

Geosci. Model Dev. Discuss., referee comment RC2
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Reply on AC1

Anonymous Referee #2

Referee comment on "SIMO v1.0: simplified model of the vertical temperature profile in a small, warm, monomictic lake" by Kristina Šarović et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-118-RC2>, 2022

The authors presented a detailed response to the comments suggesting the study would have been deeply revised. In particular, a comparison with other lake models is to be added, which would certainly add a value to the manuscript and would provide the reader with necessary information on the model performance and usability. The numerous changes described in the response imply the results differ significantly from what was presented in the original version, and the discussion assumed to be focused on the model performance compared to other lake models. After reworked in such a form, the study might provide a significant contribution to GMD and would find an appropriate readership among modelers. One remaining general question on my side is whether the proposed model has sufficient novelty compared to that of Sun et al. (2007). Below are also remarks on the Authors' responses to the first round of comments:

[11] The shortwave radiation model of Henderson-Sellers appears to be too complex for the case when no data on the light extinction properties of the lakes are available. A one-band exponential Beer Law or the two-band Jerlov's model would provide more robust alternatives, where the value(s) of the extinction coefficient(s) might be carefully adjusted based on the comparison of the model results against observations. It is an important issue, since shortwave radiation absorption will strongly affect the final modeling results in terms of the vertical stratification as well as surface temperatures.

[12] Longwave radiation balance on the lake surface: note that $r = \epsilon$ only in thermodynamic equilibrium, which is generally not the case for the lake surface. Better use more careful formulations here.