Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-257-AC2, 2018

Geoscientific Model Development Discussions

Interactive comment on "A General Lake Model (GLM 2.4) for linking with high-frequency sensor data from the Global Lake Ecological Observatory Network (GLEON)" *by* Matthew R. Hipsey et al.

Matthew R. Hipsey et al.

matt.hipsey@uwa.edu.au

Received and published: 8 January 2018

Dear Reviewer,

Many thanks for your initial thorough comments and suggestions - below are your comments in *italic* and our responses follow with the REPLY tag.

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.

REPLY: Thank you for taking the time to review the discussion paper and identify the errors - we sincerely apologise that simple issues related to units and notation were not more thoroughly checked prior to upload. In a separate discussion comment, a revised manuscript ("Preliminary Revision 5 Jan 2008") is ready for your continued review. Please note this revision does have some substantial changes to the paper including the notation, text and (some) figures, and so would request you start reading from the beginning again (or at least from the end of the introduction). We very much look forward to your comments and suggestions on this version.

Errors in equations up to eq. 16: eq. 2 and 3: I think something is wrong with the indices. hz is located between hb-1 and hb. αb and βb describe the interpolation between hb and hb+1. Thus, the indices in eq. 2 should be αb-1 and βb-1.

REPLY: You are correct; the code was looping from one step behind so we had incorrectly omitted the -1 in the equation. It is now updated

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 zmsl to get W m-3. Then, it should be divided by the water density in kg m-3 to get W/kg, and finally by cp to get °C/s for dTs/dt. Therefore, the multiplication term on the left hand side should be zmsl cp , rather than cp/(AS zmsl).

REPLY: You are correct; it is now updated and in fact this section is now significantly revised. Table 1 now also includes all the notation and relevant units.

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 = ΘZen*180/2π.*

REPLY: You are correct; This was an error in creating the plot rather than the model itself, and Figure 3 has been re-created and caption updated.

eq. 12: I think φSWS (i.e. the shortwave radiation absorbed in the surface layer) should be replaced by φSW (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.

REPLY: The notation has been improved to prevent confusion; φSWS is what enters the top of the surface layer, and so the euphotic depth computation is based on the fraction of incoming light.

eq. 16: I think in equations 16c and 16d Ta should be replaced by absolute temperature (i.e., 273.15 $^\circ$C should be added to Ta).

REPLY: You are correct; notation updated to have K and C unit options for temperature

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

REPLY: Units have been thoroughly checked and updated in the revised upload, plus the updated Table 1 summarises units for all variables/symbols.

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).

REPLY: These examples have been re-written and the revised upload has also been check for these issues.

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.

REPLY: Point noted, these papers were reflecting the diversity of 1D models we are aware of, but agree these should more specifically refer to development refs.

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.

REPLY: Sentence modified...

5/7: Incomplete sentence

REPLY: Section has been revised and reworded.

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

REPLY: Local runoff is computed by GLM (Eq 7 in updates manuscript). Submerged inflows and seepage however has been updated to be blue in Figure 1 as they are specified.

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.

REPLY: Section has been revised and reworded to hopefully introduce the layer structure and notation more clearly.

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

REPLY: Updated.

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

REPLY: Now summarised in symbol table (Table 1).

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.

REPLY: Thank-you for this suggestion. The preliminary revision upload still refers to UNESCO 1981, however, we will endeavour to implement TEOS into the code and the refer to this in the full revision, (of course, depending on the outcome of the review process)

6/24: heat balance of the surface layerâĂÍ7/2-3: why is only rain but not snow multiplied with fR? Also, even though this should be clear to the reader, it should probably be mentioned that S is in water equivalents.

REPLY: Updated.

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.

REPLY: Updated - this was the case in the code, but not properly summarised in the Eq.

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?

REPLY: This panel has not yet been updated in the preliminary revision, but we will do so in the full revision. Lakes do have variable start times (for various reasons) which is why we didn't use exact date in the x-axis. The individual lake simulations are documented on our GitHub site with explanations.

eq. 9a: instead of subtracting π/2 within the sine functions, it would be easier to use

-cos.

REPLY: Apologies, there was a "-" sign wrong in this Eq so both should not have been -π/2. In the new manuscript (Eq 12a) we have the addition or subtraction of π/2 is listed properly in order to allow us to differentiate Sth vs Nth hemisphere sites.

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).

REPLY: Thankyou for noticing this detail - this inclusion of 1.1 has been picked up from the implementation by Li et al (2006), who compared models and have this coefficient included.

However, for consistency, we have removed this from the Eq in the paper, and the code-base going forward. We note however, that the change made only a modest difference to the function.

Li, J., Scinocca, J., Lazare, M., McFarlane, N., Von Salzen, K. and Solheim, L., 2006. Ocean surface albedo and its impact on radiation balance in climate models.ÂăJournal of climate,Âă19(24), pp.6314-6333.

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.

REPLY: Thankyou again for checking this - the equation implemented is based on the Equation 1c of this publication, whereby y is cos(solar angle), the a and b coefficients were set by multi-variate regression, and x1 is RH(percent), x2 is wind speed (m/s) and x3 (-) is a parameter referring to the amount of atmospheric diffuse radiation.

We initially received the coefficients from the author (Yajima pers comm.) for our implementation, and following your comment, we have now updated the Figure 3, the citation date, and the equation. The caption will also be updated to state the values of x, x2, x3 used in the graph. For your reference, R code for the algorithm is below:

$angled = 1 : 89$

$angle = angled * pi/180$

$dr = 6$

$ux = 4$

$rh = 80$

$yalbedo = max(0.02, 0.001 * rh * (1 - cos(angle))^0.33 - (0.001 * ux * (1 - cos(angle))^(- 0.57)) - (0.001 * dr * (1 - cos(angle))^(0.829)))$

Fig 4: y-axis of panel b is not depth, but elevation, y-axis of panel c is not labeled.

REPLY: You are correct; the y-axis should be height not depth. As part fo the revised manuscript we have uploaded we have undertaken a major review of notation used throughout so that depth (z), height (h) and elevation (H) are used consistently.

This figure is not yet updated in the preliminary revision but all figures will be re-created and revised in the full revision, to reflect these issues.

Also it seems that ABEN 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.

REPLY: Please note that in fact the ABEN is now being computed based on a percentage of light reduction, rather than a specific value.

The step changes in the panel (c) time-series are not due to time-scale of calculation, but rather to do with changes in the layer thickness and structure. We will review this example simulation carefully to improve this in the full revision, and anticipate it will have the same trend but smoother changes.

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)?

REPLY: The placing of the citation to Henderson-Sellers incorrectly gave the impression this is where the full expression was from. You are correct that that we have just chosen 4 based on original sources to implment within the model. We have now cited Henderson-Sellers and Flerchinger as sources for further description and information, rather than as a source for the algorithm set.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-257, 2017.