Responses to Reviewer #1's comments:

Reviewer #1 General comments: *This paper adds to a body of studies of the effect of lakes during the North African "Green Sahara" mid-Holocene period. As it is rightly stated, there is still a discussion about which processes have enabled and sustained a relatively humid climate in that region during that period, and besides in particular vegetation, open water is one surface feature that has been proposed as a positive feedback mechanism involved in this interesting period of « recent » climate history. The manuscript does not add fundamentally new insights to this discussion, but as it stands, it is a basis for a useful contribution to this discussion, provided some necessary clarifications. These clarifications are needed in particular with respect to the model setup.*

A: We thank the reviewer for his/her general appreciation of our paper and for the constructive comments and corrections that helped to significantly improve this manuscript. We have carefully revised it as described in detail below. We would like to acknowledge that we have made corrections to figures 1-4 and figures S4 and S8 to address a mistake in the seasonal calculation. Specifically, some of the previous results displayed the May-Oct mean instead of the Jun-Sep results. This initial discrepancy has no impact on our overall findings. For the corrections in the manuscript, we provide the line numbers from the revised paper with track changes.

Reviewer #1 Comment 1: (hereafter referred to as R1C1, R1C2…) *Line 20: Editorial - In several places in the introduction, reference is made to a recent review paper instead of older key papers. For example here in line 20 where only a (good and complete) review is cited, it might be interesting to expand the list of papers cited to include some preceding key papers. However, that's an editorial question and it is also acceptable to only cite the review paper, for clarity.*

A: Thanks for your suggestion. As you suggested, more references have been supplemented in the introduction parts to strengthen the reasoning.

Line 20-21: The references have been revised as "(Gasse, 2000; Adkins, deMenocal, & Eshel, 2006; Claussen, M. et al., 2017)."

Adkins, J., deMenocal, P., & Eshel, G. (2006). The "African humid period" and the record of marine upwelling from excess 230Th in Ocean Drilling Program Hole 658C. *Paleoceanography, 21*(4). doi:https://doi.org/10.1029/2005PA001200

Gasse, F. (2000). Hydrological changes in the African tropics since the Last Glacial Maximum. *Quaternary Science Reviews, 19*(1), 189-211. doi:https://doi.org/10.1016/S0277-3791(99)00061-X

R1C2: *Line 68: A general remark: This paper used an isotope-enabled version of a GCM. I expected some more isotope-related analyses in this paper, for example to provide insights into precipitation recycling in the various simulations. I was a bit frustrated not to see more on this, as this might add some rather unique information from this study.*

A: Thank you for your comment and for bringing attention to the importance of isotope-related analyses in our study. We agree that such analyses can provide unique insights into the water cycle dynamics simulated by the isotope-enabled GCM, and we would like to clarify that the use of an isotope-enabled model was primarily aimed at capturing these dynamics, rather than solely for model-data comparison purposes.

To address this point, we have made additional clarifications in both the Method and Result analysis. In section 2.1 Lines 102-104: "Such isotope-enabled climate models have proven to be valuable tools for tracing water vapor transportation and identifying the sources of precipitation changes (Tharammal, T. et al., 2021; Liu, X. et al., 2022)."

In the Result section, we further analyzed the stable oxygen isotope ratio in precipitation to differentiate the source of increasing precipitation from ocean and land. We also made additional revisions in section 3.3 Lines 426-435: "Positive $\delta^{18}O$ anomalies suggested the presence of an oceanic moisture source in addition to the local lakes, whereas negative anomalies indicated the influence of local water cycling. The $\delta^{18}O$ increase in the northern regions (Figure S10) suggests the moisture sources from the Atlantic Ocean are associated with westerly monsoon winds. Conversely, the equatorial land areas show decreases in $\delta^{18}O$, which are also current with weakened evaporation (Figure 3k) and warming effects (Figure 3l) in MH_{WCE4}. Further examination of the $\delta^{18}O$ decrease (Figure S10d) in the equatorial land areas in MH_{WCE4} suggested that the slight precipitation increment (Figure 2d) was not driven by the westerly monsoon winds. Instead, such a warming effect induced by equatorial lakes may link to the differences in lake heating during daytime and night (Thiery et al., 2015). Hence, while lakes in WAM regions tend to result in wetter and cooler climatic responses, lakes located elsewhere (such as the eastern lakes in South Sudan) may not impact the northward WAM movement."

The use of isotopic features in the model allows us to validate our simulations against paleo-proxy records, avoiding bias from reconstructed datasets. However, these revisions emphasize that our use of an isotope-enabled model goes beyond model-data comparison and provides valuable insights into the water cycle dynamics and precipitation recycling processes in the region under study.

Tharammal, T., Bala, G., Paul, A., Noone, D., Contreras-Rosales, A., & Thirumalai, K. (2021). Orbitally driven evolution of Asian monsoon and stable water isotope ratios during the Holocene: Isotope-enabled climate model simulations and proxy data comparisons. Quaternary Science Reviews,252, 106743.

Liu, X., Xie, X., Guo, Z., Yin, Z. Y., & Chen, G. (2022). Model-based distinct characteristics and mechanisms of orbital-scale precipitation δ18O variations in Asian monsoon and arid regions during late Quaternary. National Science Review.

R1C3: *Line 79: Nowadays, T42 is on the lower end of usual climate model resolutions. Is there a reason to think that the results might be sensitive to resolution? For example, are there higher-resolution studies of the West African Monsoon system with MIROC, and is the monsoon representation in MIROC sensitive to model resolution?*

A: Thank you for your comments on the issue of model resolution. As you noted, T42 is indeed on the lower end of usual climate model resolutions. In our study, T85 simulation is our another choice but due to the number of sensitivity experiments and computational constraints, we finally chose to use the T42 resolution simulation with the isotope-enabled MIROC5-iso.

As for the high-resolution simulation on West Africa Monsoon (WAM) with MIROC, there seems not so much for the AR5. The latest PMIP4 MIROC-ES2L dataset for 6 ka also has a spatial resolution of T42, indicating the T42 resolution is acceptable for large-scale research (Ohgaito, R. et al., 2021). Besides, Steinig et al. (2018) used the Kiel Climate Model (KCM) to investigate the impact of spatial resolution on WAM precipitation, revealing that higher-resolution models produce similar results to lower-resolution models due to a reduction in convective (subgrid-scale) precipitation and increase in large-scale precipitation. Furthermore, lower resolution models may shift the African Easterly Jet (AEJ) core towards the north and strengthen the Tropical Easterly Jet (TEJ). Thus, the impact of spatial resolution of MIROC on the convective and large-scale precipitation and the position and strength of the AEJ and TEJ, will influence our research findings or not need to be further investigated. However, we agree that it would be interesting to investigate the sensitivity of the monsoon representation in MIROC to model resolution in future research. Hence, we would like to add the model uncertainty in discussion Line 516-518: "Additionally, while the main features of the WAM have been adequately captured, higher-resolution simulations are required to simulate finer convective activities and provide new insights at the subgrid-scale (Steinig, S., et al. 2018; Ohgaito, R. et al., 2021)."

While there is a possibility that our results could be sensitive to model resolution, we believe that our findings are still valid and provide useful insights into the lake influence in Green Sahara. In our studies, we have also performed model validation to ensure that our simulations capture the main features of the West African Monsoon system in section 3.1.

Ohgaito, R., Yamamoto, A., Hajima, T., O'ishi, R., Abe, M., Tatebe, H., ... & Kawamiya, M. (2021). PMIP4 experiments using MIROC-ES2L Earth system model. Geoscientific Model Development, 14(2), 1195-1217.

Steinig, S., Harlaß, J., Park, W. et al. Sahel rainfall strength and onset improvements due to more realistic Atlantic cold tongue development in a climate model. Sci Rep 8, 2569 (2018). https://doi.org/10.1038/s41598-018-20904-1

R1C4: *Line 92: "Figure S1a shows..." - Not very clear, figure hard to read. Can the procedure be explained in a bit more detail? I guess the main point is that the lake fraction in MH_ref and PI_ref (note typesetting error line 92, it should be subscript "ref") is weak, right? Because that provides a "almost no lake" reference for the other simulations. Can that be said more clearly?*

A: We apologize for any confusion caused by the unclear figure and will make sure to provide a more detailed explanation of the procedure in the revised version of the manuscript.

To address your specific question, the purpose of Figure S1a is to show the spatial distribution of lake fractions in reference simulations (MH_{ref} and PI_{ref}). The lake fraction represents the area of the grid cell that is covered by the lake, and in our simulations, we varied the lake fraction in the North Africa (NAf) basin to investigate its impact on the West African Monsoon system.

As you correctly pointed out, the MH_{ref} and PI_{ref} simulations were used as a reference to represent a scenario with almost no lake in the NAf. In these simulations, the lake fraction was set to a very low value (0.01%). By contrast, in the other simulations, we varied the lake fraction from 0.1% to 1.0%.

In the revised version of the manuscript, we revised in section 2.1 Lines 113-115: "*Figure S1a shows..(Figure S1b).*" to "In MH_{ref} and PI_{ref} experiments, the presence of lakes in North Africa (NAf) is minimal, using the global lake fraction map from the ETOPO5 as in MIROC5 standard simulations (Figure S1). In contrast, the other experiments show highly varied lake fractions, indicating a much higher lake fraction in those cases."

R1C5: *Line 110: "1.48 x 108 km^2" - please use superscripts correctly. What is the point to compare the lake area over NAf with the global land area? Lake fraction should be relative to the region you are looking at. Or is that the case here? Confusing. If it is relative to the entire land area of the Earth, it's huge...*

A: We apologize for the incorrect use of superscripts in the manuscript and corrected them in the revised version.

Regarding the lake area comparison, we agree that the lake fraction should be relative to the region of interest. In our study, we are interested in the lake area changes in the mid-Holocene compared to the present lake, and we have adjusted Figure S2g to reflect this. See section 2.1 at lines 145-147: "The average main lake fraction over the NAf region according to these different reconstructions varies from 1-10 % compared to the total land areas of NAf (Figure S2g)." We have also modified the Figure S2 caption: "(g) The fraction (circle size) of all the prescribed lakes experiments compared to the total land area of North Africa."

Regarding your concern about the large lake areas in LK1-LK4 (Here, we changed the lake map names with 'MH' to 'LK'), this is related to the datasets published in (Chen, Ciais et al., 2021), where potential wetlands (including lake areas) are defined as persistently saturated or near-saturated areas that are regularly subject to inundation or shallow water tables if there were no human disturbance (Tootchi et al., 2019). Whereas, the LK_98 and LK_02 only include lake maps. We have supplemented more map details in Lines 144-145: "LK4 has the largest lake proportion in the western, eastern, and Megalake Chad regions, and differs from LK2 primarily in its representation of Megalake Chad (Figure S2d, S2f)."; Lines 147-148: "It should be noticed that the water body delineated in LK_98 and LK_02 lake maps only pertain to the lake but the LK1-4 lake maps include both the wetland and lakes.".

R1C6: *Line 112: "In this study, wetlands are considered as lakes". What does that mean in the model world? How deep are the lakes? Does that simply mean that the water is present perennially? Please clarify how lakes are prescribed and treated.*

A: Here, we answer the questions of the reviewer in detail one by one.

"In this study, wetlands are considered as lakes". What does that mean in the model world?"

Regarding the LK_98 and LK_02 maps, we only used the small lake map (Hoelzmann, Jolly et al., 1998) and the maximum lake map (Tegen, Harrison et al., 2002). The details can be found in the data availability and Table S1. However, the latest high-resolution one (Chen, Ciais et al., 2021) includes both the wetland and lakes. Due to our model limitation, the wetland module only accounts for wetland-related processes in middle and high-latitude grids with snowmelt, as described by Nitta et al. (2015, 2017). Hence, these model features were considered in prescribing and treating lakes as wetlands in the MIROC5_iso when simulating the LK1-4 maps.

In order to further clarify this point, we make some revisions on:

Section 1 Lines 77-78: "…… and the recently-updated high-resolution lake and wetland reconstructions maps (Chen et al., 2021) over the NAf during the MH"

Section 2.1 Lines 147-150: "It should be noticed that the water body delineated in LK_98 and LK_02 lake maps only pertain to the lake but the LK1-4 lake maps include both the wetland and lakes. Generally, lakes and wetlands are persistently saturated or near-saturated areas that are regularly subjected to inundation or shallow water tables in the absence of human disturbances (Tootchi et al., 2019). In this study, wetlands are also treated as lakes in our climate model."

How deep are the lakes? Does that simply mean that the water is present perennially? Please clarify how lakes are prescribed and treated

The land component of MIROC5-iso is MATSIROC6. The lake module of MATSIRO6 considers lakes as a separate feature, and in this study, only the lake fraction boundary conditions were altered while keeping other boundary conditions constant in control experiments. By default, the maximum lake depth (H_{max}=climate + 10m) in MATSIRO6 is set to the climatology of lake depth plus 10 meters, with a minimum depth threshold (h_{min}=10m). As the lake depth map was not modified in this study, the lake depth initial values started at the minimum threshold and gradually reached a stable status over time. In our simulated areas, lake depths varied from around 10 to 40 meters in the lake fraction changed areas.

To clarify the description of the lake dynamics in our simulations, we have supplemented the lake module simulation in section 2.1 model introduction Line 98-102: "The MIROC land component is the Minimal Advanced Treatments of Surface Interaction and Runoff (MATSIRO) model (Takata et al. 2003), which could simulate important water and energy circulation. The lake module simulates the thermal and hydrological processes of lakes and their interaction with the atmosphere. It should be noted that a minimum lake depth threshold (10 m) is set, which means the lake permanently existed.".

Takata, K., Emori, S., & Watanabe, T. (2003). Development of the minimal advanced treatments of surface interaction and runoff. Global and planetary Change, 38(1-2), 209-222.

Nitta, T., K. Yoshimura, and A. Abe-Ouchi (2015) A sensitivity study of a simple wetland scheme for improvements in the representation of surface hydrology and decrease of surface air temperature bias. Journal of Japan Society of Civil Engineers, Ser.B1 (Hydraulic Engineering), 71 (4), 955–960.

Nitta, T., K. Yoshimura, and A. Abe-Ouchi (2017) Impact of arctic wetlands on the climate system: Model sensitivity simulations with the MIROC5 AGCM and a Snow-Fed wetland scheme. J. Hydrometeorol., 18 (11), 2923–2936.

R1C7: *Line 150: A r^2 of 0.33, is that really good?*

A: An R^2 value of 0.33 is indeed quite low. On the other hand, other isotope-enabled model studies for the mid-Holocene period, like Cauquoin et al. (2019) with MPI-ESM-wiso, found R^2=0.38 and RMSE=0.79‰. As Cauquoin et al. (2019), we found too low an amplitude of $\delta^{18}O$ changes compared to the observed ones. This is a common bias in isotope-enabled models. Additionally, we observed that around 50% of the data points exhibit positive anomalies in alignment with the observations, while the remaining 50% display negative anomalies. This suggests that our model accurately captures the direction of changes but with a weaker amplitude compared to the observed values. Therefore, we believe that the R^2 value of 0.33 with a very low RMSE of 0.81‰ we obtained in our study represents a reasonable correlation between the modeled and observed data, compared to other studies.

R1C8: *Line 152: "Our simulations bias..." - Can this be clarified, e.g. by restricting the scatter plot to an area with, say, Africa, Southern Europe and Western Asia?*

A: Thank you for your suggestion.

A: We agree that restricting the scatter plot to a specific area could provide a more regional perspective on the model biases. Unfortunately, there are few proxy sites only in Africa and West Asia, which limits our ability to constrain the model biases in these regions. However, we would further clarify the simulation bias in North Africa. While we acknowledge the limited availability of our using proxy records in Africa, the three North African stations for which data is available showed good agreement with the modeled data.

To further examine the model performance in North Africa, we first conducted a comparison with Figure 4a of another study by Larrasoaña et al. (2013). Our findings (Figure R1a) indicate that the MIROC5-iso simulation has difficulty in shifting the zone with precipitation less than 1000 mm/year northward, but it exhibits good agreement with the reconstructed map in the zone with precipitation exceeding 1000 mm/year. This comparison shows the simulation bias of the MIROC5-iso model in North Africa, specifically in terms of the northward movement of the monsoon system.

We also expand the comparison to include another proxy datasets compiled by Bartlein et al. [2010] that would enhance the robustness of our findings. However, we note that the proxy datasets provided by Bartlein et al. [2010] only cover the anomalies between 6ka-0ka, whereas our experiment shows the anomalies between 6ka-PI (1850y). Such difference between 0ka-PI would further bring ignorable bias to our comparison results in addition to the bias from constructed precipitation/temperature datasets. Considering such bias, the comparison results show agreeable changing trends in annual mean precipitation and mean temperature in the warmest month in spatial distribution, but they do not address a good statistical relationship between the proxies and model data (Figure R1 b-e).

In terms of the comparison between precipitation data from our model (Figures R1b and R1c) and the proxy data, we observe good agreement in the central part of North Africa (NAf). However, in the northern region, our model underestimates precipitation compared to the proxy data. These results confirm that our model has limitations in simulating abundant precipitation in the northern region of NAf. Regarding the comparison of summer season temperatures (Figures R1d and R1e), our model generally underestimates temperatures in the central part of NAf but shows good agreement in the northern part. These validation results indicate that our model fails to capture sufficient precipitation in the northern part of NAf, while precipitation tends to concentrate in the central part with lower temperatures for the mid-Holocene. This discrepancy aligns with the challenge faced by many climate models in reproducing adequate precipitation over NAf. Considering the potential bias introduced by proxy datasets construction and the differences in the study period, we consider the validation results to be acceptable.

This part of the comparison has been added in section 3.1 Lines 249-256: "To further examine the model performance in North Africa, we compare our precipitation result with Figure 4a in the study conducted by Larrasoaña et al. (2013). From Figure S4a, our results indicate that the MIROC5-iso was hard to reproduce the northward shift of the zone with precipitation less than 1000mm/year, but show good agreement with the reconstructed map in the zone with precipitation exceeding 1000mm/year. Besides, we also compared our result with precipitation and summer season temperature anomalies between 6ka-0ka, as provided by Bartlein et al. (2010) (Figure S4b-e). This comparison also revealed precipitation underestimation in the northern NAf and lower temperatures in the central NAf. These comparisons collectively suggest a simulation bias of the MIROC5-iso model in North Africa, particularly concerning the northward movement of the monsoon system."

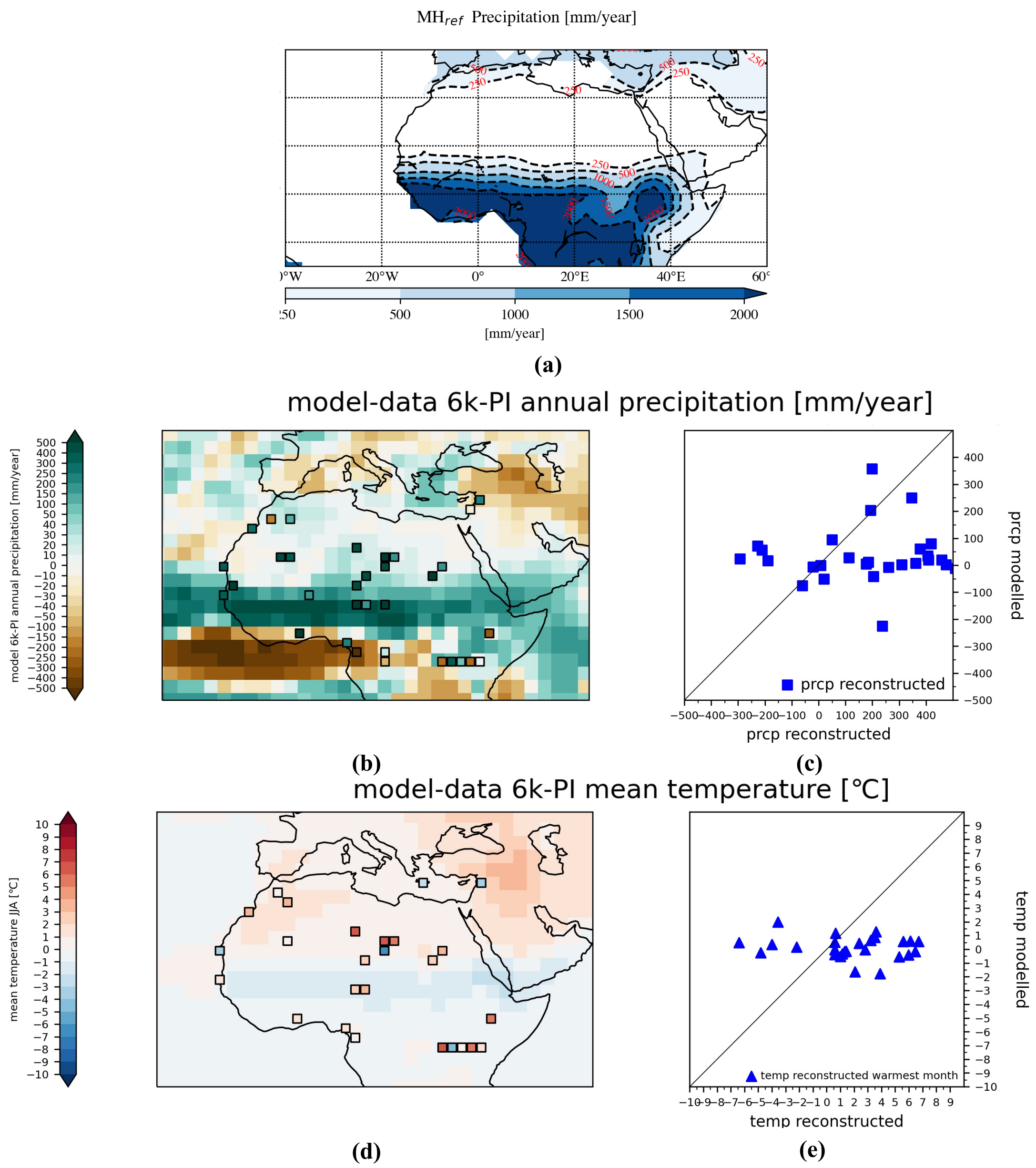

Figure R1. Precipitation and temperature model-data comparison for the reference mid-Holocene simulation in North Africa. (a) The spatial annual precipitation for MH$_{ref}$. (b) shows the simulated global pattern of annual mean precipitation between the MH$_{ref}$ and PI$_{ref}$ climate (background colors) and the observed annual mean precipitation changes (squares) between MH$_{ref}$ and the present climate. (c) is a scatter plot showing a comparison of observed precipitation changes with simulated precipitation anomalies at the same location. (d) and (e) are the same as (c) and (d) but for the seasonal mean temperature model [Summer (JJA)]-data [warmest month] comparison.

Additionally, as described in section 3.1, our model was able to successfully capture the critical components of the West African Monsoon (WAM), which are particularly relevant to our study of the lake-climate mechanism.

Hence, we acknowledge the importance of regional analyses in future studies when more data become available, and we found that there is simulation bias of the MIROC5-iso model in North Africa regarding the northward precipitation, but the simulation performance in North Africa is acceptable.

Larrasoaña, J. C., Roberts, A. P., & Rohling, E. J. (2013). Dynamics of green Sahara periods and their role in hominin evolution. PloS one, 8(10), e76514.

R1C9: *Figure 3: I appreciate that 200, 600 and 850 hPa winds and geopotential heights are also given, but it's unclear whether there is any reason why SM is associated with 200 hPa circulation, evap with 600 hPa, and t2m with 850 hPa. Is there a reason?*

A: To clarify, we included 200, 600, and 850 hPa winds and geopotential heights in Figure 3 to provide a comprehensive view of the atmospheric circulation changes associated with the simulated changes in soil moisture, evapotranspiration, and surface temperature. However, there is no specific reason why soil moisture is associated with 200 hPa circulation, evapotranspiration with 600 hPa, and surface temperature with 850 hPa. We apologize for any confusion that may have arisen from our presentation and hope that this clarification helps.

R1C10: *Figure 3a: Soil moisture changes. How much of that is prescribed? In the sense, does the prescribed lake water count here? Water quantities are huge, what does 1 m mean here (until what depth?)*

A: The soil moisture changes shown in Figure 3a are a combination of both prescribed and modeled changes. The prescribed lake water was not counted towards the soil moisture changes, as the lake water interacts with the surrounding soil and affects its moisture content.

Here, the original soil moisture means total soil moisture [kg/m^2], and the unit was transferred to [m] by dividing by 1000 kg/m^3. Hence the physical meaning is the total soil water column per area or the soil water column per meter [m/m] by dividing 1m depth. We have corrected those units in all of Figure 3.

R1C11: *Figure 3c: This is a strong cooling. What is the depth of these lakes? Is is thermal inertia due to depth or evaporative cooling?*

A: Thank you for your comments, which raise an important question regarding the cooling mechanism associated with the lake's thermal inertia or evaporation.

The simulated lake depths in North Africa range from 10 m to 40 m. In the lake module, the lake surfaces are considered in the energy balance solution, and each lake layer updates its water temperature based on the incoming downward flux and depth changes, which also is quite important for the lake-climate interaction. However, we assert that evaporative cooling plays a more crucial role based on the simulated results that the spatial distribution of evaporation anomalies and temperature anomalies exhibit similar spatial patterns, as shown in Figure 3. This finding suggests that the evaporative cooling effect may outweigh the influence of lake thermal inertia.

As for the such discussion on the comparison between lake thermal inertia and lake-surface evaporation is quite important for us to understand the lake-climate mechanism, we will do further related research work to understand their roles in energy transmission.

R1C12: *Line 269: "Additionally..." - This sentence is not grammatically correct I think.*

A: In the revised version, section 3.3 Lines 399-400, it has been revised to: "Additionally, precipitation scarcity values were lower in the western region and higher in the eastern region."

R1C13: *Figure 5a: Typo in the legend - should probably be "unitless" (as in the caption), not "uniteless".*

A: Thank you for your reminders. We apologize for this spelling error and have modified it in Figure 5a.

R1C14: *Line 279: Here are the isotopes, but the explanation is hard to follow for non-specialist readers. This needs and deserves some more explanation.*

A: Thank you for your reminders. Please see our response to the R1C2 comment.

This main purpose and findings of the isotope have been answered together in R1C2.

R1C15: *Line 314: "Limited by..." - this is confusing, not well written. One wonders whether you have dynamical lakes and vegetation in the model (you don't, if I understand correctly). Please clarify - it would be good to provide a bit more detail in the methods section about this.*

A: We apologize for the confusion caused by the wording in "Limited by the model integration and uncertainty, especially the dynamic lake or vegetation modules coupled with MIROC5-iso, ……". We meant that our model can not simulate the vegetation and lake dynamically but treat them as the prescribed boundary conditions for each experiment. Hence, further coupling of MIROC5-iso with dynamic lake or vegetation modules definitely can help us get new insights into the lake/vegetation-climate interaction in future work.

To clarify this in the revised manuscript, we added a sentence in Section 2.1 Lines 110-111: "It should be noticed that the lake fraction is treated as the prescribed boundary conditions in the model based on the corresponding datasets, as the model cannot simulate the lake dynamically." after the explanation of 'Land surface boundary conditions'. Besides, in Section 4 Lines 493-494, we further clarified the original sentence as: "Limited by the model integration and uncertainty, especially the lack of the dynamic lake or vegetation modules coupled with MIROC5-iso, ……"

R1C16: *Line 321: "out components, such as orbital forcing and greenhouses..." - you* *mean "external forcings, such as orbital parameter and greenhouse gas changes" (or something similar)?*

A: Thank you for pointing out this untechnical expression. We revise this sentence in section 4 Lines 502-503 to: "Moreover, understanding the external forcing, such as orbital parameters and greenhouse gas changes, …… ".

R1C17: *Line 327: "Limited by model dependency and module integration..." - this is unclear. Do you mean to say that the results are highly model-dependent (because the results from different studies are somewhat contradictory), and that they depend on the feedback mechanisms represented (e.g. dynamics lakes and vegetation included or not)?*

A: We apologize for the unclear wording. We meant that the results are limited by the model's dependence on certain assumptions and the integration of various modules. These factors can affect the reliability of the results and the understanding of lake/vegetation – climate interaction, especially when it comes to the representation of feedback mechanisms such as dynamic lakes and vegetation.

To make this clearer, we revised it in section 4 Lines 513-516 as: "Limited by model dependency, particularly the inclusion or exclusion of certain feedback mechanisms such as dynamic lakes and vegetation modules, as well as the differences in model components and parameterizations used in different studies, the land-atmosphere interaction mechanism forced by dynamic lake changes remains unclear."

R1C18: *Line 331: Full stop missing at the end.*

A: Corrected.

R1C19: *Supplementary material:*

Figure S2 - "Experiements" (typo). "(G)" missing in the lowest panel.

A: Corrected.