

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.

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Dear Reviewer,

Many thanks for your comments and ideas which we have found very helpful. Below are your comments in *italic* and our responses follow with the **REPLY** tag.

General Comments

1. *This paper describes the detailed functioning of the 1D physical lake model GLM 2.4 and its application potential. The model incorporates a broad range of*

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physical processes as surface heat exchange, snow and ice dynamics, in- and outflow, submerged inflow and groundwater seepage and can be coupled with or embedded into other models. The authors explain how GLM 2.4 has emerged as a response to the need of standardized, yet flexible and computationally effective community lake model to interpret environmental data from a broad range of lakes collected within the Global Lake Ecological Observatory Network (GLEON). The model has been formulated as a new code in 2012, whereas layer structure, mixing algorithms and physical formulae are based on earlier peer reviewed work. The authors state that the code is computationally efficient and well suited for embedding in larger scale modelling frameworks. The authors present also an overview of pre- and post-processing utilities as well as an innovative cloud computing environment. Lastly, they elaborate on the educational use and gained experience in the classroom.

REPLY: Thankyou for this very accurate summary

2. I realized that this manuscript is for a major part equivalent to an earlier manual of GLM (V2 Manual, October 2014, accessed on the 08.01.2017 from <http://aed.see.uwa.edu.au/research/models/GLM/Pages/documentation.html>). I think the authors should mention this.

REPLY: It is our intent that this paper replaces the online manual and we have removed the available PDF cited above. The online manual was used as an interim resource to inform users until the model development efforts had stabilised. Now this is the case, the revised version of the present paper includes more detail and numerous improvements, extensions and fixes to errors and should become the key reference. Aspects of that manual associated with model-use setup that are not covered in this (science-oriented) paper have been migrated to the website pages.

3a. The model in this paper represents with no doubt a tremendous effort in lake modelling and is of interest for modelers in various fields of environmental research. The publication of this model is a step towards better model documentation and contributes to the general scientific discussion and better lake model development. As such it falls

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within the scope of this journal. The paper is well written and the language is easily com- prehensible. Unfortunately, this manuscript has some structural problems and there are quite a few mistakes in equations and figures. After dealing with these is- sues, the manuscript should be good for publishing.

REPLY: Thank you for this recommendation. A "preliminary revision" was uploaded on the 8th Jan following comments by R1 and R2, and this does already address many of the points outlined below – apologies this was not made available before your review. We will endeavour to resolve the other specific issues that you and the other reviewers have identified in the full revision to be uploaded following closure of the discussion phase.

3b. The main problem of this very long manuscript is that it is missing an instant overview of what is in the paper and what not. Scanning through, the reader gets lost easily in the large chapter 2 ‘model overview’ and might miss the subsequent chapters that elaborate more on the possibilities and significance of this model for the scientific community.

REPLY: We acknowledge this view, which is similar to a comment 2 by R2, and we will aim to provide a better “road-map” to add clarity in the introduction.

4. I think that this problem can be fixed with some changes in the introduction: I suggest using subtitles in the introduction. (In the introduction, the authors describe the importance of the study of lakes, the importance of GLEON, the importance of lake models, the advantages of simple models, applications and features of 1D models, the need for a flexible open-source model, how GLM 2.4 answers this need and finally an overview of the paper) I suggest creating a new paragraph starting at p. 3 Line 19 ... “Nonetheless, there . . .”. The need of an open source and flexible community model that can be applied to various lakes should be highlighted better. Another additional paragraph could explain how GLM 2.4 responds to this need. As I understand, GLM 2.4 is filling the gap because it provides a standard middle complexity physics ‘shell’ (simple yet enough complex to be applied for various lakes) that can

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be connected individually to or implemented into various other models (e.g. water quality or land- climate models). I think this point could be emphasized. A figure could be helpful to draw attention to the significance of this model in the scientific community. This could also be combined with schematic overview of the model functioning (I agree with R2 that anything that gives an overview helps). The specific limitations of GLM 2.4 (not of 1D models in general) should be mentioned in the introduction. Like this, the reader may have a quick idea whether GLM is suitable for him/her. What are the key features of this model that set it apart from other models? On p. 4 lines 5-9, the authors explain the aims of the paper and in which of chapter 2-6 these aims are met. I think these lines are important and should be extended to a paragraph by itself to make sure the reader is fully aware what to expect from the paper. In the same paragraph, I would also expect some more information regarding what this paper is not about and mention that a companion paper by Bruce et al. (2017) is assessing the model's error structure against 31 GLEON lakes.

REPLY: Thank you for this very useful suggestion and we will take up this idea for improving the introduction in the full revision.

5. I think the authors did not carefully go through the complete manuscript. Many of the empirical equations are missing the definition of units for used variables. On other occasions variables were poorly described (see the examples listed below, as well as listed by R1 and R2). On several figures elevation and not the labeled depth is shown on y axis. The references are not formatted coherently. Like R2, I am of the opinion that many variable symbols are confusingly similar and that they should all be listed in a table. I also agree with R2 that all the subchapters of chapter 2 should have a small introduction paragraph. Further, I agree with the comments of R1 on the equations 1, 2, 3, 5, 9c, 12, 16 and with the comments of R2 on the equations 4, 7, 14, 26, 31, 52.

REPLY: Our sincere apologies that we didn't identify these flaws in the original upload. In our responses to R1 and R2 we have detailed many fixes to these issues, including a significantly revised nomenclature and summary table with all variables and units.

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The "preliminary revision" that is uploaded to the discussion area (Click here) now addresses most of the issues, and further updates are planned for the full revision, including updates to the figure scales, and improved contextual information in the Section 2 sub-sections.

Specific comments

6. *I think it is not very clear how the amalgamating, expanding, contracting or splitting and adding of layers works. For example, in p. 23 L 21 it is not obvious what the mentioned 'numerical criteria within the model' are. I would explain these in detail somewhere in the beginning of chapter 2.*

REPLY: We have revised Section 2.1 already in the preliminary revision that adds clarity to the layer scheme, and as also noted to R2 (specific comment 40), we will add some further detail in the full revision to better describe this aspect of the model.

7. *p. 6 eq 2 and eq 3: It seems odd that the interpolation of values between levels $b-1$ and b are depending on $b-1$, b and $b+1$ and not only on $b-1$ and b .*

REPLY: This error is fixed in the preliminary revision

8. *p. 10 Line 5-8: φ SWS is defined only in text form and not as an equation, yet it is used in equation 6. There is the danger that φ SW defined in eq 10 will be confused with φ SWS. I suggest mentioning early on in this subchapter how you approach calculating φ SWS.*

REPLY: This notation for energy fluxes has been significantly updated in the preliminary revision; the extended Table 1 now makes the symbol definitions and units clear, and the text is updated accordingly.

9. *p. 12 eq 17: formula only for forced convection? Wind speed at what height? What are the units? I would introduce first the concepts of sensible heat (free and forced convection) and latent heat (evaporation and condensation) before showing the equations.*

REPLY: As per point 8 above, the notation for heat transfers has been significantly

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updated in the preliminary revision. This makes explicit the reference height. As per the above suggestions for an opening paragraph in the sub-sections, we will introduce free and forced convection here in the final revision.

10. p. 18 L10: An intro with possible conceptual options to reproduce a surface mixed layer would be good. I would like to know how the chosen approach of a bulk mixed layer depth compares to other approaches in other models (e.g. k -epsilon turbulence closure with Fickian diffusion) and what the consequences of this approach are.

REPLY: A related comment by R2 (specific comment 27) was also made; as above we will improve the opening context in this sub-section in the final revision.

11. p. 22 L 15 and eq 44 and eq 45: I think an explanation of the concept behind this numerical scheme is necessary

REPLY: We will improve the description/justification for this diffusion algorithm in the final revision.

12. p. 24. Figure 10: This figure is not enough self-explanatory to me.

REPLY: A revised version of this figure has now been included in the preliminary revision, better depicting the interaction of the inflow parcels with the lake layer structure, and using the updated notation. We also propose to provide a further refinement to this figure in the final revision in response to R2 specific comment 34.

13. p. 28 eq 60: Shouldn't G not just be another term in eq 4 for all cases?

REPLY: The term G is a vertical flux into the soil below the lake and is only applied to the bottom-most layer of lake ($i = 1$). This changes the thickness of the bottom layer ($\Delta z_{i=1}$), but not the thickness of the uppermost layer ($\Delta z_{i=N_{LEV}}$), or any other layer. You are correct that the change in the thickness of the bottom layer then also leads to a downward vertical shift in the elevation of all the layers above (equivalent to advection, as also discussed with R2 specific comment 8). As this step occurs separately to the surface dynamics routine we had not included a term for this in Eq 4, and instead included a sentence describing this effect (page 7 line 11 in the original upload, and

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page line 7 in the preliminary revision). Given this is a potentially confusing point, we will therefore update Eq 4 in the full revision to have an additional term for height change due to inflow/outflow dynamics, and update the text to explain this.

14a. p. 40 lines 21-24: move this sentence to the intro

REPLY: In light of the proposed changes to the introduction discussed in comment 4, we will update this accordingly.

14b. p. 40 L 24 – 26: This needs to be better explained.

REPLY: The concept being referred to here is the idea that a long-term simulation from GLM can be developed for a lake, however, since the model is not as detailed as a 3D hydrodynamic model it may not fully resolve event dynamics at a sufficient level of detail (eg a flood inflow event in a reservoir or a localised algal bloom event). In this case, the GLM model can be used to provide the necessary initial conditions and boundary conditions for an event-scale simulation of a higher-resolution model. We propose to mention this in the full revision in Section 5 as part of the integration text.

List of Corrections

15. p. 1 lines 31-32. Consider splitting sentence as it contains different ideas.

REPLY: Thank you for the suggestion, we will revise the abstract in the full revision.

16. p. 2. Line 1: write only ‘standing’ as this word is comprehensible and you don’t use lentic in the rest of the text.

REPLY: Thank you for the suggestion, we will adopt this change.

17. p. 5 Line 17. Write the definite instead of the indefinite integral or otherwise phrase it in a sentence.

REPLY: We will update the integral to be between H_0 and H_{max} .

18. p.6 eq 1: could be simplified

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REPLY: Thank you for the suggestion, we will adopt this change.

19. p.6. Line 11-18: *Should this go in the introduction?*

REPLY: This section has been improved in the preliminary revision, and we will reconsider the placement of this statement when revising the introduction in the full revision.

20. p. 9 eq 9b: *Contrary to R1, I managed to get the peak at 80° SZA. The equation seems to be the same as used in fig 3.*

REPLY:

21. p. 9 eq 9c: *Specify units, also see comments of R1.*

REPLY: This is updated in the preliminary revision and also see the reply to R1.

22. p. 9. L 6: *U_x is wind speed at which height?*

REPLY: Within the preliminary revision, all meteorological variables are now referenced to 10m, U_{10} . This is computed from the user input data (U_x) as $U_{10} = f_U U_x$. The updated equation in the preliminary revision (now Eq 12c) however still needs correcting from U_x to U_{10} .

23. p.9 figure 3: *Specify the values of relative humidity, wind speed and atmospheric diffusive radiation used for eq 9c. I agree with R1 that the label is wrong, but I think it should be $SZA = 360^\circ \Phi_{zen}/(2\pi) = 180^\circ \Phi_{zen}/\pi$*

REPLY: This figure is updated in the preliminary revision, and, we will extend the caption to specify the values.

24. p. 11 eq 16 a-d: *Use either only \hat{U}_C or only K in equations, now they are mixed. I found eq 16 c in Henderson-Sellers (1986) but strangely I couldn't find this equation in Brutseart (1975).*

REPLY: The notation is now updated so as to distinguish between them (T and θ , respectively).

25. p. 12 L 12: no units specified for latent heat of vaporization

REPLY: Table 1 in the preliminary revision has been updated to have unit descriptions of all variables.

26. p. 13 L 9 and L 13: I guess the authors meant eq 17 -18 and not eq 16-17?

REPLY: Yes, corrected in the preliminary revision.

27. p. 16 L 6: 'penetrating the surface', which surface?

REPLY: This is referring to the ice/snow surface, now corrected in the preliminary revision.

28. p. 20 eq 35: What is u_0 ?

REPLY: Following our updates to the notation in the preliminary revision it is now $u_{b,old}$; it is the layer velocity of the previous time-step. We will explain this more fully in the full revision (and add to Table 1 as it has been overlooked).

29. p. 22 eq 39: Please explain the variables in this equation.

REPLY: Apologies for this oversight and confusing notation. The preliminary revision includes the description of V further down (and in Table 1), however we will move this first description up to this point. The symbol for the surface layer thickness is also now matching the earlier use. However, we noted a further error since the k in this expression should be squared, and will be updated as $k_{TKE}^2 = \frac{c_{wn}AS}{Vz_{sml}}$.

30. p. 22 eq 43: check index, should be h_l not h_i

REPLY: This is updated in the preliminary revision (now Eq 50).

31. p. 23 L 17: typo: *entrain* (not *entrains*)

REPLY: This is updated in the preliminary revision (now Page 24 Line 17).

32. p. 23 L 18: typo: *the* (not *th*)

REPLY: This is updated in the preliminary revision.

33. p. 34 figure 17: increase font size and size of arrows.

REPLY: We will update the resolution and fonts in this figure for the full revision.

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34. p. 34 L 19: *Insert the references into the place holder*

REPLY: This is updated in the preliminary revision.

35. p. 35 L 26: *Who is testing these ‘Wrappers’ and examples? What is a wrapper?*

REPLY: We will update this sentence in the full revision.

36. p. 37 L 13: *What is HTCondor?*

REPLY: We will update this sentence in the full revision.

37. p. 27 L 16: *Start a new paragraph at ‘GRAPLER’s Web service . . .’ to highlight this idea.*

REPLY: Agreed - we will update this in the full revision.

38. p. 40 L 24 – 26: *Explain better.*

REPLY: See the reply to comment 14b above

39. *References Formatting: Some parts are underlined, remove it. Change all to coherent formatting.*

REPLY: The reference formatting will be refined in the full revision.

40. P. 49 L 23, 25 *same author, write same initials.*

REPLY: Agreed - we will update this in the full revision.

41. P. 51 L 13, *is there a translation of this Japanese paper? Check the year (2014 in text, here 2015)*

REPLY: Unfortunately there is no English translation of this article, however, Yajima pers comm provided an English summary of the algorithm performance and coefficients. Year has been corrected in the preliminary revision.

42. *Table 1: If there is no default variable, can you give a range for snow density, compaction coefficient, and thermal conductivity of snow?*

REPLY: Table 1 has been updated to specify “computed” and in the comments the relevant equation is now referred to. We will aim to add the ranges to the table where possible in the full revision.

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43. *Table 1: Latent heat of fusion: remove the trailing zero 334*

REPLY: Updated in the preliminary revision.

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