. As expected for precession-dominated forcing, the annual-mean changes are small, while the largest responses occur seasonally and regionally.

The revised introduction now clearly frames why estimating the sensitivity of the Miocene climate system to seasonal insolation anomalies is essential, and directly connected to our results.

Another drawback, in my opinion, lies in the underlying assumption of the experiments. This is illustrated by lines 85–86: “A cold-orbit simulation with minimum Northern Hemisphere (NH) summer insolation (orbmin), and a warm-orbit simulation with maximum NH summer insolation (orbmax), were performed for both the preindustrial (PI) and the Miocene.”

Why should one expect a warmer equilibrium climate in response to stronger Northern Hemisphere summer insolation (NHSI) during the Miocene? The phase relationship between NHSI and global (mean) climate described here is primarily based on observations from the Pleistocene and is likely influenced by boundary conditions specific to that period—such as the presence of large Northern Hemisphere ice sheets and the way ocean circulation modulates the global carbon cycle. There is no evidence that these conditions existed during the Miocene.

In order to determine the phase relationship between Northern Hemisphere summer insolation (NHSI) and global climate, we need a precise orbital-scale chronology that is independent of astronomical tuning—something that is currently unavailable for the Miocene interval. In other words, one could equally hypothesize that the Miocene was warm during periods of weak NHSI (and thus stronger Southern Hemisphere summer insolation) due to a reduced continental ice sheet and/or higher pCO2, potentially resulting from, for example, enhanced deep-ocean ventilation. In this scenario, the spatial climate responses could also differ significantly from those simulated in this study.

Thank you for this thoughtful comment. We fully agree that the phase relationship between Northern Hemisphere summer insolation (NHSI) and global climate in the Pleistocene cannot be assumed to apply to the Miocene, given the very different boundary conditions and the absence of large Northern Hemisphere ice sheets. This is precisely why we performed the orbital sensitivity experiments: to test whether the Miocene climate system responded differently from the PI climate under the same insolation perturbations.

Our approach does not assume that stronger NHSI must lead to a warmer global equilibrium state in the Miocene, although we admit that the use of the terminology “cold-orbit”/ “warm-orbit” was influenced by Quaternary climate studies and may not be appropriate for the Miocene. To avoid any confusion and misunderstanding, in our revised manuscript, “cold-orbit” and “warm-orbit” are not used anymore, and “orbmin” and “orbmax” are replaced by “NSImin” and “NSImax”, respectively.

That said, if this paper is published without a major redesign of the experiment, the author may wish to revise the introduction to clearly articulate and define the scope of the research, clarify the underlying assumptions, and highlight the major limitations—ensuring readers are aware of the study’s potential biases, constraints, and motivations.

Thank you for the suggestion. We have substantially revised the introduction to better articulate the scope, assumption, objectives and limitation of our study.

Some minor points:

Line 26: The Southern Ocean warms unexpectedly

Why “unexpectedly”

We mean that the Southern response is different from what we would expect from local insolation change and from the PI phasing. This has been rephrased as “reversed Southern Ocean anomalies under low insolation, where poleward-restricted Miocene sea ice enables winter insolation changes to trigger positive ice-albedo feedbacks”.

Line 27: “Lower internal temperature variability in the Miocene”

What do you mean by “Lower internal temperature variability”?

We mean that the temperature difference between orbmin and orbmax in the Miocene was smaller than PI’s. This has been clarified as “weaker orbital-scale temperature response”

Line 37: “glacial-interglacial cycle through climate feedbacks (Milanković, 1969).”

The original work was published in the 1940s. Are you certain Milankovitch had any understanding of climate feedbacks at that time?

Thank you for this question. Milankovitch’s work demonstrates that he had considered climate feedbacks (in particular the thermal effects of insolation and the snow and ice feedbacks) in his climate model (see his 1941 book or a summary of his work by Andre Berger 2021, https://doi.org/10.5194/cp-17-1727-2021). As his 1941 book is the best known, we now cite this book and the subsequent studies.

Line 70-72 “it explores how the absence of NH ice sheets, expanding Southern Ocean sea ice and strengthening monsoon rainfall shape Miocene orbital-scale climate variability on orbital scale.”

What evidence suggests that there was expanding Southern Ocean sea ice?

We meant sea ice expansion during the late Miocene with climate cooling down. But this has been removed in the reorganized introduction.

Line 80: “MioMIP2 protocol”

What is the MioMIP2 protocol? What is its source? How does it differ from the MioMIP1 protocol?

MioMIP2 refers to the 2nd phase of MioMIP (submitted), and have for MCO, and a bit of ice sheet in Greenland. We will update the reference when the protocol becomes publicly available.

Line 242: rather than the 40 ka and 100 ka cycle of the Pleistocene

10 ka cycle?
There is a typo with 10ka, it should be 100ka. We have fixed this.

RC2: 'Comment on egusphere-2025-4485', Anonymous Referee #2

Review: Zhang et al., Weakened and Irregular Miocene Climate Response to Orbital Forcing compared to the modern day

Summary

This manuscript explored the impact of orbital forcing during the Miocene. The authors provided a comparison between a preindustrial and a high CO2 middle Miocene simulation and various sensitivity experiments with orbital min and max configuration. The authors suggest a weaker seasonality response to orbital configuration primarily due to the weak response of surface albedo feedback. Although the results presented are interesting, the current version of the manuscript presents more questions than answers. This is mostly due to insufficient analysis being presented.

We thank the reviewer for the insightful comment. Please find detailed response below.

Major

From a first-principle standpoint, why does your middle Miocene run have a weaker seasonality? Is it CO2, paleogeography, or ice-sheet configuration? The author raised all of these things in the introduction, but doesn't really provide any answers.

Thank you for raising this key question. Although a full attribution is beyond the scope of this study, we added result from an additional simulation (MCO-1x: same boundary conditions but lower pCO2) shown in Fig. S4. The MI-1x seasonality (~3.5 ℃) lies between PI (3.7 ℃) and MCO (3.2 ℃), indicating that both elevated CO_2 and Miocene boundary conditions contribute to the reduced seasonality, with the latter exerting a slightly larger influence. We added a sentence in Section 3.1 to clarify this.

It seems apparent in Figure 2 that your baseline MCO run shows an overall weaker seasonality, so in turn, your other sensitivity experiments also have a similar response to orbital changes. This leads to the question, is it because your PI run have lower CO2 that is leading to a stronger seasonality? Is it a general statement that warm climate intervals have weak seasonality or is it unique to the MCO?

Thanks for this interesting point. Fully disentangling the role of background CO_2 and broader warm-climate mechanisms would require a larger ensemble spanning multiple CO_2 levels and additional warm intervals (e.g., MioMIP + PlioMIP). In this study, our aim is more limited: we show that under identical insolation anomalies, the MCO simulation exhibits a weaker temperature response than PI. Whether this reduced orbital sensitivity reflects a general feature of warm climates or is specific to the MCO cannot yet be determined. We now clarify this explicitly in the revised manuscript: "Because comparable analyses are not yet available for other warm climate intervals, it remains uncertain whether the reduced orbital response identified here is specific to the MCO or reflect a more general feature of warm climate states. This question requires further investigation.".

Although it is interesting to see a weaker surface albedo response in the MCO simulations, it should be noted that this feedback is inherently linked to the prescribed vegetation and land ice. It is really only sea-ice and potentially cloud feedback that's responding to the orbital changes. The authors should show which parameter is causing the large albedo change. I assume from Figure S6 that sea-ice in the PI is responding much more readily, where your MCO runs most likely do not have any sea ice.

Thank you for this helpful comment. Because surface albedo in the model is computed diagnostically (reflected/incoming shortwave), land ice, vegetation and sea ice cannot be perfectly separated.

Regarding whether MCO has sea ice, the MCO simulations do retain seasonally varying sea ice despite with greatly reduced perennial ice, as shown in Fig. S8. Thus, this seasonal ice still responds to orbital forcing, but its variability—especially in NH—is much weaker than in the PI, where extensive sea ice allows a much stronger albedo feedback. This partly explains the stronger PI response.

In the Southern Ocean, limited but sensitive Miocene winter sea ice can still generate local positive ice–albedo feedbacks (e.g., in MCO_NSImin), so the sea-ice contribution is region-dependent.

We have clarified this in Section 3.2.1 and 3.2.3.

It would be useful to see how the SST, deep ocean, and various MOC respond to the orbital changes. I suspect this could be one of the reasons why you have such a weak climate response. For example, the PI run would most likely have a strong AMOC and could be easily impacted by orbital changes, while your MCO 3x simulation does not. Also, the authors primarily use ocean proxy evidence to indicate a weaker orbital response; its only appropriate the authors should supply some type of ocean analysis.

Thank you for raising this important point. We agree that examining the oceanic response—particularly SST, deep-ocean temperatures, and the overturning circulations—would provide valuable context for interpreting the climatic sensitivity to orbital forcing. A full analysis of the ocean circulation is substantial and is being prepared in a companion paper focused specifically on Miocene ocean–atmosphere dynamics.

To address the reviewer's concern here, we showed Atlantic Meridional Overturning Circulation (AMOC) response in our simulations (see figure below). Orbital forcing induces only modest AMOC anomalies in both the PI and Miocene experiments. Importantly, the Miocene AMOC is weaker and shallower, lacking a strong deep North Atlantic branch. This reduced overturning diminishes the system's ability to amplify orbital forcing, consistent with the weak global temperature response.

We now highlight this in the discussion and state that SST and deep-ocean analyses will appear in the forthcoming study.

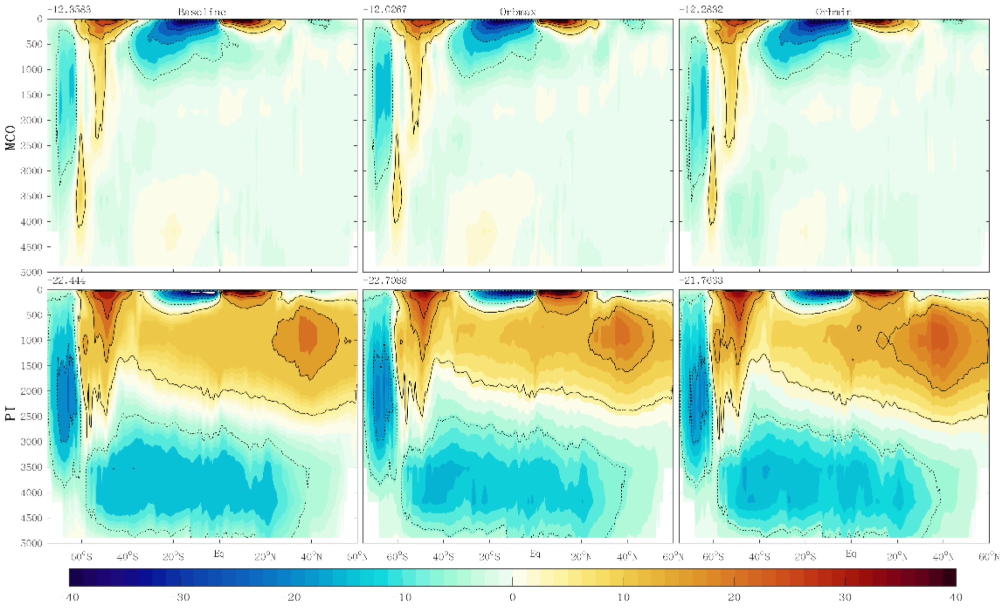

Fig. 1 MOC streamfuction in the simulations.

The author should modify the use of the general term “Miocene” to either middle Miocene or MCO since the boundary condition utilized in the experiments does not represent paleogeography, vegetation, ice sheet and etc changes in the late Miocene.

We agree and now refer to the simulations consistently as “MCO” to reflect that the boundary conditions correspond to the Miocene Climatic Optimum rather than the entire Miocene epoch.

Minor

The title is a bit misleading since regardless of the orbital changes with or without your MCO runs have a weak seasonality; nothing about it is irregular.

we agree that even if we do not consider the change of orbital forcing, the MCO base run already has a weaker seasonality than PI, but what we stress here is a weaker climate response to orbital forcing during the Miocene (which might be partly linked with its weaker seasonality?). Regarding the word “irregular”, we indicate the response in some region is stronger and in some region is reversed and have revised the title as “Weakened Miocene Temperature Response to Orbital Forcing compared to the modern day”.

Line 57 extra parenthesis

This has been fixed.

Line 66 vague sentence. Mechanism for what? Also, plenty of examples of Miocene modeling targeting specific mechanisms including orbital forcing. A generic statement is a bit disingenuous.

We mean there is no modelling work to specifically insolate orbital-driven variation for Miocene. This has been clarified as “Although geological archives provide evidence for persistent orbital pacing during the Miocene, the mechanisms linking these variations to climate response—particularly in warm climates lacking large Northern Hemisphere ice sheets—remain poorly constrained. In particular, there is a scarcity of climate modelling studies that isolate orbital effects under realistic Miocene boundary conditions."

Lines 106-112 TOA imbalance of .34 suggest not fully equilibrated I would suggest modifying "reach equilibrium" to quasi-equilibrium.

True, this sentence has been accordingly modified.

Line 190 citation needs to be fixed.

This has been done.

Line 269 I'm not sure what you mean by "less stable anti-phased behavior" ? Please provide a timeseries that shows fluctuation or instability in mean climate. From your results overall weaker seasonality would suggest much more stable climate.

Aplologies for confusion. We mean the orbmax and orbmin simulations (revised as the NSImax and MSImin) have opposite response in PI. But in MCO, they do not appear as expected. We have clarified this as "Both climates exhibit broadly anti-phased temperature response between maximum and minimum boreal summer insolation, but the Miocene response is ~1℃ weaker, spatially less coherent, and shows greater regional diversity".