**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 recognition of value of this work and sincerely thank the 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 noticed by reviewer, given the Nuozhadu Reservoir does not experience ice-covered period, WRF-rLake could not be tested for its ability in simulating ice cover dynamics and this limitation has been discussed in the revised manuscript (last paragraph of the conclusion):

We also have to admit this study has its limitations as it is conducted at only one ice-free reservoirs. Evaluation of the parameterizations, discretization, and sensitivity analyses recommended here for the Nuozhadu Reservoir should be performed at other reservoirs or lakes with different bathymetry and climate, especially those with ice-covered periods.

*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 is clarified in section 3.2 of the revised manuscript as follows:

The lake module was run off-line, i.e., driven directly by the forcing data acquired from local meteorological stations rather than WRF-simulated fields, in order to evaluate the lake module free from any potential biases originated from 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.

***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:

During fall and early winter, when the lake surface is warmer than the overlying air, high-latitude lakes (like the Great Bear Lake) 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.

*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:

section2.1.1: A more thorough discussion of the relationship between lake surface fluxes and aerodynamic resistances is provided by Subin et al. (2012) in section 2.1.8.

section2.2.2: As discussed in section 2.1.1, the aerodynamic resistances for heat () and vapor () heat fluxes are critical for surface energy balance predictions. The aerodynamic resistances themselves are functions of momentum () and scalar roughness lengths ( for sensible heat and for latent heat).

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

**Response:**

Thanks for the reviewer’s advice. The modification for frozen lakes is also adopted from Subin et al. (2012), which is roughly consistent with literature values (Andreas, 1987; Morris, 1989; Vavrus et al., 1996).

We agree the parameterization of roughness lengths for frozen lakes should also be improved and justified. However, as the Nuozhadu Reservoir is unfrozen throughout the year, no data is available now to develop or validate a physically-sound parameterization. We also believe that in our future work, more tests on lakes with frozen periods should be carried out to further justify the parameterization of roughness lengths.

*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 .

We also clarified the statement in section 2.2.3:

We therefore evaluated an increase in by a factor of 100 for all layers in lakes deeper than 50 m 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), which should have been but was less stated in the previous manuscript.

We revised the conclusion as follows to address this concern:

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 (Jain and Singh, 2003; Ahmad et al, 2014), 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. In addition, our future work will couple WRF-rLake module with the whole WRF framework to further examine the online performance of 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 as follows:

Although probably underestimating peak radiation values, linear interpolation may serve as an acceptable approximation as no data of higher temporal resolution is available. We are also planning installing our own meteorological stations near the reservoir, once accomplished, forcing data of finer temporal resolution would be available and will be applied in our study.

*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 is now clarified in the revised manuscript as follows:

section4.1: 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 collapse 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:

section4.1: Here the results of other diffusivity experiments (i.e., Diff\_1 and Diff\_2) are not shown for better legibility.

*13. P. 15, line 10: “by as much as ∼1.3C”?*

**Response:**

Colored as suggested.

*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.*

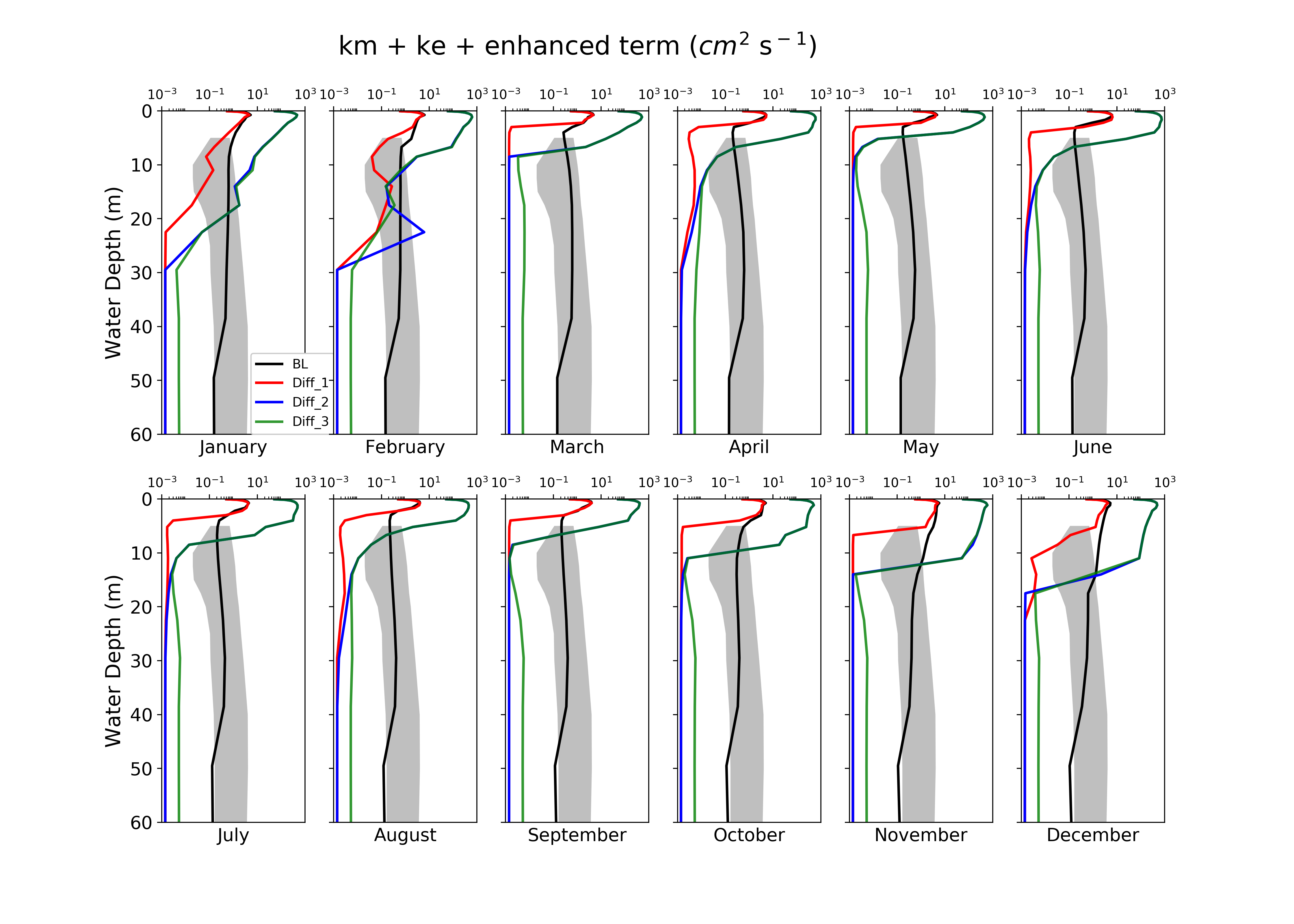
**Response:**

Corrected as suggested.

*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.*

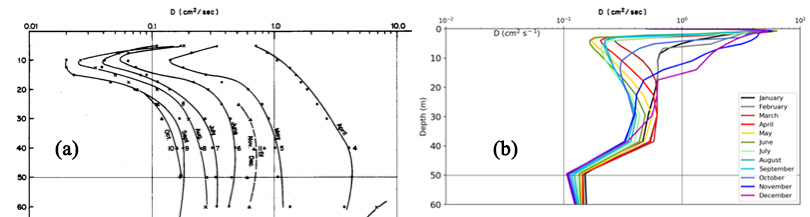
**Response:**

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):



**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 estimated by Li (1973). (This is a reprint of Figure 9 in the manuscript)

Also, we put the simulated diffusivity of all months in one figure to compare with the data from Li (1973) for clearer illustration (Figure R2). As shown in the following figure. The left part is observation from Li (1973), the right part is simulated by updated WRF-rLake. For most months, the observed diffusivity for the main water body ranges between 0.1~1.0 cm2 s-1 and the simulation agrees with observation well.



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

*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.