We are grateful for the through and professional reviews by the reviewers. Our response is given in italic font. Both comments delivered as text in the open discussion page and key comments in the PDF file are replied in this file. Minor wording/editing comments are corrected in the manuscript but not all presented here for easy reading. The locations, e.g. line numbers, sections correspond to the merged manuscript.

Following is the response to the comments from reviewer1 in the open discussion.

General Comments:

The quality of the writing needs to be improved. I have noted many instances of grammatical errors, poor/awkward wording and unclear/vague wording.

More complete and/or clearer descriptions of some aspects of the methodology are required. For example, a much better description of how the MODIS freeze-up and break-up dates were calculated is needed. Other examples are noted in my detailed comments.

Response: We have revised the writing. Calculation of freeze-up and break-up dates is rewritten and organized as section 3.4. Other parts in section 3 is revised.

A clearer explanation of how the one-dimensional lake models are implemented for Lake Nam Co is required.

Response: The coupling strategy (following) is added in second paragraph of section 3.1.

The two lake models have been coupled with WRF in a one-dimension way, i.e. no horizontal water flow is simulated. In the coupling strategy, a grid point with 50% lake cover is defined as a lake point. At each time step, lake models are driven by atmospheric forcing including air temperature, pressure, humidity, wind, short wave and long wave radiation, precipitation and reference height (height of first atmospheric layer). While the lake models feedback momentum, sensible heat and latent heat fluxes.

I was able to determine that the WRF model had 116 grid points over the lake by reviewing their earlier paper. But it is not clear if the lake models were simulating 116 water columns or not.

Response: It is the same, also 116 water columns, this information was added in the revised version. (Line 135)

What is also not clear is why a uniform depth of 40 m was used in the lake models. No information about the actual lake bathymetry is provided and no justification for the use of 40 m is provided.

Response: A uniform depth is a general way for treating lakes in climate models. 40m is based on previous studies with field observation and used in the study of La et al., (2016). It is calculated by the total water storage divided by lake area. The total water storage is based on Zhang et al., (2016). The above was added in the main manuscript. (section 2.1, line 81-82)

Because actual lake bathymetry has been provided in previous studies. E.g. Huang et al., (2019). A short description about the lake bathymetry and citations of the related papers were added (section 2.1, line 82-84).

References

Huang, A.N., Lazhu, Wang, J.B., Dai, Y.J., Yang, K., Wei, N., Wen, L.J., Wu, Y., Zhu, X.Y., Zhang, X.D. and Cai, S.X. 2019. Evaluating and Improving the Performance of Three 1-D Lake Models in a Large Deep Lake of the Central Tibetan Plateau. J Geophys Res-Atmos 124(6), 3143-3167.

La, Z., Yang, K., Wang, J.B., Lei, Y.B., Chen, Y.Y., Zhu, L.P., Ding, B.H. and Qin, J. 2016. Quantifying evaporation and its decadal change for Lake Nam Co, central Tibetan Plateau. J Geophys Res-Atmos 121(13), 7578-7591.

Zhang, B., Wu, Y., Zhu, L., Wang, J., Li, J. and Chen, D. 2011. Estimation and trend detection of water storage at Nam Co Lake, central Tibetan Plateau. J Hydrol 405(1), 161-170.

The authors stated that "…a setup with identical depth for all grids can, to some extent, reflects the horizontal energy mixing associated with lake water circulation". I am quite puzzled by this statement and by others where claims about better simulation of lake energy circulation in the lake are made.

Response: It is deleted to avoid misleading the readers.

The authors also need to provide some better arguments regarding the significance of their findings. They write the following in the summary section:

"Therefore, the main results and findings of this work can provide a good reference for climate model applications and the improved model also has practical application prospects over the alpine lake covered regions."

This is a vague and unclear statement. Some more detailed and specific statements regarding the significance and novelty of this study are required.

Response: we were supposed that this statement is supported by the previous part of the paragraph. To be more specific, it was modified to 'Therefore, we expect that the

main results and findings of this work can provide a good reference when simulating lake ice phenology by climate models, especially for the alpine lake.' (Line 491-492)

Confusing. Alpine lakes influence lakes mean what exactly? Small alpine lakes or only the large ones? Are the lakes being influenced not alpine lakes?

Response: To be more specific, it is revised to 'For example, lakes, especially large lakes, can generally enhance and/or change the temporal/spatial distributions of the precipitation over lakes and surroundings through both dynamic and thermal processes'. (Line 30-32)

Following is the response to key comments from reviewer1 in the PDF file.

The definition of phenology is: "the study of cyclic and seasonal natural phenomena, especially in relation to climate and plant and animal life." So using it here along with seasonality is not correct.

Response: 'seasonality' in the sentence is deleted to avoid duplication.

What do mean precisely by "lake energy circulations"?

Response: Revised as following. 'to better simulate lake energy circulations…' was changed to 'to better simulate key lake energy related processes, for example, turbulent heat fluxes with ice-free conditions, solar radiation transfer with ice-on conditions…'. (Line 73-74)

Large lakes are known to influence the local and regional climate but what specific role does the ice-water phase change play?

Response: One sentence was added to explain the roles. 'For example, compared with open water, frozen lake has much weaker evaporation and energy release to the atmosphere, and thus the climate effect of lakes will weaken simultaneously.' (Line 97-99)

More details on this field monitoring are required. Make and model of instruments used. How many moorings and their locations etc.

Response: revised. (Line 102-105)

'These data are the daily average water temperature data at different depths (3m, 6 m, 16 m, 21 m, 26 m, 31 m, 36 m, 56 m, 66 m and 83 m) and were obtained through field monitoring. The data were continuously recorded by deploying a water quality multi-parameter probes (CTD 90m, Germany) and temperature thermistors (VEMCO Minilog-II-T, Canada; accuracy: ±0.1 °C from −5 °C to 35 °C; resolution: 0.01 °C) in the water.'

This figure (Figure 1) is too small and does not have enough detail.

Response: The figure is enlarged to better show the surrounding topography of lake Nam Co.

Might be better to change this to: WRF-CLM since otherwise the two acronyms are easily confused.

Is the default lake model the CLM model without any revisions? That would then mean the revised default lake model is a revised CLM model. If this is the case do not use the term "default lake" model - just called it the CLM model or the revised CLM model.

Response: After checking literatures, we find that 'WRF-CLM' is usually used for the WRF model coupled with CLM land model. Instead, 'WRF-Clake' is used. The texts in the figures were also revised.

Define acronym

Response: Revised. Overall the manuscript, the full names of sensible heat and latent heat are used instead of acronyms.

How the model was initialized requires a more complete explanation? MODIS LST were3 discussed previously so presumably these were used here. Explain this fully and completely.

Response: revised (Line 138-140). 'The water temperature at the first layer was set to MODIS observed values, while the water temperature at the other layers is linearly interpolated with depth under the assumption that the water temperature at the bottom layer is equal to 3.5 ℃ (the temperature at maximum water density).'

I am confused about how the 1D lake models are coupled to WRF. WRF has 116 grid points over the lake. So are the lake models simulating 116 vertical water columns one at each grid point? Do the lake models simulate any horizontal flow? Presumably not but more details regarding how the coupled models work is needed here.

Response: Revised. The coupling strategy was added including the number of grid points (Line 135) and how the atmosphere part and lake part interact with each other (Line 128-132).

Should this be finite difference models

Response: Corrected.

This statement regarding horizontal energy mixing is not convincing. How does a constant depth reflect or account for water circulation?

Response: Yes, we also agree with you. Without supported by literature, we decided to delete such descriptions and just state that the model was set to a uniform depth of 40 m.

Why is the subscript q used for hydraulic roughness length and h for the thermal one? Seems like these should be reversed.

Response: The subscript q usually represents humidity. h is the first letter of heat and used for representing heat fluxes in the WRF model. Using 'q' and 'h' is consistent with the model code. Using such subscripts is also a usual way as in previous studies [Wang et al., 2018; Ma et al., 2022].

Wang, B. B., Ma, Y. M., Wang, Y., Su, Z. B., and Ma, W.: Significant differences exist in lake-atmosphere interactions and the evaporation rates of high-elevation small and large lakes, J Hydrol, 573, 220-234, doi:10.1016/j.jhydrol.2019.03.066, 2019.

Ma, X. G., Yang, K., La, Z., Lu, H., Jiang, Y. Z., Zhou, X., Yao, X. N., and Li, X.: Importance of Parameterizing Lake Surface and Internal Thermal Processes in WRF for Simulating Freeze Onset of an Alpine Deep Lake, J Geophys Res-Atmos, 127(18), doi:10.1029/2022JD036759, 2022.

Why are the 0.031 and 0.54 values appropriate for this lake?

Response: It is from previous studies based on instiu observations [Wang et al., 2018]. (clarified in Line 195)

The fact that the WRF model cannot simulate enough is presumably something that needs a citation.

Response: The sentences are revised to better clarify. 'Such treatment of lake surface albedo directly takes into account the snow influence on lake surface albedo. Therefore, we can make sure that the modeled snow fall biases will not directly bring uncertainties in simulating the lake processes. Over the complex terrain region, precipitation fall has been a great challenge and it is very difficult to correctly simulate precipitation amount. In TP, there is a systematic over estimation of precipitation in climate models [Gao et al., 2015; Su et al., 2013].' (Line 213-217)

Not enough detail here. Some basic description of the sensitivity analysis should be added.

Response: Added in line 219-223. In the sensitivity study, all the simulations are initialized at the 1st January 2014, a few days before lake freeze-up, which make sure the lake water thermal status and the freeze-up dates are correct at the initial stage. Before freeze-up, the lake water is fully mixed and a uniform temperature

initialization is reasonable. With an ice surface absorption ratio of 0.65, the model shows the best performance in simulating lake ice break-up date compared with other values (0.5, 0.55, 0.6, 0.7, and 0.75).

Must state clearly how the bias is defined e.g. difference between MODIS dates, positive is later dates etc.

Response: Revised both in main text and captions of associated figures. (for example, Line 275-276; 365-367; captions in Figure 8, 10, 12)

Writting issues

Revised, such as from 'too-early' to 'too early'.

Unclear. What artificial scaling method - provide more detail.

Response:an explanation is added after. '(i.e. When calculating the sensible heat and latent heat, multiply by a constant) ' (Line 428)

This paragraph reads like conclusions so it seems premature to locate it here. Could be moved to the later section.

Response: Moved to first paragraph of section 5.

Repeating what was said in the last paragraph above - more reasons to move the paragraph above.

Unnecessary sentences in line 380-385.

Response: Deleted.

I think this figure (Figure 12) should be presented much earlier. Perhaps right before Table 1 is presented since the dates in Table 1 were computed from the data shown in this figure.

Response: moved to figure 6, just after table 1. Reasons: Table 1 investigates as the whole lake, while the figures 7-12 investigates the spatial pattern. Spatial pattern in MODIS is treated as reference and more consistent with the later figures.

This statement and a similar one made previously need to be justified. How does a uniform depth reflect the horizontal energy etc..... I agree with the last part of this that inaccurate simulation of circulation is a possible explanation. But I really do not follow the argument about FLake or Lake being able to account for circulation. My understanding is that these are one-dimensional i.e. vertical models.

Response: Yes, we agree with you. Such statment was deleted. See our previous response.

Reference needed here.

Response: Two most commonly used models were taken as examples, related citations

were added (Line 443-444). 'such as the widely used CESM [Oleson et al., 2013], and COSMO [Steppeler et al., 2003]'

Unclear. How the coupled models account for snow and what the specific disadvantage is not explained well enough. A clearer explanation of this when the model description is presented in an earlier section would help. But make sure your point here is explained in more detail nonetheless here.

Response: One sentence was added in line 223-224 to express how the lake surface albedo is treated with snow. 'During freeze-up, if the ice is covered with snow, then the lake surface albedo will be set to the snow albedo.'.

Further, following statement was added, 'However, such processes are not included in the model version used in current study and the applicability of such a scheme needs to be further investigated.', to better demonstrate the model disadvantage assoicated with snow. (Line 464-465)

Additionally, we want to discuss that the errors in precipitation (snow fall in cold season) simulation in the model could also bring large uncertainties in the study region. Thus, following contents were added in this paragraph (Line 466-470). 'Noting that the accurate simulation of precipitation over the complex terrain has long been a challenging issue, i.e. the precipitation in TP can be overestimated by more than 50% in climate models and even reanalysis data [Gao et al., 2015; Su et al., 2013], the simulated over lake snow fall will also bring large uncertainties in modeling the lake surface albedo and thus can influence the lake ice phenology as also demonstrated in previous study over high latitude lakes [Fujisaki-Manome et al., 2020].'.

Almost identical sentence to line 372.

Response: this sentence is removed in section 6, and we only summarize the main results and conclusions.

Following is the responses to the comments from reviewer2 in the open discussion.

The results and discussions largely agree with those of similar studies over Northern European and Canadian lakes. Unfortunately, the authors seem to be unaware about them, at least those studies are not mentioned in the large list of references.

Response:Papers from other lakes were added in the introduction and discussions. For example line 47-49, line 441-442, Line 469-470.

I would suggest a (minor) revision to improve the presentation of the models and

results. It would be good for the authors to carefully check the language and concepts used in manuscript - there are inaccuracies, sometimes misleading formulations, inconsistencies. As a non-native speaker of English I have only randomly pointed out some instances where it was not evident what the authors mean with their sentences. Some detailed comments can be found in the annotated PDF of the manuscript.

Response: Thank you very much for your thorough comments, they were revised in the new version. Additionally, we have carefully checked and revised the language editing throughout the manuscript.

Following is the responses to the comments from reviewer2 in the PDF file.

Please clarify what do you mean with long-term freeze-up?

Response: we agree 'long term' may misleading the readers. Thus, the sentence was revised to 'For example, lake freeze-up in cold season can maintain water levels by preventing evaporation losses in lakes [Lei et al., 2018].' (Line 39-40)

'Release of lake water'please clarify - artificial, natural, something else?

Response: The sentence was revised to 'The freeze-up date of a lake depends on the balance of energy storage.' (Line 43)

Please improve formulation of this sentence, it is unclear now.

'Noting that Zhou et al. [2023] proved by artificially adding a proper scaling factor on fraction velocity can better simulate the surface turbulent heat fluxes and ice phenology of Nam Co. However, such sensitivity method is hard to be regarded as advancing a climate model.'

Response: Reviewer 1 alsocomments on this point. It was revised to 'Noting that Zhou et al. [2023] proved by empirically multiply a constant on fraction velocity can better simulate the surface turbulent heat fluxes and ice phenology of Nam Co. However, such sensitivity method is lack of observation support.' (line 69-71)

Valuable data! But you did not measure close to the surface, like 20 cm below the surface that could be used instead or compared to MODIS LSWT?

Response: There is no measurements at depth less than 3m. The reason is that freeze-up of lake water and strong surface flow can destroy the instruments. Additionally, due to the long distance journey and harsh environment, the cost of long term manual duty is too high.

The whole WRF with FLake or CLM coupled? Please clarify. Perhaps also mention before this how WRF was coupled to the lake models/parametrizations - every time

step during the model forecasts, i.e. every 20 seconds or whatever is the time-step in your simulations (please mention it) or otherwise.

Response: Revised as following. (Line 135-141).

'The coupled modes were run with a time step of 10 seconds. There are 116 lake grid points in the model. The lateral boundary conditions were provided at six hourly intervals. The simulations were performed for two years from 1st July 2013 to 30th June 2015. The lake water temperature in the coupled model was initialized at 00:00 on 1st July 2013 using MODIS observations. The water temperature at the first layer was set to MODIS observed values, while the water temperature at the other layers is linearly interpolated with depth under the assumption that the water temperature at the deepest layer is equal to 3.5℃ (the temperature at maximum water density). The other variables in the coupled model are initialized by ERA_interim data at the same time.'

Your comments about the following sentences are revised. 'For both lake model setups in the coupled model, the lake depth was set to the average value of 40 m for lake grids, which was consistent with the offline study by La et al. [2016]. Thus, the lake status can be initialized by their simulation results to save computational expense for long-time spin up in the coupled model. Noting that both lake models used in the current study are one-dimensional models, such a setup with identical depth for all grids can, to some extent, reflects the horizontal energy mixing associated with lake water circulation.'

Response：To better clarify, the above sentences were revised to 'For both lake model setups in the coupled model, the lake depth was set to the average value of 40 m for all lake grid points, which was consistent with the offline lake model simulations by La et al. [2016]. Thus, to save computational expense, their simulation results were used to initialize the lake water temperature in the coupled model. Noting that both lake models used in the current study are one-dimensional models, no horizontal water flow is simulated in the coupled model. Such a model setup is a general way for modeling lake processes in a climate model, such as Community Earth System Model (CESM), Consortium for Small-scale Modelling (COSMO), as well as WRF model.' (Line167-173)

'The water extinction was set to 0.12 according to Wang et al. [2009] and Huang et al. [2019a]. The temperature at maximum water density was set to 3.5℃ according to

Wang et al. [2019b] and Wu et al. [2019].' All this was about the default setup, before your modifications tested in experiments?

Response: These parameters are based on observation, and one sentence was added to clarify 'The setup of following parameters is based on observation.' (line 173-174)

Lake surface albedo should be snow albedo when there is snow, ice albedo when there is no snow, basically when it has melt in the spring and revealed the darker ice surface. Please see if you can improve your text in these paragraphs to explain this more clearly.

Response: To better clarify, one sentence was added in the model description section. (Line 203-204). 'During freeze-up, if the ice is covered with snow, then the lake surface albedo will be set to the snow albedo'

'Such treatment of lake surface albedo is a matter of expediency due to the model disadvantage in modelling snow fall…' Do you mean that WRF produces too little snow precipitation and that makes the snow depth forecast by FLake too small?

Response: To better clarify, it is revised to 'Such treatment of lake surface albedo directly takes into account the snow influence on lake surface albedo.'. (Line 213-214)

Would you have bathymetry available? At least FLake would be able to use it.

Response: Yes, there is bathymetry data. Considering that using uniform depth in climate models is a common way, we follow this setup. Even using the real bathymetry, the lake water circulation still cannot be reflected, which is our viewpoint. Using the real bathymetry in the coupled model will be tested in the future. A general description about the bathymetry was added, to avoid duplication, citations are added in the main text. (Line 82-84).