Geoscientific
Model Development
Discussions

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

Anonymous Referee #2

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Review for "Evaluation of the WRF lake module (v1.0) and its improvements at a deep reservoir", 2018, by Wang et al.

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.

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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.

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.

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

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.

Specific comments:

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

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 men-

tioned alongside the discussion of the FLake model in the introduction, as it is the only other lake model that has been coupled with WRF, according to my knowledge.

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

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.

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

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.

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?

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

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).

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.

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 later, 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.

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

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

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

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

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.

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

18. Figure 9: The logarithmic axes here makes 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.

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.

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

References:

Gula, J. and Peltier, W. R.: Dynamical downscaling over the Great Lakes basin of North America using the WRF regional climate model: the impact of the Great Lakes system on regional greenhouse warming, J. Climate, 25, 7723–7742, 2012.

Mallard, M. S., Nolte, C. G., Bullock, O. R., Spero, T. L., and Gula, J.: Using a coupled lake model with WRF for dynamical downscaling, J. Geophys. Res., 119, 7193–7208,

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