Review of "A General Lake Model (GLM 2.4) for linking with high-frequency sensor data from the Global Lake Ecological Observatory Network (GLEON)" by Hipsey et al.

This article describes the scientific basis of a 1-dimensional hydrodynamic lake model that can be coupled to ecosystem models. The model has already been applied to many systems in the scientific community, and I think it is useful publish the model description in a scientific paper that can be referred to for future applications of the model. That said, I stopped reviewing after equation 16, because there were simply too many errors in the equations. I therefore propose to reject the current version of the manuscript and that the authors carefully check all equations before resubmitting the manuscript to this or another journal, depending on the decision of the editors.

Errors in equations up to eq. 16:

eq. 2 and 3: I think something is wrong with the indices. h_z is located between h_{b-1} and h_b. α_b and β_b describe the interpolation between h_b and h_{b+1}. Thus, the indices in eq. 2 should be α_{b-1} and β_{b-1}.

eq 6: I think this equation is wrong. The right hand side is the total heat flux to the surface layer in W m^{-2}. This should be divided by z_{msl} to get W m^{-3}. Then, it should be divided by the water density ρ in kg m^{-3} to get W/kg, and finally by c_p to get °C/s for dT_s/dt. Therefore, the multiplication term on the left hand side should be $z_{msl}\ c_p\ \rho$, rather than $c_p/(A_S\ z_{msl})$.

eq 9b / Fig. 3: I could not reproduce the maximum of the Briegleb function at 80 degrees zenith angle. Using equation 9c yielded a monotonically increasing function between 20 and 90 degrees (with a minimum at about 20 degrees). Also the equation in the legend is wrong, it should be SZA = Θ_{Zen}*180/2π.

eq. 12: I think ϕ_{SWS} (i.e. the shortwave radiation absorbed in the surface layer) should be replaced by ϕ_{SW} (z=0) in the nominator. Otherwise, the euphotic depth increases with increasing radiation absorbed in the surface layer, which does not make sense. Same in caption to Fig 4.

eq. 16: I think in equations 16c and 16d T_a should be replaced by absolute temperature (i.e., 273.15 °C should be added to T_a).

Also units should always be provided, especially for empirical equations (e.g. e_a in eq. 16, and U_x, RH, and diffusive radiation in eq. 9c).

Besides that, a few other points I noticed up to page 11 (Page xx, Line yy is abbreviated as xx/yy)

In general, the paper is well written and easy to read, but there are quite a few long and complicated sentences which I think should be simplified to facilitate reading (first two examples: 2/24-29 , 3/12-17).

3/10: This list of references seems to be somewhat inconsistent. Some of the references refer to model development, some to model applications. It would be more logical to cite only model development references.

4/30: The text seems to imply that the requirement for site-specific calibration in other models is due to numerical diffusion caused by the fixed grids. If that is the intention, this should be explained. If not, the sentence should be modified.

5/7: Incomplete sentence

Figure 1: Shouldn't the local runoff, and the submerged inflows and groundwater seepage be written in blue?

eq. 1: From the text (layer volumes are determined ...), I would have expected an equation for the individual layer volumes here, but this is the integrated volume from the bottom of the lake to the top of each layer. This should be clarified in the text.

6/3: technically, it is the same, but I think it would be clearer to write $2 \leq b \leq N_{BSN}$.

6/4: how are these finer depth increments determined?

6/9: Since the Unesco (1981) equation has been replaced by TEOS-10, I think it would make sense to use the latter rather than the former in a new model. I also think it should be mentioned that the density effect of salinity in these seawater equations is quite different from that in most lakes where carbonates are usually the dominant species rather than NaCl.

6/24: heat balance **of** the surface layer

7/2-3: why is only rain but not snow multiplied with f_R? Also, even though this should be clear to the reader, it should probably be mentioned that S is in water equivalents.

eq 5: to be precise, this equation should be limited to a minimum of zero, as otherwise it will become negative if the rainfall is too weak.

Fig 2: Add some space between the 10 and the exponent in the y-axes labels of panels c and d. Do all these time series start on 1 January of a year?

eq. 9a: instead of subtracting $\pi/2$ within the sine functions, it would be easier to use -cos.

eq. 9b: where does the factor 1.1 in the nominator of the first term come from? Maybe I overlooked something, but I could not find it in Briegleb et al. (1989).

eq. 9c: I was not able to check this equation, as the source is in Japanese, but I did not get anything similar to what is shown in Fig. 3 trying different values for RH, U and the diffusive radiation. Please check whether the equation is correct, and specify the values used to produce Fig. 3. Furthermore, Yajima and Yamamoto is dated 2014 here but 2015 in the reference list.

Fig 7: y-axis of panel b is not depth, but elevation, y-axis of panel c is not labeled. Also it seems that A_{BEN} is calculated on a different time scale than the radiation in (c). Many low radiation events are clearly visible in (b) but do not show up in (c). This probably makes sense, but the time scale should be mentioned somewhere.

11/11: It does not look like the equations were copied from Henderson-Sellers (1986), but rather from either the original sources or from Flerchinger (2009)?