**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) given 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.*

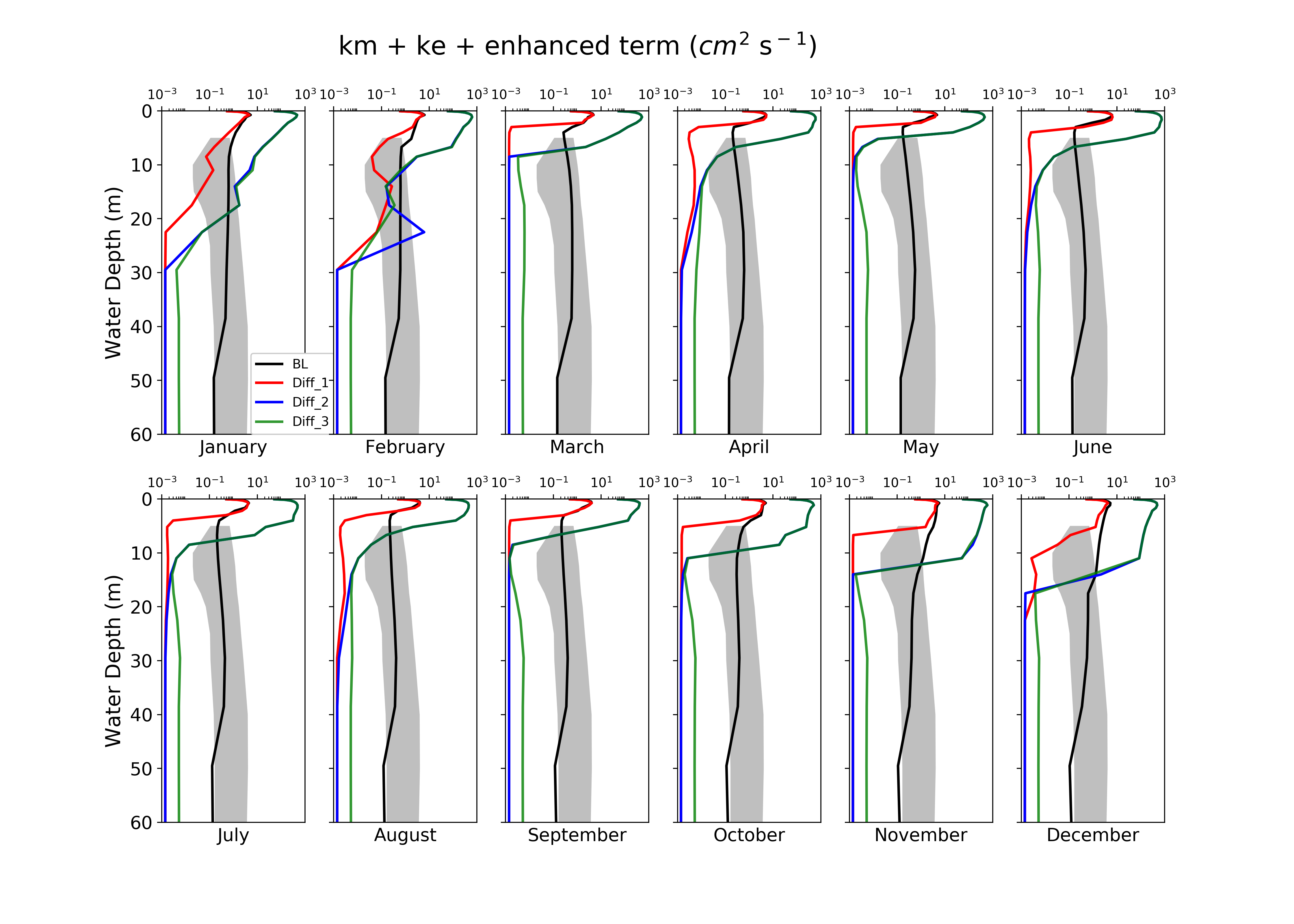
**Response:**

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 elaborate our thinking. We are really grateful for that. The reviewer’s comments can be essentially summarized as follows:

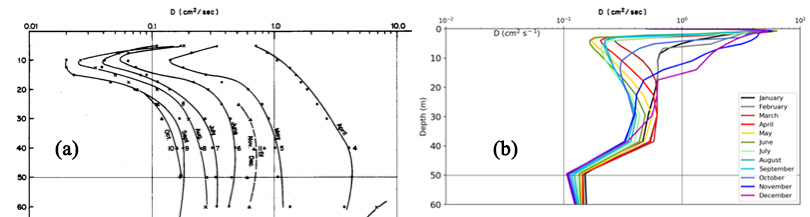
1. *The reviewer considers the modification of vertical resolution a trivial technique to improve the model performance.*
2. *The reviewer deems the approach of this study to adjust thermal diffusivity unphysical (e.g., neglect of suppression of mixing by stratification) and asked the authors to propose 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. The 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 based on fixed factor (FF) to allow layer thicknesses adaptively increase with depth, which guarantees a smooth thickness change and thus better numerical stability. For the model performance, we clearly demonstrated the outperformance of this new discretization scheme in simulating temperature profiles compared with the original scheme (cf. Figure 7). Also, we note this is for the first time an adaptive discretization scheme being introduced to WRF-Lake. Also, similar adaptive discretization techniques are widely used by various geoscientific models to improve the model performance (e.g., grid stretching in GFDL HiRAM (Harris et al., 2016), nonuniform meshing in MPAS (Skamarock et al., 218), etc.), which can hardly be considered as trivial modifications in model development.
2. First, we CANNOT agree with the reviewer that our approach is unphysical because this approach is based on Gu et al. (2015), whose parameterization of 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 term (i.e., equation (9): ) to account for the unresolved 3D diffusion. However, as Subin et al. (2012) pointed out, is of the same order of magnitude of molecular diffusivity and may not make up for underestimated diffusivity. Also, a thorough calibration of empirical constant in (i.e., ) is infeasible for the deep reservoir examined in this study as the necessary water temperature observations of very fine temporal and spatial resolutions were unavailable during the study period. As such, we adopted a compromised but effective approach by enlarging the constant with a factor of 100, which produced overall diffusivity similar to that measured at Lake Zürich (Li, 1973), a lake of similar topography and depth: see Figure R1 (a reprint of Figure 9 in the manuscript) and Figure R2.



**Figure R1.** Monthly vertical diffusivity profile for the first 60 m water by 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).



**Figure R2.** Diffusivity profiles of different months from a) observations reported in Li (1973) and b) simulations of this study.

Besides, we deem a simple but physically-sound parameterization is necessary for the WRF-Lake model as the reviewer expected, which we aim to propose based on new observations that are being collected at our study site, the Nuozhadu Reservoir.

*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’s suggestion for a PAR-based separation scheme, and we deem a more site-specific cutoff depth for WRF-Lake (which is currently set as 0.6 m) is arguably needed. However, we CANNOT agree with the reviewer that the design for a cutoff depth by the current radiation parameterization in WRF-Lake (Bonan, 1995; Subin et al., 2012) is unphysical.

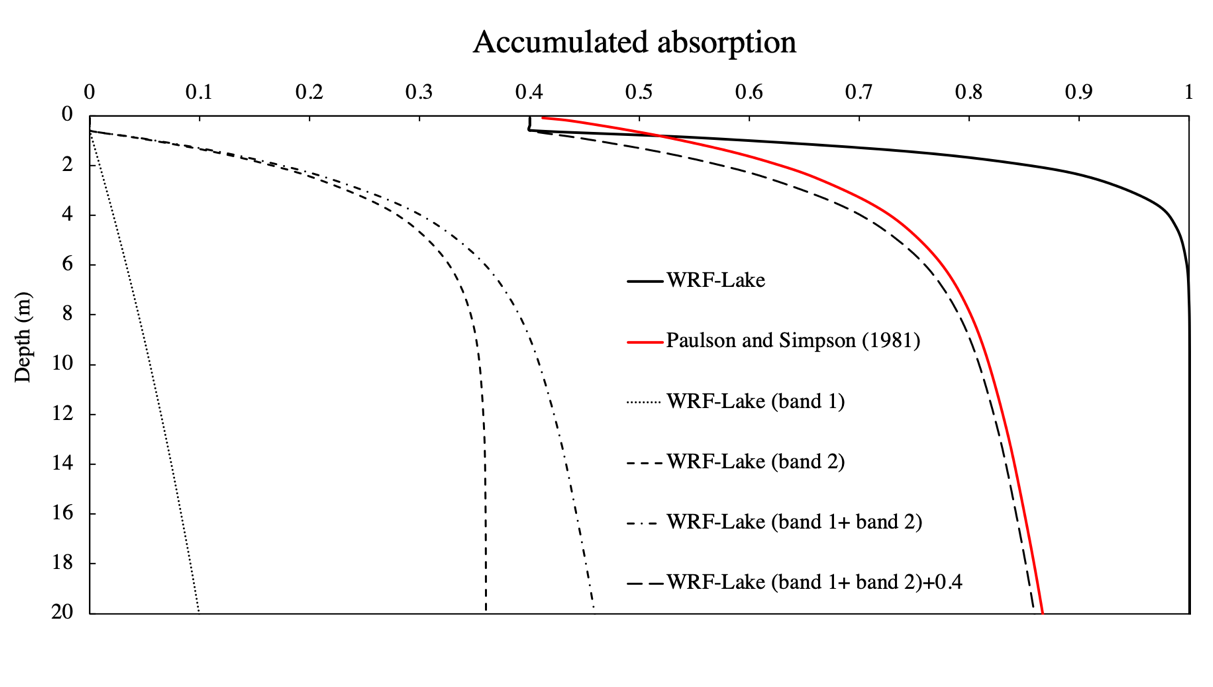
We deem the essential components in a physical radiation parameterization for water body should at least include:

1. An intensity-decaying formulation as a function of penetration depth following the Beer–Lambert law (Jerlov, 1976).
2. A wavelength-based scheme for absorption coefficients.

And we deem the current radiation parameterization in WRF-Lake does include all the two essential components: the first component is formulated with a cutoff at a certain depth (e.g., 0.6 m in the current WRF-Lake) and second adopts a simplified parameterization of absorption coefficients (cf. Deng et al. (2013) for further discussion on the depth impacts on simulated absorption).

By comparing the WRF-Lake radiation scheme with a more sophisticated 9-band scheme (Paulson and Simpson, 1981) (Figure R3), we can see that 1) the cutoff depth set by WRF-Lake essentially incorporates absorption effects of all minor bands (i.e., band 3 – 9) in the topmost part and 2) the simplified parameterization of absorption coefficients demonstrates good consistency with Paulson and Simpson (1981) scheme in the absorption effects of major bands (i.e., band 1 and 2) at deeper depths.

As such, we deem the current radiation parameterization in WRF-Lake is physical and performs reasonably well in capturing the absorption effects of water bodies.



**Figure R3.** Shortwave radiation absorption simulated by WRF-Lake and Paulson and Simpson (1981). *WRF-Lake* applies the same extinction coefficient as the manuscript (i.e., ). *WRF-Lake (band 1)* and *WRF-Lake (band 2)* applies the same extinction coefficient as band 1 and band 2, representatively.

*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:**

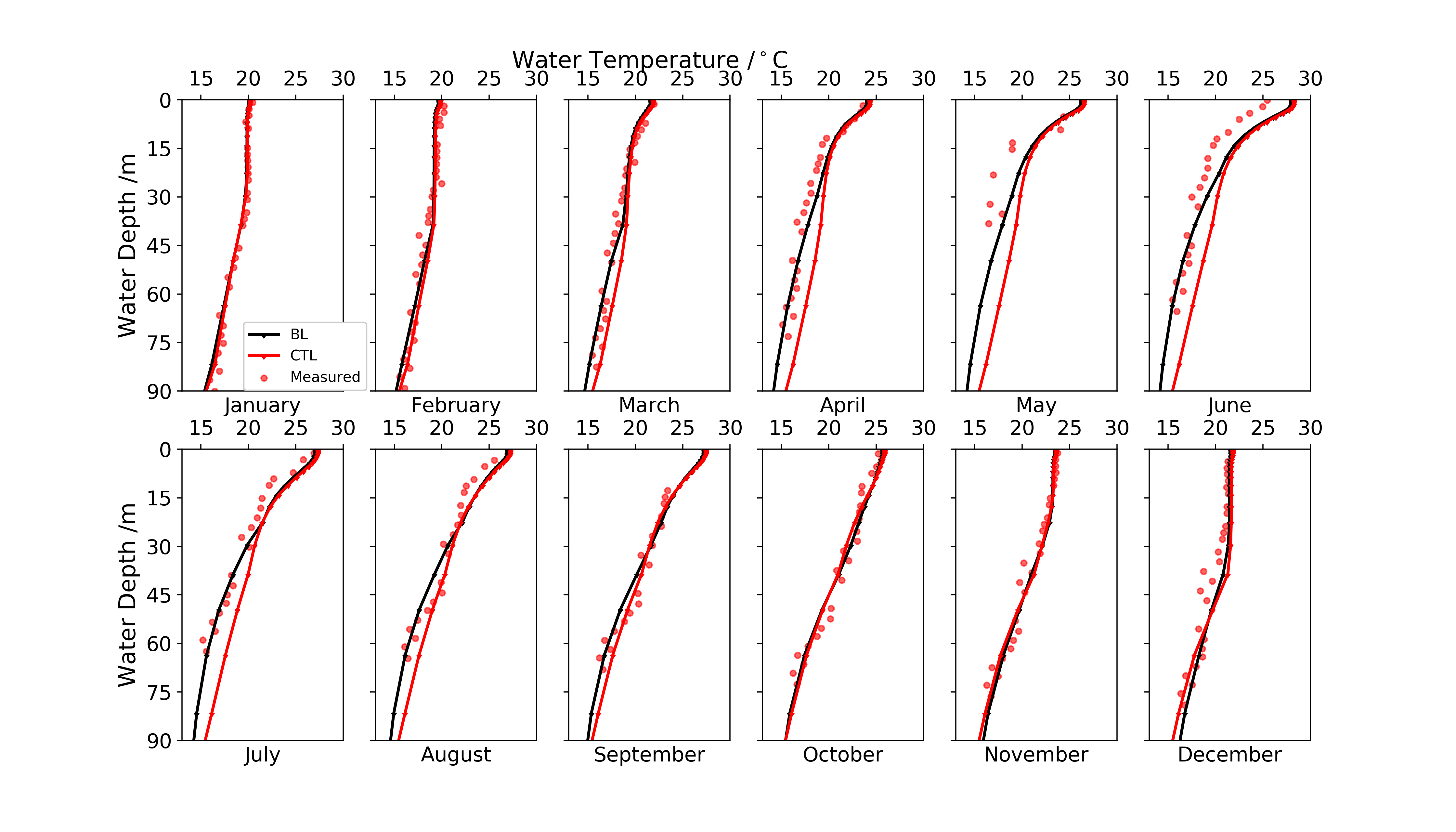
We thank the reviewer for this advice, which would be very helpful if any improvement in radiation scheme of WRF-Lake should be implemented. However, given the infeasibility of deployment of Secchi disk measurements in our study site (a very deep reservoir under frequent operation), the improvement of radiation scheme in WRF-Lake had to be on hold in this study. We also note a similar difficulty undergone by Gu et al. (2015) who ended up with default parameterization for light extinction coefficient as well.

*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 fully agree with the reviewer that the impacts of water level change on water thermal regimes should be accounted for in WRF-Lake and we deem such impact is one of the signatures of operational reservoirs that differ from natural lakes.

As such, we have been 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). The preliminary results (Figure R4) of the next release of WRF-rLake (BL, black lines) can better reproduce the temperature profile below 20 m compared with the current release described in this study (CTL, red lines), in particular for warm months (April, May, June, and July).



**Figure R4.** Monthly vertical temperature profile for the first 90 m water in year 2015 by observation (red dots), BL (the next release of WRF-rLake) and CTL (WRF-rLake).

As the complete results of next release of WRF-rLake are not ready for presentation in this paper, to address the reviewer’s concern, we have added discussion on the effects of inflow and outflow in the revised manuscript:

Besides, the operation-induced in/outflow, a key difference from natural lakes, is yet to be considered; given reservoirs as essential infrastructures for utilisation and management of water resources (ref), the WRF-rLake framework can be extended with operation-aware features (e.g., in/outflow parameterization) for reservoirs to better characterise the reservoir-atmosphere interactions under more realistic anthropogenic influences.

**Reference:**

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

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