

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

General comments

1. This paper presents the formulation of a one-dimensional model of thermodynamics, mixing, and evaporative and momentum fluxes from lake surfaces, which conceptually should be applicable to a wide variety of lake morphologies. However, the manuscript

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achieves the paradox of simultaneously containing too much information and not enough information. It goes into great detail with many equations. However, in order to present this level of detail without losing the reader requires more care and at least some additional details.

REPLY: We greatly appreciate the comments and insights. We acknowledge the paper is heavy with information, and chose the journal Geoscientific Model Development for this submission as it does support papers focusing on model description. Given the nature of the model it is our desire to have a comprehensive description of the numerous sub-models and options that is peer-reviewed and citable; this has led to a long paper compared to a traditional manuscript. As outlined in the below comments, our full revision version will endeavour to improve the narrative and flow of the paper.

Note : A "preliminary revision" is referred to below, which is on the discussion at this link: [Click here](#)

I have a few broad suggestions that I think might help.

2a. Before anything, you need to have a clear idea of who is in your audience. You could even have an explicit statement of this very near the beginning, and direct users with a lower level of expertise toward a simpler users' guide.

REPLY: The paper is intended as a description of the model rather than a research paper. Whilst there have been some unpublished manuals and incomplete documentation for the model during its initial development, we felt this is unsatisfactory for a model that has had increasing uptake by lake scientists. In our initial submission, we had stated the aim as:

"Given that individual applications of the model are not able to describe the full array of features and details of the model structure, the aim of this paper is to present a complete description of GLM, including the scientific background (Section 2), model code organization (Section 3), approach to coupling with biogeochemical models

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(Section 4), and to overview use of the model within the context of GLEON specific requirements for model analysis, integration and education (Section 5-6)."

Our audience is therefore advanced users looking to understand the mechanics of the model, and/or end-users publishing applications that need to cite the model methods and approaches adopted in their individual simulations. We acknowledge some users will not dig into the detail and can following the "user" material we have provided on the model website. We note that we have not provided many details about how to run the model, but rather it was our intention for readers to be able to understand the science basis and model structure.

2b. This reviewer has a back- ground explicitly in meteorology, but significant exposure to lake dynamics as well, albeit mostly regarding very large lakes. Depending on your intended audience, you might have readers who will have difficulty with terms such as aliquot and even the intended understanding of "scalar concentrations" as used on p. 27, lines 2-3.

REPLY: Thank-you for pointing these out. Already in the "preliminary revision" (upload dated 8th Jan on the discussion page) we have attempted to improve both these descriptions to improve the clarity of this terminology.

3. To reduce the length of a single paper, one option is to break it into multiple papers. Another is to move more of the detail to appendices. Since it is an online journal, there is probably not a large problem with overall page length. Things as detailed as conversion of area as a function of depth to volume in a layer (eq. 1) seem like they could be skipped in the main text and relegated to an appendix.

REPLY: We appreciate the comment and agree with the general desire for shorter papers, and feel that in a model description paper such an argument could be made for several of the sub-model descriptions. However, a key purpose of our paper was to

describe the model from start to finish (as is done through Section 2), and the layer structure and depth-area-volume relationship is key to the model approach. Aspects such as solar radiation and atmospheric stability computation are optional modules in the code-base and not directly related to what happens within the lake model, and so are included as appendices. Nonetheless, in the preliminary revision we have made some changes to Section 2.1 (referred to specifically in this comment) and feel the new wording of this section has a stronger narrative that hopefully connects it with the subsequent sections.

4. *It would be useful to have a brief introduction to each sub-section. This should start out by stating the goal of that sub-section, i.e. what will be the final equation (or set of equations) derived in the section. Then state what elements will combine to get that final equation(s). One particularly glaring issue is that sub-section 2.5.2 ends with eq. 45, defining the variable f , but nowhere does it say how f relates to any other part of the model. It seems to be something that one would multiply by the difference in temperature (or another scalar) between layers) to get the exchange of that scalar between the two layers in a single time step. Whether this is exactly correct or not, the statement is missing from the manuscript.*

REPLY: The preliminary revision uploaded has attempted to address this general comment by strengthening the narrative in the sections from 2.1-2.7 (the main sub-model description sections), and we will further work on improving this for the full revision. In reference to the comment about the “ f ” function, the use of the term scalar (and symbol C) is now better introduced in the preliminary revision, placing this Eq in context. We note that the f computed in this equation is required in the equation where concentrations are updated based on the magnitude of diffusivity between layers (Eq 51 in the preliminary revision, and Eq 44 in the original version).

5. *A simple overall schematic would be good to have early on (Fig. 15 with less detail). This would make it less abrupt when “water quality model AED2” is*

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mentioned on p. 27 (I may have missed it, but I don't think it was mentioned before).

REPLY: In light of other comments (8 of the general comments and 42 of the specific comments), we will prepare a revised Figure 1 and 16 that will address this.

6. *Even if only for your own reference as author, Table 1 needs to be expanded to include every variable used in every equation! And in this expanded table, include every variant of each variable based on the use of different subscripts, prime, and circumflex (“hat”). With this many equations, it is rather inevitable that you also end up using the same symbol for different things (I noticed N in particular). Then, for each variant of each variable, put additional columns for: description, units, spatial type (defined at surface only, spatially continuous, or at discrete layers), and which equations it is used in. The reader needs to have all of these carefully defined.*

REPLY: The preliminary revision includes a fully updated Table 1, and addresses the issue of confusion of notation by having a more consistent nomenclature scheme.

7. *My high school chemistry teacher is the one who taught me how to use units in equations, and the importance of doing so, hence the need for them in the big table above. Some examples of problems with units in the equations that may indicate that the equation is simply wrong: In eq. 52, Q seems like it should have units of m cubed per s, and h units of m, so the right-hand side does not have units of velocity.*

REPLY: We apologise for this mistake - this equation (now Eq 59) has been corrected (and the code was checked for consistency). The updated Table 1 now also has consistent units throughout, included for each variable.

8. *I approached this manuscript with the immediate question of what makes this model better and more useful than the many others that are available. The introduction does a pretty good job of answering this, but it may be good to give some examples of uses that are not satisfied by other models. This could be introduced with a schematic of its components, options, and functions, at a lower level of detail than in Figs. 15 and*

16.

REPLY: Thanks for this suggestion. We will add a statement to the conclusion section with this idea. We do not feel in this paper we have the scope to do a full meta-analysis of the features of all the other lake models and where they are inadequate. Given the number of sub-modules and options, and very wide array of application contexts, it becomes difficult to make judgments about what model options are required for specific lake types and we defer to users to use the model in an appropriate way. We note that Figure 1 does currently aim to be a schematic for the different model components, and functions, but this is currently not linked to applications or specific lake types. We will aim to address this in the full revision, along-with the specific comment 42 by making more clear an overview of the sub-models that may be engaged.

9. Anything that can be done to bring the reader's intuition into play when introducing equations will improve comprehension of the manuscript. Eq. 27 isn't the most problematic one, but I'll use it as an example. You might introduce it by saying something like: "Shortwave radiation is absorbed and attenuates with different e-folding depths for snow, white ice, and blue ice, and these also depend on the light's wavelength. The overall effect is..."

REPLY: Thanks for this suggestion. Some wording has been updated across sections 2.1-2.6 to try to improve the flow of the text and clarity of the descriptions. We will further update this in the full revision following compilation of the set of all reviewer comments.

10. Eq. 47 has me very mystified about how a standard definition of Richardson's number translates into this equation in terms of the angles of inflow geometry (part of the problem may be that I don't feel like I understand the meaning of the angle labeled alpha in Fig. 10; the illustration isn't helping me). It also has me asking "Richardson number at what location?" At the interface of the river water and ambient

lake water?

REPLY: We have cited the original source (Fischer et al 1979) of this equation in the original paper, however to improve the interpretability of this we will add some brief detail to this definition and approach to justify its basis.

11. Several of the figures have characters that are so small as to be illegible.

REPLY: We will redraft the schematic figures with larger symbol fonts.

12. What about modularity of the model? Can other schemes for pieces of the model be plugged in? The details of how may be an appendix or even another paper or guide.

REPLY: The model is not written in an object-oriented fashion, however, custom schemes can be linked to for key sub-module components (e.g do-mixing, do-deep-mixing etc) with some code-adjustments. A comment on this will be added to the code structure section (Section 3).

Some particular examples of the general problems above are among the specific comments below.

Specific comments:

1. In nearly every review that I do, I refer to the rules of hyphens found at <http://www.grammarbook.com/punctuation/hyphens.asp> particularly Rule 1 on that page. Your manuscript is actually better than most, but here are the problems that I found: P. 2, line 23 should have “system scale” (no hyphen). “Time-scale” is sort of borderline; I tend to use it without a hyphen when it doesn’t modify another noun. P. 4, line 15 and some figure captions: “time series”. P. 13, lines 22 and 24: “Wind sheltering”. P. 24, line 3: “user-specified”.

REPLY: Thankyou for this link and advice. These are updated now in the preliminary revision.

2. P. 2, line 33: Change “spatial” to “vertical”, to contrast with horizontal from earlier in the sentence.

REPLY: This is updated in the preliminary revision (now page 2 line 32)

3. P. 3, line 10: Stepanenko is misspelled.

REPLY: This is updated in the preliminary revision (now page 2 line 7)

4. P. 3, lines 17-18: Do you have any comment on use in small vs. large lakes? Both in terms of area and depth? There might also be considerations in terms of morphological complexity.

REPLY: We will aim to refine the sentence to highlight this specific detail in the full revision.

5. P. 3, lines 30-31 specifically mention temperature, salinity, and density. Later on, there is mention of the broader category of “scalars”. Are you unnecessarily limiting yourself here? I am thinking of such things as dissolved oxygen (mentioned later) and concentrations of nutrients and contaminants.

REPLY: The reference to scalars is now updated in the preliminary revision (now page 5 line 30) to be more clear, and the option of coupling to a water quality model is mentioned (page 6 line 1)

6. P. 3, line 32: I think many readers will object to the use of “hydrodynamic” here, since it does not explicitly represent advection of water. Continuity equations say that dynamics requires at least two dimensions.

REPLY: We would argue that term hydrodynamic refers to the motion of water in general, rather than specifically need to resolve advection in two dimensions. In addition to vertical mixing and transport of water and its constituents, the model does parametrise horizontal advection of inflow water (Eq 59 in the preliminary revision). Therefore, whilst the model is not resolving the Navier-Stokes equations, the model

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approach is intended to capture the movement of water within the lake domain as it is filling, drying, and subject to inflows and withdrawals.

7. *P. 4, lines 31-32: “user-defined” and “set by the user” are redundant.*

REPLY: Section 2.1 has been significantly revised in the preliminary revision upload and this problem is removed.

8. *P. 5, line 8: This model doesn’t include it, but in reality, vertical advection of heat can be important along with vertical mixing. Again this may depend on the size of the lake, but this factor should be acknowledged here.*

REPLY: Aside from mixing, the model captures to some degree the advection of heat and constituents in the water as brought about by inflow or outflow dynamics. For example, new inflowing water would add a layer and lift up layers above its insertion depth, which could be considered as vertical advection. Short term advection associated with internal waves, or perhaps upwelling in large lakes, however is not included. We will add clarity to this capability in this section in the full revision.

9. *Eq. 4 has issues with sign conventions, units, and possible missing terms depending on how one understands it. It is E multiplied by lake area should be considered a volume flux. For practical purposes these can be considered equivalent, but there would be another term for water density to convert between mass flux and volume flux. Are the evaporation, snowfall, and rainfall defined as being specifically over the area of the lake itself, or over the drainage basin? If over the lake, how is the Q term for runoff optional? I have a hard time imagining many cases of lakes in which it is not a major term in the water balance. If these variables are defined over the whole basin, is there an issue with agreement in timing between $P - E$ and runoff?*

REPLY: Eq 4 and this section is now corrected in the preliminary revision, with units clarified in Table 1. The heat flux conversion to volume is clarified in this version, accounting for density. The P , E and S terms are computed over the active lake

surface. The inflows from the drainage basin into the lake are set by the user as an "inflow" (section 2.6). The local runoff term is not referring to runoff from the wider drainage basin where the lake sits, but the local land within the lake area ($A_{max} - A_s$) that is not inundated with water. This could be important in reservoirs where the volume is drawn down significantly from the maximum level set by the user, or for ephemeral wetlands; in both these cases this area would generally not be considered as part of the inflows from the wider drainage basin, and so Eq 7 (in the preliminary revision) allows for this "dry lake-bed area" to contribute to the water balance.

10. *P. 7, line 21: "However" is an interjection, not a conjunction. Here, it stands between two independent clauses, so preceded with either a semicolon or period. Also p. 33, line 10 and p. 35, line 11.*

REPLY: This is updated in the preliminary revision

11. *Eq. 7 is a place where I started to distinctly feel the problem of definition of variables. I had to really think through what had happened to the subscript S from eq. 6 and why the "hats" were there in eq. 7.*

REPLY: This is updated in the preliminary revision; both the updated notation in Section 2 and Table 1 should prevent this confusion.

12. *Eq. 9a has a strange step function in space. Can you justify this not being a smooth function, but rather one value for all of the northern hemisphere, another for the equator (an infinitesimal amount of space), and another for the southern hemisphere?*

REPLY: This function has been previously used by many authors for albedo and so is included - we acknowledge this option is simple and hence Option 2 and 3 are provided in the model (Eq 12 in the preliminary revision), with specific resolution of the solar angle.

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13. P. 9, line 6: *What makes the different formulas oceanic and lacustrine? Is there anything to help the reader's intuition for why these formulas should be different?*

REPLY: We will add detail here in the full revision.

14. *Audience issue: P. 10, line 14 departs from the main conceptual theme to give a specific file name. Detailed mathematical formulation and detailed user information don't mix very well.*

REPLY: We agree with this observation and will remove the user detail in Section 2 in the full revision.

15. *Eq. 14: Are you using a different temperature at the skin (interfacial layer) or bulk temperature a little deeper? Specify how deep.*

REPLY: The surface temperature used in the long-wave computation is from the upper-most model layer. This is homogeneous over the depth of the surface layer (surface layer has a sub-script denoted "s"), however we cannot specify the exact depth, as the model computes the surface layer thickness dynamically, based on the density gradient and layer limits. A sentence will be added to clarify this for readers, mentioning the indicative depth as ranging from $0.5\Delta z_{min}$ to $0.5\Delta z_{max}$.

16. P. 11, line 10: *Air temperature at how high above the surface? Standard is 2 m, but there may be adjustment needed if measurements are at a different height than that intended in the formulation.*

REPLY: The air temperature used throughout the paper is now standardised to 10m and this is defined specifically in Table 1; see the preliminary revision upload. For users with data from a different height, that may use the f_U parameter to scale the data appropriately.

17. *Eq. 163: "Brutsaert" is misspelled.*

REPLY: Updated in the preliminary revision

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18. P. 11, line 13: *The range of octals should be 0 to 8.*

REPLY: Updated in the preliminary revision

19. P. 12, line 11: *This should specify molecular weight of dry air, and it might be better to say mass rather than weight.*

REPLY: Updated in the preliminary revision

20. P. 13, lines 5 and 9: *I think these should reference eqs. 17-18 rather than 16-17.*

REPLY: Updated in the preliminary revision

21. Eq. 23: *The Latin ν and Greek ν are somewhat difficult to distinguish here, and are even more difficult to distinguish in the text following.*

REPLY: Updated in the preliminary revision; the notation has been standardised, thereby removing this issue.

22. P. 13, lines 16-18: *Thanks for explicitly using units here. Am I correct in understanding that molecular heat conductivity is molecular heat diffusivity multiplied by heat capacity? I think it would be better not to use heat conductivity, but use diffusivity and capacity.*

REPLY: Note that units in the preliminary revision are updated in the text where appropriate, and Table 1 now includes all units for all variables. You are correct that conductivity is related to diffusivity and heat capacity. The notation adopted (now Eq 27) is based on the original description in TVA (1972), however, we agree it is redundant in a way and will modify accordingly.

23. P. 13, line 22: *“may be” should be two words.*

REPLY: Updated in the preliminary revision

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24. P. 15, line 13: *This use of “conductive heat flux from the ice or snow cover to the atmosphere” particularly triggers my thought bias as a meteorologist. That bias prompts me to assume that this means flux through the atmosphere’s interfacial layer, where molecular diffusion dominates. But I also wonder whether it means heat conduction through the ice. These are not equal in general, and need to be distinguished from each other.*

REPLY: It does mean the upward conduction through the upper ice layer. For clarity this is depicted in the schematic in Rogers et al (1995) that we had mentioned in the text, on which the model is heavily based (Figure 5 in this source). As such, we chose not to reproduce the energy flux schematic in this manuscript, but will revise the text to prevent this confusion, and propose to add an energy flux (sub-)plot specific to the ice model in the full revision given our notation is now slightly different.

25. Eq. 26: *Why isn’t shortwave radiation here?*

REPLY: This is the way the approach was reported in Rogers et al (1995) (Eq 7 in their paper) and we have followed that; in this case at the surface interface it is assuming the non-penetrative heat flux is in balance with the conduction of heat up to the surface. The conduction of heat up is based on the amount of light that has penetrated (as computed in Eq 31 in the revised upload). We will update the text here to prevent confusion and be more explicit that the net is referring to the non-penetrative component.

26. P. 16, lines 11 and 12: *These are very wide ranges of albedo. Please describe the conditions under which different parts of this range manifest.*

REPLY: The variation is discussed in the Vavrus et al (1996) paper that we cited; we will add a brief explanation about the variability to this sentence in the full revision.

27. P. 18, line 10: *The idea of energy required for mixing needs to be intro-*

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duced more carefully. If you think of the water column as continuous in space, mixing is also a continuum. But the concept here is based on discrete layers, and this indicates how much energy is required to outright include a model layer in the mixed layer.

REPLY: The approach to bulk mixed layer models is well established and as such we provided suggested references for readers interested to gain a deeper understanding of the background: Kraus & Turner (1967) and Kim (1976) and Imberger & Patterson (1981). The model approach we have adopted also has been the subject of numerous prior publications that have thoroughly validated that the layer discretisation can accurately capture the mixing continuum in monomictic and polymictic lakes. To provide further clarity on this point we will revise the opening text in this section (Section 2.5), as suggested by the reviewer, in the full revision.

28. Eq. 31: C sub T seems completely undefined.

REPLY: Updated in the preliminary revision via Table 1

29. P. 20, line 14: This cannot be reproduced unless your definition of “epilimnion” and “hypolimnion” are precisely stated.

REPLY: Updated in the preliminary revision (now Eq 42.)

30. P. 20, line 20: Is K sub epsilon eddy diffusivity.

REPLY: Thankyou for noting this; the preliminary revision now includes an update to specify this.

31. Since the vertical axis in Fig. 8 has zero at the bottom, it appears to be height above the bottom, not depth. It might be worth explaining that the varying top height of the color fill is due to varying overall depth (assuming that this is correct).

REPLY: You are correct; in the preliminary revision we have been more careful in use and notation associated with depth, height and elevation, and in the full revision will

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update this figures as suggested to mention height instead of depth.

32. *P. 22, line 13 and eq. 43: This seems to say that sigma should have units of inverse seconds squared, which implies that the square-bracketed part of eq. 43 has units of (m/s) squared, but exponential operations can only be performed on unitless numbers.*

REPLY: Thankyou again for noting this inconsistency. Both the top and bottom of the fraction in this equation (Eq 50 in the preliminary revision) are in the units of m^2 , so the fraction for the exponential is dimensionless. The term sigma is poorly described in this sentence and we will update this in the full revision to be more explicit.

33. *P. 24, line 5: Slope is usually defined as a ratio of rise divide by run, not an angle. I would say that the slop is the tangent of the angle.*

REPLY: You are correct, we will update this in the full revision.

34. *The inset in Fig. 10 does not help me understand the meaning of the angle alpha, and it could use larger lettering.*

REPLY: Note that we have made an initial modification to this figure in the preliminary revision, however, we will refine the clarity of this schematic in the full revision to better depict the angle and slope, and readability.

35. *Equation 52 seems messed up. Units-wise, it seems to make more sense if h squared is in the denominator instead, but I also wonder why there is no dependence on width of inflow.*

REPLY: We apologise for this error and it has been corrected in the preliminary revision (now Eq 59). The scheme does not account for the width of the inflow, thereby assuming the inflow intrusion has a roughly uniform thickness and width as it progresses down the bathymetric depression (termed the thalweg). Whilst this may not be exactly the case, the approach implicitly assumes that any change in

width would be compensated by a change in thickness (due to conservation of mass) as the inflowing water progresses – and therefore the net area Q is being divided by is therefore equivalent. This is obviously a simple approximation for an inflow parameterisation in a 1D model, and we will add clarity to the text to better explain this point in the full revision.

36. P. 26, line 16: In standard usage, dz represents an infinitesimal difference in z in continuous space, but here it is used to indicate a finite distance in discretely sectioned space.

REPLY: This expression and associated notation has been revised in the preliminary revision upload (now Eq 67). The symbol δ is now uniformly used to refer to a length-scale as is the intent here.

37. P. 26, lines 16-17: It is difficult to understand the distinction between “height of withdrawal” and “edge of the withdrawal layer”.

REPLY: This description has been further developed in the revision (now Page 28 Line 10), to explicitly define how the withdrawal thickness and the relevant layer edge is determined.

38. P. 26, line 19: Why say “fluid” rather than “water”?

REPLY: You are correct that water is more specific – we will replace the use of fluid with water in the revision.

39. A formatting problem put some labels that belong to Fig. 12 on p. 27, while the rest of the figure is on p. 28.

REPLY: This has now been rectified, and will be checked in the final revision.

40. P. 28, lines 10-11: Removing no more than half of a layer’s mass per time step seems like a reasonable way to ensure numerical stability, but it would be good

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to re- mind the reader here of the layer merging scheme that is likely to kick in. This merging and disaggregation scheme, also mentioned on p. 34, is never really described well.

REPLY: Thankyou for this suggestion, we will clarify this in Section 2.6.5 and also 2.6.3 in the full revision. Note that we have revised both these sections to improve clarity of these descriptions in the preliminary revision, including making both consistent by enforcing a 90% limit. The merging and splitting scheme is introduced (page 6 line 7 of the preliminary revision), however we note the concern this is not described fully, and we will therefore address this by adding further detail in the full revision.

41. P. 29, line 3 says “wind speed and fetch...calculated as”, while eq. 61 only shows the formula for fetch.

REPLY: The sentence introducing this Eq will be re-worded to remove this ambiguity in the full revision.

42. To help solve the problem of small print in Fig. 16, it may be useful to transfer some of the information to a table instead.

REPLY: Thankyou, we will address this in the full revision, along-with the general comment 8.

43. P. 34, line 19 has a placeholder for a citation. This is evidence of a poor final edit on the part of the authors.

REPLY: Updated in the preliminary revision; apologies for this oversight.

44. P. 34, line 23: If you mention calibration, it would be well to describe this process more fully, in particular which parameters you consider adjustable for purposes of calibration. Where p. 35, line 10 mentions “compare”, I wonder whether this might also imply calibration.

REPLY: The initial line referred to is an introductory statement with the intent that the

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text in Section 5.2 would describe the calibration process more fully. We will revise this section in the full revision to link to better link to our associated model validation paper (Bruce et al., 2017) and list the recommended parameters for fitting in Table 1.

45. P. 48, line 12: "Anneville" misspelled?

REPLY: Updated in the preliminary revision

References cited in this comment are as listed in the original submission.

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

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