Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
https://doi.org/10.5194/gmd-2021-118-RC1, 2021

Comment on gmd-2021-118

Anonymous Referee #1

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-RC1, 2021

General comments

This manuscript describes a new, comparably simple model for the 1D-simulation of the vertical temperature profiles in a lake. The paper is clearly structured and easy to read, and I appreciate the detailed and self-critical analysis of the model results. However, I have some general concerns about the scientific novelty and approach as well as the scientific rigour of the work.

First, the scientific focus and novelty of the paper remains unclear. It includes two different topics that are, from the scientific point of view, largely independent. And for both of these topics, a different approach would have been more appropriate for a thorough scientific investigation.

The first topic deals with estimating the heat fluxes at the lake surface in the absence of direct measurements of shortwave and longwave radiation. Such parameterizations of the surface heat fluxes have been previously described in numerous publications. If there is any novelty in the approach that the authors use here for this purpose, it is not made clear to the reader. The only thing that I haven't seen in the context of lake modelling is the suggestion to derive the daily dynamics of solar radiation from UV-B measurements. However, this seems to be mainly a workaround for this specific case than a generally applicable approach, as in general, observations of global radiation are much more frequently available than observations of UV-B radiation. Furthermore, the approach used in the study does not allow to check whether the applied heat flux parameterization works well. In fact, the results seem to indicate that it doesn't, given that simulated lake surface temperatures significantly and consistently overestimate observations even in very short model runs of a few days.

The second topic is the temperature model for the given lakes. Again, it is not clearly pointed out what is new about the modelling approach. The model seems to be mostly taken from the paper of Sun et al. (2007) with the addition of a turbulent term. And also here, the approach of the study doesn't really allow to assess how well the model works. This would probably be done best by comparing the simulations with those of other models forced with the same surface heat fluxes, which might then allow to assess to what extent the relatively large discrepancies between simulations and observations are caused by the surface heat flux parameterization and by the actual lake model.

Second, the model description and the equations contain several errors that are described in the following detailed comments. I did not check all equations in detail, but some things are clearly wrong. Some of the errors are probably only typos in the text or errors when creating the figures, but others might also be wrong in the model formulation.

For these reasons, I cannot recommend to accept publishing this paper in Geoscientific Model Development in its present form.

Specific comments

Line 15: I don't think that it is clear for the reader here what is meant with "a sensitivity analysis of the simulation length"

Study area: it would be easier for the reader to have the lake properties in a table rather than in the text.

Figure 3: is there any specific reason for using J/m2/h rather than the standard W/m2 for UV radiation?

Line 149: check the usage of phi, there is capital phi in the text and small phi in the equation. Small phi is also used for latitude and capital phi later for the surface heat flux. Please use consistent and unique symbols.

Equation (2): I don't know the source of that equation, as Sun et al (2007) don't give a reference for it, but for high temperatures, the density calculated with this equation seems to be quite far from other standard equations that are usually applied in lake and ocean models (e.g., Chen and Millero or IES-80).

Equation (3): I think there is a factor z missing in the equation.

Line 169: I don’t think it makes sense to neglect turbulent transport even in lakes shallower than 10 m. This is usually one of the main drivers determining the surface mixed layer depth (e.g. Monismith and MacIntyre, 2009, https://doi.org/10.1016/B978-012370626-3.00078-8)

Chapter 3.1.1: It is not clear from the text how exactly the chosen approach accounts for the effect of cloudiness on surface downward solar radiation.

Equation (12): I think this should be 6.11 not 0.611 if the unit of the vapor pressure is hPa (=mbar). It is correctly implemented in the code, although the wrong unit is given there (Pa instead of hPa).

Line 227: difference in day length between what and what?

Equation (22): the function of light transmission as a function of depth was somehow derived by Wu et al. based on a relationship between Secchi depth and lake depth of a range of Swedish lakes by Hakanson (1995). That means, the information of surface clarity (Secchi depth) as a function of total lake depth for a range of lakes is transferred to a function of lake clarity within a specific lake as a function of depth. In my opinion, this does not make sense. If no Secchi depth measurements or other clarity information is available for the studied lakes, I think it is preferable to use a constant default value for clarity.

Equations (23) and (26): I think the first epsilon is redundant in both these equations. Furthermore, reflection of the longwave radiation at the lake surface of about 3% of longwave radiation is neglected (e.g. Henderson-Sellers, 1986, https://doi.org/10.1029/RG024i003p00625). Randomly, these two things (neglecting 3% removal and adding an epsilon factor of 0.96) more or less cancel each other.

Lines 260 ff: In Crawford and Duchon (1999), f was defined as 1 minus the ratio of observed radiation to clear-sky radiation. This never reaches zero because even at 100% cloudiness, significant radiation remains. Does this have any implications for how the model is applied here?

Chapter 3.1.4: I don’t think this approach is correct. Assume Tprec is equal to the lake surface temperature. Then the precipitation does not change the lake surface temperature. But in the model it does increase the temperature. Tprec should probably be replaced by (Tprec-Ts) in the equation?

Line 320: The implicit Euler method is unconditionally stable, but it can still lead to significant errors if the time step is too large. A time step of one hour seems comparably long for this model, where the forcing data can change quite strongly from hour to hour. Did you check whether the solution would be significantly different with a shorter time step?

Figure 5: There is something wrong here. The theoretical upper limit of the shortwave heat flux is the solar constant of 1368 W/m2, the typical upper limit of observed surface solar radiation is about 1000 W/m2. The July peak in the figure is 20’000 W/m2.

Line 396: Add some quantitative information about the error in the onset of stratification. That is difficult to read from the figures.

Figures 9 and 10: for which period are these measures averaged? This should be mentioned in the caption of the figures. Also, the fact that the temperature bias at the lake surface is consistently positive even in simulations of very short duration (1 day), seems to clearly indicate that there is something wrong with the surface heat flux parameterization (see main comment above).

Line 474: Again, some quantitative information on the error of the simulated onset and termination of stratification as well as the thermocline depth would be useful.

Table 2: There are numerous lake modelling studies reporting quantitative errors compared to observed data. Below, some other studies that could be considered here, but there are many more:

- LakeMIP publications: Goyette et al. (2013), https://doi.org/10.5194/gmd-6-1337-2013 and Perroud et al. (2009), https://doi.org/10.4319/lo.2009.54.5.1574
- Read et al. (2017), https://doi.org/10.1016/j.ecolmodel.2014.07.029
- Gaudard et al. (2019), https://doi.org/10.5194/gmd-12-3955-2019
- Moore et al. (2021), https://www.sciencedirect.com/science/article/pii/S1364815221001444

I understand it would exceed the scope of this manuscript to completely review this literature, but at least the formulation that there are only few studies reporting such information should be reconsidered.

Line 521: I find it surprising that the turbulent term has no effect. This would imply that for the present lakes, vertical mixing is practically exclusively driven by convection, which seems unlikely. Maybe the turbulent term is underestimated and this is the reason why the simulated thermocline position is too shallow as suggested on line 430? What are the vertical turbulent diffusivities resulting from the model?

Line 542: I disagree that the position of the thermocline and its deepening were well captured. The position of the thermocline seems to be 5 to 10 m off for most of the year in Figure 13 (but see request above to provide some quantitative measures for this).