Response to Reviewer 1

This manuscript presents a study striving to improve and calibrate an existing 1D lake model (parameterization) applied to a large artificial reservoir in China. Though the paper falls well into the journal scope, the main problems of the paper (lack of scientific novelty and methodological drawbacks) call for major revisiting of the whole study.

Response:

We thank the reviewer for all his/her efforts in evaluating our work. We have prepared point-to-point responses and revised the manuscript carefully with detailed changes (in blue in the main text and also) below.

1. The base version of the model used is WRF-Lake, used in a number of studies during recent years (Gu et al., 2015; Gu et al., 2016; Xiao et al., 2016). The authors introduce into the model physical parameterizations already tested in other 1D models, especially CLM4-LISSS (Subin et al., 2012). Increasing the vertical resolution of the model is rather a technical improvement, as it is quite evident a-priori, that having 10 numerical layers is a rough resolution for deep lakes, where only 2-3 layers would cover the mixed layer. The authors follow the same approach as in (Gu et al., 2015; Gu et al., 2016; Xiao et al., 2016) to tackle additional mixing in thermocline, which is just to multiply the molecular diffusivity by 100 or other large calibrated multipliers. This is the simplest way which does not take into account the evident physical effects like suppression of mixing by stratification. The approach by Fang and Stefan (1996) to parameterize background diffusivity takes into account stratification (eq. (9)), but the authors reject it. They argue, that original eq. (9) provides insufficient mixing. I would expect, that true development of the model physics would mean to replace primitive calibration of constant multiplier by calibration of constants in eq. (9) or likewise still simple but physically-sound parameterizations.

Response:

First of all, we would like to thank the reviewer for his/her insightful and constructive comments and critique, which provide us an opportunity to improve communication of our results. The reviewer's comments can be summarized into two main points:

1) *The reviewer considers the modification of vertical resolution a trivial technique to improve the model performance.*
2) *The reviewer deems the approach of adjusting thermal diffusivity to be unphysical (e.g., by neglecting the suppression of mixing by stratification) and asked the authors to propose a physically-sound parameterization following the formulation by Fang and Stefan (1996) (which the reviewer thought the authors rejected in this study).*

Our point-to-point responses are as follows:

1) Our vertical resolution refinement is not trivial in both the technical implementation and model performance. For the technical implementation, we did not simply increase the vertical resolution; instead, we adopted a mechanism (i.e., the often-observed exponentially varying structure of temperature profiles in deep water bodies) to allow layer thicknesses to adaptively increase with depth, which guarantees a smooth layer thickness change and thus better numerical stability. This approach is described in

Page 8 (Lines 1-6) of the revised manuscript. We demonstrated the improved performance of this new discretization scheme in simulating temperature profiles compared with the original scheme (cf. Figure 7). Also, we note this is the first time an adaptive discretization scheme has been applied in WRF-Lake. Similar adaptive discretization techniques are widely used by various geoscientific models to improve model performance, e.g., grid stretching in GFDL HiRAM (Harris et al., 2016) and nonuniform meshing in MPAS (Skamarock et al., 2018) etc. These approaches are considered by their authors to be important improvements in model structure. We have added text to Page 8 (Lines 6-8) to provide this context.

2) First, for diffusivity parameterization, our approach is based on Gu et al. (2015), whose parameterization of k_e does consider the suppression of mixing by stratification and depth (cf. equation A1). Second, we did not reject the Fang and Stefan (1996) approach but actually adopted their formulation of the enhanced D_{ed} term (i.e., equation (9): $D_{ed} = 1.04 \times 10^{-8}(N^2)^{-0.43}$) to account for unresolved 3D diffusion. However, as Subin et al. (2012) pointed out, D_{ed} is of the same order of magnitude as molecular diffusivity and therefore may not make up for the underestimated diffusivity. Unfortunately, a thorough calibration of the empirical constant in the D_{ed} formulation (i.e., 1.04×10^{-8}) is infeasible for the deep reservoir examined in this study as the necessary water temperature observations at very fine temporal and spatial resolutions were unavailable during the study period. As such, we adopted a compromise, yet effective, approach by increasing the constant by a factor of 100 (the baseline simulation (BL); Figure R1; a reprint of Figure 9 in the manuscript), which produced overall diffusivity similar to that measured at Lake Zürich (Li, 1973), a lake of similar topography and depth.

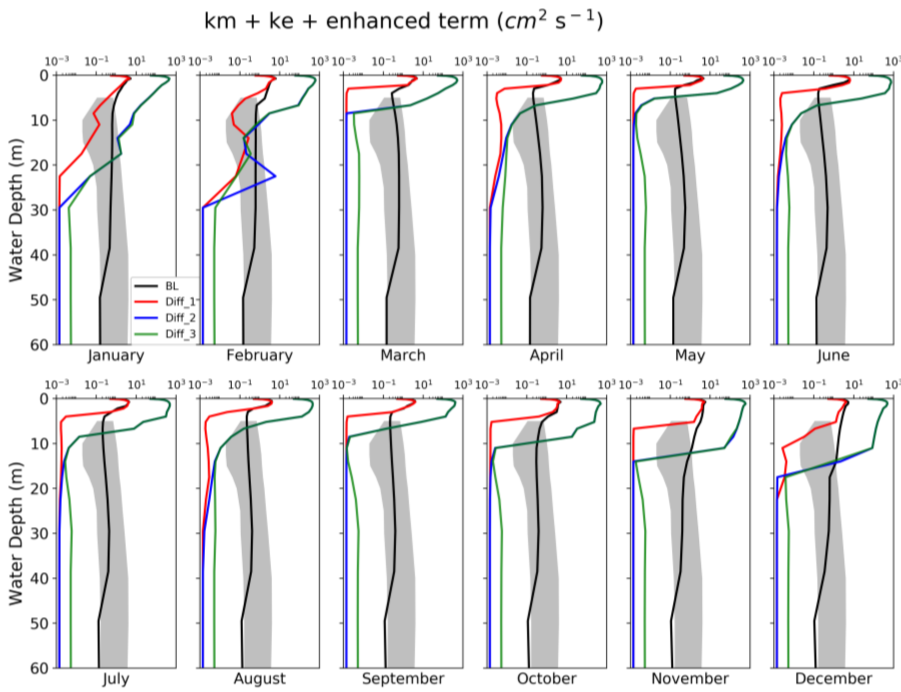

Figure R1. Monthly vertical diffusivity profile for the first 60 m water by the baseline simulation (BL; black line), Diff_1 (red line), Diff_2 (blue line), and Diff_3 (gray line) in year 2015. The gray shading indicates the diffusivity range of Lake Zürich reported by Li (1973).

A simple but physically-sound parameterization for the WRF-Lake model that goes beyond what we have done here is a long-term goal of our group, which, as the reviewer indicated may improve predictions. We are planning to develop such a parameterization using new observations that are currently being collected at our Nuozhadu Reservoir study site.

We thank the reviewer for these suggestions, and have added text to Page 8 (Lines 1-9) of the revised manuscript to better explain these points and clarify these important caveats.

2. Radiation parameterization (eq. (2)) assumes that shortwave radiation starts to decay with depth only below top 0.6 m, which is unphysical and easy to fix, assuming non-PAR (photosynthetically-active radiation) radiation to be absorbed at the surface, and PAR to be attenuated immediately below (and it is done in a such a way in almost all 1D lake models).

Response:

We thank the reviewer for the suggestion to (1) remove the 0.6 m assumption and (2) include a PAR-based separation scheme.

For the first point, we also questioned the reliability of the WRF-Lake default value for the base of the surface absorption layer ($z_a = 0.6$ m). Thus, in our original analyses we tested setting z_a to 0, 0.1, and 0.6 m (the default value). However, these three values resulted in almost no difference in water temperature profiles at the Nuozhadu Reservoir, so we simply retained the default value of z_a. However, to address the reviewer's concern, we have set z_a to be 0 m in WRF-rLake and reproduced all the figures with that value (there were no discernible differences in any of the figures).

For the second point, we note that the current radiation parameterization in WRF-Lake includes (a) an intensity-decaying formulation as a function of penetration depth following the Beer–Lambert law (Jerlov, 1976), with a revised 0 m cutoff depth and (b) a wavelength-based scheme for absorption coefficients based on a simplified parameterization of absorption coefficients (cf. Deng et al. (2013) for further discussion on the depth impacts on simulated absorption). Although separating radiation into non-PAR and PAR bands with a more mechanistic approach could be valuable, we note that many 1D lake models (Minlake: Fang and Stefan, 1996; LAKE: Stepanenko and Lykosov, 2005; CLM4-LISSS: Subin et al., 2012) and even more complicated 3D lake models (Delft3D FLOW: Hydraulics, 2003) do not currently make this distinction, and that doing so requires further information about dynamic turbidity and biological activity, which can add further model uncertainty in absence of observational constraints at our studied lake (Cristofor et al., 1994). Thus, we choose to keep the current radiation scheme in this version, but have added discussion on the potential benefits of more sophisticated approaches (such as that suggested by the reviewer) in section 2.1.2 and section 4.6 (the second paragraph) of the revised manuscript.

3. I also see a notable drawback of the paper in that no empirical constrains have been involved on water turbidity for this particular lake. I can hardly imagine that no Secchi disk measurements have been performed at all. Treating both radiation extinction coefficient and background diffusivity which are the main controls for vertical distribution of heat as calibration parameters, you may attain similar vertical temperature distributions at different combinations of those parameters, and what would be the physical sense of that?

Response:

We thank the reviewer for this advice, which would be very helpful if any improvement in the WRF-Lake radiation scheme were implemented. Unfortunately, no Secchi disk measurements were available for our study site and period. We note similar issues in the recent work by Gu et al. (2015), who also applied the default parameterization for light extinction coefficient.

We updated Page 15 Lines 8-14 in the revised manuscript to explain our choice of light extinction coefficient:

Although the default parameterization of light extinction coefficient has been applied in previous WRF-Lake studies (e.g., Gu et al., 2015), we tested the impacts of different values of this coefficient. Given that no Secchi disk measurements were available in our study site, no empirical constrains for the light extinction coefficient could be directly developed. Thus, we tested a range of light extinction coefficient values: 0.13 m^{-1} (default), 0.30 m^{-1}, 1.00 m^{-1}, and 3.00 m^{-1}. Although measurements have reported larger variability of light extinction coefficient (e.g., 0.05 to 7.1 m^{-1} in Subin et al. (2012)), we found simulated temperature profiles were insensitive to values outside of the 0.13 to 3.0 m^{-1} range.

4. The reservoir exhibits drastic surface level changes (about 30 m!), which would certainly influence the temperature profiles and introduce the vertical velocity in eq. (4), but the latter was not done, and the possible effects of level changes were not even discussed.

Response:

We agree with the reviewer that the impacts of water level change on water thermal regimes should be accounted for in WRF-Lake and we recognize that these dynamics are important features of operational reservoirs that differ from natural lakes. As such, our future work includes developing a new module for the next release of WRF-rLake (not shown in this study) to take the effects of inflow and outflow into consideration (e.g., water level change). However, to address the reviewer's concern, we have added a new section (4.6: <u>Uncertainties and limitations</u>) to discuss the effects of inflow and outflow:

Operation-induced inflows and outflows are key features of artificial reservoirs and can strongly affect seasonal and interannual evolution of reservoir surface water levels, intensity of thermal stratification, and thermal structure (Anohin et al., 2006; Çalışkan and Şebnem, 2009). Given that reservoirs are essential infrastructures for utilisation and management of water resources (Jain and Singh, 2003; Ahmad et al, 2014), the WRF-rLake framework should be extended to include reservoir operation features (e.g., inflow and outflow controls) to better characterize reservoir-atmosphere interactions.

Reference:

Çalışkan, Anıl, and Şebnem Elçi.: Effects of selective withdrawal on hydrodynamics of a stratified reservoir. Water Resources Management, 23(7), 1257-1273, 2009.

Deng, B., Liu, S., Xiao, W., Wang, W., Jin, J., & Lee, X.: Evaluation of the CLM4 lake model at a large and shallow freshwater lake*, Journal of Hydrometeorology, 14(2), 636-649, 2012.

Fang, X., & Stefan, H. G.: Long-term lake water temperature and ice cover simulations/measurements, Cold Regions Science & Technology, 24(3), 289-304, 1996.

Harris, L. M., Lin, S.-J. and Tu, C.: High-Resolution Climate Simulations Using GFDL HiRAM with a Stretched Global Grid, Journal of Climate, 29(11), 4293–4314, doi:10.1175/jcli-d-15-0389.1, 2016.

Hydraulics. (2003). Delft3D-FLOW: simulation of multi-dimensional hydrodynamic flows and transport phenomena, including sediments—user manual.

Skamarock, W. C., Duda, M. G., Ha, S. and Park, S.-H.: Limited-Area Atmospheric Modeling Using an Unstructured Mesh, Monthly Weather Review, 146(10), 3445–3460, doi:10.1175/mwr-d-18-0155.1, 2018.

Jerlov N. G.: Marine optics. Elsevier Scientific Publishing Company, Amsterdam, The Netherlands, 231 pp, 1976.

Paulson, C. A. and Simpson, J. J.: The temperature difference across the cool skin of the ocean, Journal of Geophysical Research, 86(C11), 11044, doi:10.1029/jc086ic11p11044, 1981.

Response to Reviewer 2

Overall comments:

The manuscript describes several modifications to the lake module within WRF, which are systematically included in a series of experiments, ultimately showing that these modifications result in improved model performance within a large, deep reservoir. Overall, these modifications are explained and justified well, and it is encouraging to see that they result in more accurate simulation of surface temperatures and in more realistic temperature profiles. I have identified mostly minor issues which are outlined below, and the paper should be accepted once these are properly addressed.

Response:

We appreciate the reviewer's appreciation of this work and sincerely thank him/her for these in-depth comments. We have prepared point-to-point responses and revised the manuscript carefully with detailed changes (in blue) given below.

1. The one notable drawback to this paper is that there is no evaluation of simulated ice coverage, which can be a significant factor in how some lakes interact with the atmosphere. Although this follows naturally from the fact the that the reservoir being evaluated does not appear to experience freezing temperatures, this may limit the applicability of these results to large, deep lakes that do experience freezing, such as the Great Lakes. This limitation should be discussed.

Response:

We agree with the reviewer that the ability in simulating ice coverage dynamics, or lack thereof, should be a key feature of lake models. However, as noted by the reviewer, the Nuozhadu Reservoir does not experience any ice-covered periods. Therefore, in this study, WRF-rLake could not be tested for its ability to simulate ice cover dynamics. We have added text to the revised manuscript (section 4.6: first paragraph) to address these issues:

Ice and snow processes could play a significant role in lake-atmosphere interactions (Brown and Duguay, 2010), especially for high-latitude lakes (e.g., North Eurasian lakes (Subin et al., 2012), Great Lakes (Xiao et al., 2016)). However, given the warm climatology of the Nuozhadu Reservoir, we only examined here the performance of WRF-rLake under ice-free conditions. Future work should be carried out to assess WRF-rLake performance at more reservoirs or lakes with ice-covered periods as well as different bathymetry and climate to evaluate the broader model applicability.

2. It should be clarified early on that this evaluation of the lake model in WRF is done with observed forcing data, instead of model simulated fields. I understand that the authors' intent is most likely to evaluate the lake module free from bias that may be present in the WRF-simulated fields. However, as the model is referred to as WRF- Lake, readers may assume that the coupled system is being evaluated here. The need for such analysis isn't even mentioned until the last line of the paper, but would be better placed much earlier on.

Response:

Thanks for the advice and this point is now clarified in section 3.2 of the revised manuscript as follows:

We ran the lake module off-line, driven directly by forcing data acquired from local meteorological stations rather than WRF-simulated fields, in order to evaluate the lake module free from potential biases originating in WRF.

3. Several figures (1, 2, 3) are never referenced in the text.

Response:

They are now referenced in the revised manuscript.

4. Here, observed water temperature profiles are used and the description of the experiments implies that no spin-up time was given to the model. This differs from the practice of many other modeling efforts where observed profiles of lake temperatures are not available, some of which use larger domains that include multiple lakes. In such applications, a sufficiently long spin-up would be the only way to obtain realistic temperature profiles. Clarify whether spin-up was used and discuss the implication for your results.

Response:

We did conduct a spin-up for seven days before the analysis period. This is now clarified in the revised manuscript (Page 12 Line 2).

specific comments

1. P. 2, line 11: During this time of the year, snow is enhanced around the Great Lakes, not reduced.

Response:

We have clarified in the revised manuscript that this conclusion (i.e., snow is reduced during fall and early winter) by Long et al. (2007) is only applicable to northern lakes or high-latitude lakes like the Great Bear Lake, rather than the Great Lakes (Page 2 Line 11):

During fall and early winter, when the lake surface is warmer than the overlying air, high-latitude lakes (e.g., the Great Bear Lake in Canada) release the heat collected during summer to the atmosphere, reducing snow accumulation in the surface areas around the lakes (Long et al., 2007).

2. P. 3: Works by Gula et al. (2012) and Mallard et al. (2014) (which coupled WRF to FLake in 1-way and 2-way model configurations, respectively) should be briefly mentioned alongside the discussion of the FLake model in the introduction, as it is the only other lake model that has been coupled with WRF.

Response:

Thanks for providing us with the importance references, which have been added in the revised manuscript (Page 3 Line 7-8).

3. P. 7, line 7, "approximately 10%" as 90% is included plus the 0.1 m first layer.

Response:

Corrected as suggested.

4. P. 8, first paragraph: Relationship between SH and LH and Zom is not well-explained in the earlier referred to section. Subin et al. (2012) contains equations that do relate the fluxes to aerodynamic resistance, and I suggest pointing readers to the appropriate section so they can find a more thorough discussion.

Response:

Rephrased as suggested:

Section 2.1.1: A more thorough discussion of the relationship between lake surface fluxes and aerodynamic resistances is provided by section 2.1.8 in Subin et al. (2012).

Section 2.2.2: As discussed in section 2.1.1, the aerodynamic resistances for heat (r_{ah}) and vapor (r_{aw}) heat fluxes are critical for surface energy balance predictions. The aerodynamic resistances are functions of momentum (z_{0m}) and scalar roughness lengths (z_{0h} for sensible heat and z_{0q} for latent heat).

5. P. 8, line 18: This modification for frozen lakes does not appear well-justified.

Response:

We thank the reviewer for this comment. The modification for frozen lakes is also adopted from Subin et al. (2012), which is similar to values reported by Andreas (1987), Morris (1989) and Vavrus et al. (1996). We agree that the parameterization of roughness lengths for frozen lakes should also be improved and justified. However, the Nuozhadu Reservoir is unfrozen throughout the year, so we did not make any adjustments for the effects of lake ice. We expect in future work the revised model would be applied to other reservoirs with frozen periods, which will allow more tests to be carried out to further justify the parameterization of roughness lengths.

The following sentence is added to the last paragraph of section 2.2.2:

It is worth noting that the parameterization of roughness lengths for frozen lakes could also be improved. However, as the Nuozhadu Reservoir is unfrozen throughout the year, we did not make any modifications to the representations for lake ice. Future work should investigate lakes with frozen periods to further improve the roughness length parameterization.

6. P. 9, last paragraph: K is stated to be lake dependent, but a constant for it is then specified. Does K need to be provided in each lake or is it assumed to be equal to the provided constant? Also, clarify whether the Kx100 modification is applied everywhere in lakes deeper than 50 m or if it is only applied below 50 m.

Response:

K is empirical and prescribed rather than lake dependent (Fang and Stefan, 1996). We are sorry about the confusion brought up by our previous statement. We have replaced *K* by directly applying the constant 1.04×10^{-8}.

We also clarified the statement in section 2.2.3:

Therefore, for lakes deeper than 50 m, we imposed an increase in D_{ed} by a factor of 100 for all layers and argue that more analyses are required to robustly represent unresolved turbulence.

7. P. 10. It is stated that this reservoir provides a good example of the impacts of artificial water bodies on regional climate, but this focus is not put in further context. Why did the authors choose to study an artificial body instead of a natural one?

Response:

We thank the reviewer for bringing up the very valuable concern on the broader impacts of this study (i.e., influence on regional climate of human exploitations of water resources). We have added discussion to the revised manuscript regarding the uniqueness of artificial reservoirs compared to natural lakes (section 4.6: Uncertainties and limitations). Please refer to our response to the 4th comment by Reviewer 1.

We also noted in the revised conclusion that:

Our future work will couple the WRF-rLake module with the WRF framework to examine the performance of the coupled system.

8. P. 11 first paragraph: As the LW and SW data are interpolated from 3 hourly observations, peak radiation values may be underestimated. This should be stated in the text.

Response:

Discussion on the underestimated peak values are now added to Page 12 Line 5-6:

Although it probably underestimates peak radiation values, linear interpolation may still be considered to be an acceptable approximation given no data of higher temporal resolution is available.

9. Fig. 4. Label the y-axis. Also, clarify that the "water level" shown (according to the inset box) is not actually the water level (which, having a mean of 812 m, does not seem to be consistent with the values shown here).

Response:

The y-axis issue has been fixed in the revised Figure 4.

For the water level, "812 m" mentioned in P. 10, line 14 refers to the "normal water level" of the reservoir, rather than average water level. For a reservoir whose outflow is controlled wholly or partially by movable gates, normal water level is the maximum level to which water may rise under normal operation conditions. So, it is normal for the water level to fall below 812 m throughout the year of 2015. The explanation of the term "normal water level" is added as a footnote in section 3.1.

10. Table 3 "Roughness Lengths" column: I believe the constants given here refer to the roughness lengths for unfrozen lakes, based on previous discussion, but this should be clarified.

Response:

Clarified as suggested in a new note for Table 3.

11. Figure 5 and other similar figs: The observed temperatures shown here were taken near the dam of the reservoir. Are the simulated LSTs taken and averaged over a similar area or are they representative of lake-average conditions? If it's the latter, then direct comparison to observations over a smaller subset of the lake would be problematic, as temperatures from shallow and deep portions of the reservoir are averaged together.

Response:

The former: we used simulation results of an area near the dam, where the observations were collected, to conduct the evaluation. This point is now clarified in the revised manuscript as follows:

Section 4.1: The simulation results near the dam, the same place where the observations were collected, were used to conduct the evaluation.

12. Figure 5: Why was Diff_3 included here and no other sensitivity run?

Response:

This was intentionally chosen for better legibility: Diff_1, Diff_2 and Diff_3 would overlap if put together as their differences are more pronounced in temperature profiles compared with the surface temperature (cf. Figure 8). This is now clarified in the revised manuscript:

Section 4.1: Here the results of other diffusivity experiments (i.e., Diff_1 and Diff_2) are not shown.

13. P. 15, line 10: "by as much as ~1.3C"?

Response:

The phrase "up to" is added to make the statement more precise.

14. Table 4: Coloring indicates the smallest and largest absolute values.

Response:

Colored as suggested.

15. P. 17, line 9: "in top 10-m temperatures".

Response:

Corrected as suggested.

16. P. 18: Consider including RMSE or other error metric here, as done in the previous section, as Diff_1 and 2 both contain over and underestimates of temperatures in the profiles and a quantitative measure would be valuable to the reader.

Response:

Thanks for the valuable suggestion. We have added more metrics as suggested in Table R1 in the revised manuscript:

BL yields the smallest RMSE of 1.13 °C against monthly observed lake temperatures profiles, while Diff_1, Diff_2, and Diff_3 yield 1.62 °C, 1.47 °C, 1.47 °C, respectively.

Table R1. Statistics of the discrepancy between simulated (BL, Diff_1, Diff_2, and Diff_3) and observed monthly temperature profiles during year 2015. Coral and green coloring indicate the largest and smallest absolute values among three simulations, respectively.

		BL	Diff_1	Diff_2	Diff_3
Monthly Temperature Profile	RMSE (°C)	1.13	1.62	1.47	1.47
	MBE (°C)	0.57	-0.27	0.32	0.32
	Max Bias (°C)	3.39	4.56	5.64	5.63
	Min Bias (°C)	-1.37	-4.32	-3.61	-3.58
	MAE (°C)	0.84	1.23	1.11	1.10

17. Figures 8 & 9, 10 & 11: Keep coloring for runs consistent between plots.

Response:

The coloring is now made consistent between these plots.

18. Figure 9: The logarithmic axes here make it hard to put the simulated values in context with the observations from Li (1973). Consider using gray shading in the background to plot the observed range directly on the figure for comparison.

Response:

Thanks for the valuable suggestion. We have added the shading in Figure 9 of the revised manuscript to indicate the observed values reported in Li (1973). Also, please refer to our response to the first comment by Reviewer 1 (Figure R1).

19. P. 21: Use "are fixed to 1 mm (Rou_1)" on line 7 and "at 10 mm (Rou_2)" on line 10 for greater clarity.

Response:

Corrected as suggested.

20. P. 23, line 2: "minimal changes to LSTs"

Response:

Corrected as suggested.

Reference:

Andreas, E. L.: A theory for the scalar roughness and the scalar transfer coefficients over snow and sea ice, Boundary-Layer Meteorology, 38(1-2), 159–184, doi:10.1007/bf00121562, 1987.

Morris, E.: Turbulent transfer over snow and ice, Journal of Hydrology, 105(3-4), 205–223, doi:10.1016/0022-1694(89)90105-4, 1989.

Vavrus, S. J., Wynne, R. H. and Foley, J. A.: Measuring the sensitivity of southern Wisconsin lake ice to climate variations and lake depth using a numerical model, Limnology and Oceanography, 41(5), 822–831, doi:10.4319/lo.1996.41.5.0822, 1996.

Response to Editor

Dear Editor:

We thank you for the opportunity to respond to the reviewer concerns. In previous parts of this document, we have prepared detailed responses to all of the comments, and feel that doing so has substantially improved communication of our results in the revised manuscript.

Reviewer #1:

Reviewer #1 has indicated a number of concerns which do not seem to have been addressed in the revision. I recommend you further review these comments. Note that further peer review may then be required.

Response:

Please see our detailed responses to each of Reviewer #1's concerns in this document.

1) No changes seem to have been made to the paper to acknowledge this comment noting, for example, that you change both the number of layers and their distribution, and that there are many ways in which this could be done.

Response:

Please see our response to Reviewer #1's first comment. Changes are now made to section 2.2.1 in the revised manuscript to better explain and justify our approach. The updated text is annotated and related references have been added to the manuscript.

2) I think the text around equation (9) needs to be clarified. The final sentence of page 9 is particularly unclear.

Response:

The units for equation (9) are specified in the text, and the last sentence was updated with a clearer statement.

2. Discussion has been added to the response but not the paper.

Response:

The following discussion is added in the revised manuscript to the last paragraph of section 2.1.2:

Though there exist more sophisticated radiation schemes in other lake models (e.g., the 9-band scheme by Paulson and Simpson, 1981), we kept the current WRF-Lake radiation scheme since it includes the essential components in a waterbody physical radiation parameterization: an intensity-decaying formulation as a function of penetration depth following the Beer–Lambert law (Jerlov, 1976) and a scheme for absorption coefficients. Such an approach is also accepted by many other 1D lake models (Fang and Stefan, 1996; Stepanenko and Lykosov, 2005). To improve the model performance, we tentatively set the cutoff depth z_a to be 0 m in this version as 0.6 m is usually an overestimated value, especially for shallow lakes (Deng et al., 2013). Although adopting this z_a value (0 m) demonstrates acceptable performance in this work, a more lake-specific cutoff depth may be needed for better model performance.

We also updated our response to the reviewer regarding this point.

3. You have acknowledged the drawback in the response but not the paper.

Response:

The text of the fifth paragraph in section 3.4 of the revised manuscirpt has been updated to acknowledge this omission:

Light extinction coefficient ("Ext" set): through model tests, we conclude that in addition to the schemes we modified, the light extinction coefficient is also a key parameter for accurately modelling deep lakes (Hocking and Straskraba, 1999). Although the default parameterization of light extinction coefficient has been applied in previous WRF-Lake studies (e.g., Gu et al., 2015), we tested the impacts of different values of this coefficient. Given that no Secchi disk measurements were available in our study site, no empirical constrains for the light extinction coefficient could be directly developed. Thus, we tested a range of light extinction coefficient values: 0.13 m^{-1} (default), 0.30 m^{-1}, 1.00 m^{-1}, and 3.00 m^{-1}. Although measurements have reported larger variability of light extinction coefficient (e.g., 0.05 to 7.1 m^{-1} in Subin et al. (2012)), we found simulated temperature profiles were insensitive to values outside of the 0.13 to 3.0 m^{-1} range. We concluded that the best performance could be achieved by increasing the light extinction coefficient to ~1.00 m^{-1}, which thus is adopted by our baseline run (BL).

4. You have added only a very short paragraph in response to the reviewer comment.

Response:

In our current response and revised manuscript, we have added more detail in a new section (4.6: Uncertainties and limitations) to discuss the effects of inflow and outflow:

Operation-induced inflows and outflows are key features of artificial reservoirs and can strongly affect seasonal and interannual evolution of reservoir surface water levels, intensity of thermal stratification, and thermal structure (Anohin et al., 2006; Çalışkan and Şebnem, 2009). Given that reservoirs are essential infrastructures for utilisation and management of water resources (Jain and Singh, 2003; Ahmad et al, 2014), the WRF-rLake framework should be extended to include reservoir operation features (e.g., inflow and outflow controls) to better characterize reservoir-atmosphere interactions.

Reviewer #2:

Overall comments:

1. The quoted text in the response is not as it appears in the paper.

Response:

Thanks for this reminder. We have added a new section 4.6: Uncertainties and limitations, to acknowledged the drawback of evaluation without ice coverage period:

Ice and snow processes could play a significant role in lake-atmosphere interactions (Brown and Duguay, 2010), especially for high-latitude lakes (e.g., North Eurasian lakes (Subin et al., 2012), Great Lakes (Xiao et al., 2016)). However, given the warm climatology of the Nuozhadu Reservoir, we only examined here the performance of WRF-rLake under ice-free conditions. Future work should be carried out to assess WRF-rLake performance at more reservoirs or lakes with ice-covered periods as well as different bathymetry and climate to evaluate the broader model applicability.

We have updated our response to the reviewer accordingly.

Specific comments:

5. You do not seem to have discussed or addressed the reviewer comment in the paper.

Response:

To address this reviewer comment, the following statement is added to the last paragraph of section 2.2.2:

It is worth noting that the parameterization of roughness lengths for frozen lakes could also be improved. However, as the Nuozhadu Reservoir is unfrozen throughout the year, we did not make any modifications to the representations for lake ice. Future work should investigate lakes with frozen periods to further improve the roughness length parameterization.

The response to the reviewer is updated accordingly.

6. I do not understand the current text around equation (9). This lacks units, and I find the final sentence of page 9 to be unclear.

Response:

We have added the unit for D_{ed} ($m^2\ s^{-1}$) in the paper. As N has a unit of s^{-1}, the unit for the empirical constant 1.04×10^{-8} should be $m^2\ s^{-0.14}$. The last sentence on page 9 is further clarified in the revised manuscript.

8. The quoted text in the response is not as it appears in the paper.

Response:

The quoted text in the response is updated to be consistent with that in the paper.

13. I do not understand your response.

Response:

The updated response is as follows (response to reviewer 2):

The phrase “as much as” is added to make the statement more precise.

17. Figures 9, 11: The caption colors do not match the legend.

Response:

The captions are corrected.

Other comments:

1. The article is missing an "Author Contribution" section

Response:

The "Author Contribution" section is added.