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Interactive comment on "Evaluation of the WRF lake module (v1.0) and its improvements at a deep reservoir" *by* F. Wang et al.

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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.

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 multiplier. 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. 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). 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? 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.

I suggest that this paper cannot be accepted and the authors should significantly change the methodology of their study prior to submitting the manuscript again.

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