

Geosci. Model Dev. Discuss., author comment AC2
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Reply on RC2

Manuel C. Almeida et al.

Author comment on "Modeling reservoir surface temperatures for regional and global climate models: a multi-model study on the inflow and level variation effects" by Manuel C. Almeida et al., Geosci. Model Dev. Discuss.,
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Anonymous Referee 2: In this work, Almeida et al. compared the performance of 2-D lake models with/without accounting for lateral flow, 1-D lake models (Hostetler-based and FLake) and data-based ANN models in simulating the thermal regimes of 24 reservoirs in Portugal. They demonstrated that for reservoirs with short WRT, it is important to represent the effect of lateral flow and water level fluctuation in the lake models of GCMs and RCMs. Although the importance of lateral flow in the thermal regimes of reservoirs has been investigated by previous studies, the work of Almeida et al. is novel in three aspects: 1) the investigation of a large set of reservoirs, 2) the inclusion of ML methods, and 3) the comparison of multiple 1-D lake models. The manuscript is well written and easy to follow. I agree with the comments of the first reviewer and provide additional comments. I recommend the publication of this work after these comments are addressed.

RESPONSE: We would like to thank the reviewer for the time taken and for the thoughtful comments and valuable suggestions for improving our manuscript. To facilitate the work of the reviewers and editor, we will refer to the line numbers in the original manuscript when describing the revisions made.

Major comments

COMMENT: First, I possibly misunderstood but it seems that the 1-D Hostetler-based model was developed by the authors for this work. If so, I do not quite understand the rationale because there are many well-tested Hostetler-based models that have already been publicly available, such as WRF-Lake. As a lake modeler myself, I worry that the development of a new model would unavoidably introduce bugs.

RESPONSE: Thank you for pointing out this fact. The "Hostetler-type" model was not developed specifically for this work. The model was previously applied in a PHD thesis (<http://hdl.handle.net/10362/11982>) and followed the straightforward Hostetler (Hostetler and Bartlein, 1990) approach. We agree that potential bugs might be occasionally introduced by re-coding even a simple model like this one but preferred to have full control over all components of at least one 1-D model. By doing so, we were able to reproduce the behavior of the Hostetler model, as implemented in various systems

including WRF, and to additionally refine the model eddy diffusivity parameterization.

COMMENT: Second, it looks that the 1-D lake models were not calibrated in the study but the ANN model because it is based on the 2-D reservoir model was implicitly calibrated. Thus, in my view, the comparison of their performance in the current format is unfair. According to my own experience, by calibration, 1-D lake models can also mimic some effect of lateral flow and water level change. But whether this is physically sound is another story. However, my point is that the current experiment design does not convince me the superior of ANN over 1-D models in representing lake thermal dynamics for GCMs and RCMs because when we have data to train ANN we can also use the data to calibrate 1-D models.

RESPONSE: Thank you for this comment. We understand the reviewer's concern. In our opinion, the comparison of the models' results - 1-D models *versus* ANN - would be unfair only if the terms of the comparison were unknown. Simple models like FLake and Hostetler are coupled with numerical weather prediction models, due to their computational efficiency, but also because their parameters should not be re-evaluated when the model is applied to a specific lake. This is the principle that guided the development of both models. Also, this is the reason why the parameterization of eddy diffusion described by Hostetler and Bartlein (1990) followed the Henderson-Sellers (1985) method instead of parameterizations requiring individual model calibration (e.g., Sundaram and Rehm, 1973). It is not feasible to calibrate dozens or hundreds of lakes for numerical climate prediction. Therefore, to estimate the performance of 1-D models in the way they are applied in regional and global models we did not calibrate them during the development of this work.

At this point, in our opinion, a question needs to be answered: what is the way forward when it comes to improving on or reducing the impact of all of the above-mentioned limitations, in particular the neglect of horizontal transport process?

We agree with the reviewer. Through the calibration of 1-D models it is possible to "mimic" some effect of lateral flow but in our opinion this is not the best way to address the issue. We think that, by forcing other parameters or constants, we are probably unbalancing the model's response in certain specific conditions. Moreover, as the reviewer says, we do not ensure a physically sound response. Could the solution for improving the parameterization of lakes inflows and outflows be in the consideration of a simplified hydrological model? Reducing this approach to its basics: we would compute inflows from precipitation, taking into consideration a constant runoff coefficient and a constant lake outflow. In our opinion, considering that we need to avoid the calibration of the model, this solution could also substantially increase the errors associated with surface-water temperature predictions.

This was the reason why we have included the ANN in our study. We think that progress in improving the parameterization of lakes in the climate system can be obtained by a combination of both approaches: process-based physical models and machine-learning solutions, when the limitations and advantages of each of them have been considered. It is true that the use of machine-learning approaches relies on the existence of training data that can sometimes be difficult to obtain. We think that, with the constant development of remote-sensing technologies, this limitation can be considerably diminished. It is also important to mention that, after the initial work of defining the neural network and all its components is done, the ANN needs to be trained not calibrated, which is different. In our study we show that this approach can be a good solution for this problem.

Specific comments

COMMENT: L22-24: as indicated above, I do not think the current results can make such a statement. Further, there is another difficulty for ANN models to replace 1-D lake models in GCMs and RCMs. Compared with ANN models, 1-D lake models are much more generalized because they are physically based. For example, due to the limitation of model resolutions, usually the lake grid cells in GCMs and RCMs do not directly correspond to real lakes. We still do not know whether ANN models trained by data from real lakes can be extended to artificial lake grid cells.

RESPONSE: Thank you for your comment. We understand the reviewer's concerns. To clarify our point, we have included the following sentence in the revised version of the manuscript in line 24.

"Overall, results suggest that the combined use of process-based physical models and machine-learning models will considerably improve the modeling of air-lake heat and moisture fluxes."

We think that the result is quite balanced because we say that: "Our findings also highlight the efficiency of the machine-learning approach, which may overperform process-based physical models both in accuracy and in computational requirements, if applied to reservoirs with long-term observations available." We do not say that machine-learning approaches are better, only that they may perform better in certain conditions.

The heat fluxes retrieved from the output of an ANN will affect the near-surface atmospheric layer in the same way as a physically based model. The type of output of both models is precisely the same. In our opinion, the mismatch of the lake grid cells in GCMs and RCMs with the real lake dimensions is indeed a problem, but it is a problem for the physically based model - whose performance is greatly affected by the quantification of the lake maximum and mean depths. Our concern regarding the coupling of an ANN with a GCM or RCM relies more on the implementation of the training phase of the ANN. Nonetheless we believe that this constraint, with time, can be overcome.

COMMENT: L60: please add references to other Hostetler-based models that are involved in the ISIMIP lake sector, such as Tan et al. (2015). "Tan, Z., Zhuang, Q., & Walter Anthony, K. (2015). Modeling methane emissions from arctic lakes: Model development and site-level study. *Journal of Advances in Modeling Earth Systems*, 7, 459-483."

RESPONSE: Thank you for pointing this out. The references were included.

COMMENT: L66: please cite the most recent modeling intercomparison study of Guseva et al. (2020). "Guseva, S., Bleninger, T., Jöhnk, K., Polli, B. A., Tan, Z., Thiery, W., ... & Stepanenko, V. (2020). Multimodel simulation of vertical gas transfer in a temperate lake. *Hydrology and Earth System Sciences*, 24, 697-715."

RESPONSE: Thank you for pointing this out. The reference was included.

COMMENT: Table 1: Did you use the bathymetry data of the 24 reservoirs to setup the models? Or did you only use mean depth, maximum depth and surface area to construct ideal bathymetry for these reservoirs? Sometimes, the uncertainty in bathymetry can introduce large uncertainty in 2-D lake modeling.

RESPONSE: Thank you for this question. Yes, we used the bathymetry data retrieved from 1:25000 topographic charts of the future flooded watersheds area, prior to the dams' construction. We understand the reviewer's concern, as uncertainty in bathymetry can indeed affect considerably 2-D model results. The majority of the 2-D models considered here were also used for water-quality research studies which were finalized before the development of this manuscript. Therefore, they were thoroughly tested. In order to address a comment by reviewer 1, we have included the abovementioned information and a table with the grid dimensions of each reservoir.

COMMENT: L152-154: please rewrite this sentence. It is difficult to understand.

RESPONSE: Thank you for pointing this out. This sentence has been rewritten.

Line 152-153: "SWT time series were compared using statistic error measures (see Sect. 3.3 for more details), which allowed the assessment of the relation between reservoir WRT and the error that results when the advection due to inflows and outflows is neglected (as mentioned in the introduction, a common feature of contemporary GCMs and RCMs)."

Was replaced with "SWT time series obtained with both scenarios, W2 hydrology and W2, were compared using statistic error measures (see Sect. 3.3 for more details), assessing the relationship between the reservoir WRT and the error resulting from the neglect of advection due to inflows and outflows (as mentioned in the introduction, a common feature of contemporary GCMs and RCMs)."

COMMENT: Equation 4: What is the definition of Φ ?

RESPONSE: Thank you for pointing this out. This is the self-similarity function. The self-similarity of the temperature profile implies universality of the function for all lakes.

Line 261: the following sentence has been included in the manuscript:

"...and is the self-similarity function (dimensionless temperature)."

COMMENT: Section 3.3: I suggest adding the Kling-Gupta efficiency (KGE) as a model evaluation metric.

RESPONSE: Thank you this comment. We agree and understand the reviewer's suggestion. The metric was included. Hence, all tables were modified accordingly.

Line 302: the following sentence was included in the manuscript:

"...and the KGE varied from 0.61 to 0.96 ($\bar{KGE} = 0.78$; $SD \pm 0.09$)."

Line 306: the following sentence was included in the manuscript:

"...and the KGE values varied from 0.62 to 0.76 ($\bar{KGE} = 0.71$; $SD \pm 0.04$) (Fig. 3e). The results show that a KGE value above 0.6 describes a reasonable fit between both datasets."

Line 351: the following sentence was included in the manuscript:

"Accordingly, the KGE values are above 0.96 (Table 6)."

Line 371: the following sentence was included in the manuscript:

"However, it is relevant to mention that the KGE values obtained for 1-D models indicate that, overall, they performed well (Table 6)."

COMMENT: L319-320: It is not true for Hostetler-based models. They can account for the wind sheltering effect, as documented in Guo et al. (2021). The difference is that the 2-D models can account for the direction effect of the wind sheltering but the Hostetler-based models cannot, which may be important for elongated reservoirs. "Guo, M., Zhuang, Q., Yao, H., Golub, M., Leung, L. R., Pierson, D., & Tan, Z. (2021). Validation and Sensitivity Analysis of a 1-D Lake Model across Global Lakes. *Journal of Geophysical Research: Atmospheres*, 126, e2020JD033417."

RESPONSE: Thank you this comment. We understand the reviewer's concern. However, as explained above, we intentionally avoided calibration of 1-D models. Therefore, in our simulations with the 1-D models, the wind-sheltering coefficient was kept with a value of one for all simulations and wind velocity was kept unchanged.

COMMENT: L323: delete "effect"

RESPONSE: Thank you pointing this out. We agree with the reviewer and the word was deleted.

COMMENT: L325: This sentence is unclear to me. Do you mean the difference of RMSE between W2-reservoir and W2-lake?

RESPONSE: Thank you pointing this out, the reviewer is right. The sentence was modified as follows.

Line 325: "RMSE values reached..." was replaced with following sentence:

"The difference of RMSE values between W2 hydrology and W2 scenarios reached 2.7 °C, 1.2 °C and 0.9 °C, respectively (Fig. 4)."

COMMENT: L355: please cite Guo et al. (2021). "Guo, M., Zhuang, Q., Yao, H., Golub, M., Leung, L. R., Pierson, D., & Tan, Z. (2021). Validation and Sensitivity Analysis of a 1-D Lake Model across Global Lakes. *Journal of Geophysical Research: Atmospheres*, 126, e2020JD033417."

RESPONSE: Thank you pointing this out and for sharing this manuscript. The revised

version of the manuscript includes this citation.

COMMENT: L369-370: I do not think it is true. As shown in Guo et al. (2021), the thermal regimes of deep lakes usually can be better simulated by Hostetler-based models than shallower lakes because deeper lakes usually have larger Wedderburn numbers. Here, the larger errors in these deeper reservoirs may be caused by other factors. For example, the default parameters, such as light attenuation coefficient, may be not suitable for these deeper reservoirs. Also, lateral flow may destabilize the thermal structure of these reservoirs, making them difficult to simulate by 1-D models.

RESPONSE: Thank you for this comment. It is important to mention that in the manuscript we say that " HLM had a worse performance for reservoirs R3, R11, R14, R1 and for the six deepest reservoirs, R19, R20, R21, R23, R22 and R24, which indicates that the vertical heat diffusion was not optimally computed (Fig. 5b). Specifically, the explicit approximation of convective mixing in the HLM model by convective adjustment of unstable temperature profiles is apparently too rough, to simulate convective mixing in deep lakes (Bennington et al., 2014)." We are not saying that the model results are better for shallow lakes when compared with deep lakes. Reservoirs R1 and R3 are very shallow reservoirs, and reservoirs R11 and R14 are shallow when considering the depths of the other mentioned reservoirs.

The Hostetler model tended to overestimate the water-surface temperature at the same wind conditions during the entire year. This behavior is determined by the underestimation of heat diffusion to deeper layers. Perroud et al. (2009), while modeling the water temperature profiles of Lake Geneva (Maximum depth = 309 m) concluded that the Hostetler model performs well on the surface layers (0-5 m) but, due to the overestimation of the maxima squared buoyancy frequency (N^2), diffusion of heat below a depth of 5m is underestimated. A similar result was described by Martinov et al., (2010). The model performed well in shallow lakes, but differences between modeled and observed water temperatures were significant in lakes with depths > 60m, due to underestimation of horizontal and vertical heat diffusion. Several authors (Subin et al., 2012; Bennington et al., 2014; Xiao et al., 2016) suggested artificially increasing heat diffusion to compensate the lack of 3-D mixing processes, when modeling the Laurentian Great Lakes.

It is also important to mention that we have replaced the eddy diffusion parameterization of the Hostetler-based model with the parameterization proposed by Sundaram and Rehm, 1973, while preserving all other default parameters (e.g., light-attenuation coefficient). This solution improved the results considerably.

Nevertheless, we agree with the reviewer: lateral flow can also contribute to the differences observed. However, to validate and quantify this fact, we would have to include this parameterization in the 1-D models and test the partial contribution of the eddy diffusion parameterization versus the lateral flow effect. A modification that will be addressed in a future study.

COMMENT: Section 4.2.2: please also add the computational time of 2-D models for reference.

RESPONSE: Thank you this comment. We agree with the reviewer and the computational time of 2-D models was added to the revised version of the manuscript.

Line 381 - Table 7 was modified in order to include the computational time of 2-D models

Line 412 - the following sentence was included:

"Table 7 also shows the significant difference in computational time between the 2-D model and all the other models."

COMMENT: Figure 8: The caption is confusing. I think all models use the same atmospheric forcing. For 2-D models, what this figure presents is the wind stress after accounting for the sheltering effect. Please make it clear.

RESPONSE: Thank you this comment. We agree with the reviewer and the caption has been modified.

Line 454 - the following caption:

"Figure 8. Mean annual wind velocity values obtained with W2 Reservoir scenarios (W2R), W2 Lake scenarios (W2L), HLM, FLake and ANN considering hourly meteorology (2005-2008). Bias between W2 Reservoir (W2R) and the other models SWT results"

Was replaced with:

"Figure 8. Mean annual wind-velocity values obtained with W2 hydrology-H (W2 hydro.-H), W2-H, (accounting for the wind-sheltering effect), HLM-H and FLake-H scenarios taking into consideration hourly meteorology (2005-2008). Bias between W2 hydrology-H and the other scenarios' mean wind-velocity values"

COMMENT: L483: All reservoirs tested in this study are under the Mediterranean climate. So the conclusion here is too broad. It is better to say "for the same morphometry and under the Mediterranean climate".

RESPONSE: Thank you for this comment. We agree with the reviewer. This change was included in the revised version of the manuscript.

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