Geoscientific Model Development Discussions

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Reply on RC4

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Author comment on "Modeling subgrid lake energy balance in ORCHIDEE terrestrial scheme using the FLake lake model" by Anthony Bernus and Catherine Ottlé, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-432-AC4, 2022

This manuscript reports on experiments with the 1-D lake model FLake, which has been incorporated into the ORCHIDEE land surface model for global scale simulations driven by various atmospheric forcing datasets. This is the first (and necessary) step in the development of a fully coupled atmosphere-lake modelling system (in this case LMDZ apparently). Even though FLake has already been coupled with other NWP systems, and some of the deficiencies found here are already known, this study is still of interest because the problem of lakes in environmental prediction is so important. However, as outlined below, some clarification is needed in a few areas, and interpretation of some of the results seems a bit oversimplified. Also, the paper would be strengthened with a more detailed analysis of bias, and some discussion on the problem of partial ice cover.

Response: We would like first to thank you for your comments and suggestions. The manuscript was thoroughly revised following the four reviews and especially the evaluation of the ice cover is now discussed more deeply, the observations are better presented and the problem of the partial ice coverage is better discussed.

Key Points

The 1-D lake model descriptions (especially how they differ from each other) and references mentioned in the Introduction could be improved. In FLake only the metalimnion temperature profile is estimated via a similarity argument, not the entire column (Line 57). In fact a surface mixed layer is computed based on a (more or less conventional) bulk turbulence kinetic energy (TKE) approach. As such the model has only two layers: a surface mixed layer (with a computed mean temperature and thickness), and a metalimnion (with parameterized temperature profile), making it exceptionally inexpensive to run in terms of computer resources. In contrast, the Hostetler model solves turbulent mixing on a grid (*e.g.* 10 – 25 levels) based on a parameterization of eddy diffusivity. An important reference for this approach, and also the model LISS, that should be mentioned is Subin *et al.*, 2012 (*JAMES*). I agree that in some sense the CSLM (MacKay, 2012) is a kind of “hybrid” approach – employing a similar bulk TKE surface mixed layer model to that used in FLake, along with a resolved metalimnion and hypolimnion (~ 20 levels). A very recent CSLM study – one quite similar to your own – is Garnaud *et al.* 2022, *JAMES*. Because the goals of this study have significant overlap with your own, a comparison of your results with theirs would be of great interest. Note that the CSLM was not included in the LakeMIP studies of Stepanenko et al. (Line 60). On the other hand, that study did include several k – ε models, representing an alternative to the simple Hostetler eddy diffusivity approach of modelling turbulent mixing. If any of these has been coupled to a land surface or atmospheric model it might be worth a mention in your Introduction.

Response: The model LISSS was mentioned without the reference, we add the associated reference as you recommended it. We discuss the result of Garnaud et al. 2022 in the discussion part. We corrected the mistake where we mentioned that CSLM was included in the LakeMIP study . We did not added k - ε models since they have not been used in ESMs, and this introduction was focused on lake modeling in ESMs.

The LSWT RMSE seems fair in general (2 – 4 K), but how do these values compare with other studies?

Response: The comparison with the results obtained by previous studies are discussed in the manuscript. We only focused on the global scale works and forced simulations, which were presented in Dutra et al. 2010. We have more difficulties to compare with others studies such as Lemoigne et al. 2016 where only the difference of RMSE between the different experiences are shown.

Also, there is too little discussion of bias. The ice phenology analysis needs a bit more clarity. It seems that the start of freezing (SOF) indicator – both in the simulation and the observed data – refers to the date of complete (*i.e.* 100%) ice cover. Since FLake cannot really model fractional ice cover this seems reasonable. But many large lakes, for example the Laurentian Great Lakes, virtually never freeze over completely though they all produce partial ice cover. Thus there may be a large error in the simulation of ice cover that you are missing. Your bias statistics in Table 2 can only include lakes that freeze completely in both the simulation and the observations (in order to compute the bias), and thus under-represent a potentially important model deficiency in this (and all) 1-D lake models. This is an important point and warrants some discussion.

Response: We only take lakes that are completely frozen in both simulations and observations. We highlight this point in our new version according to your remark (line 340-342). We discussed more deeply about this deficiency in the discussion part in this new version.

It is interesting to see the partition of RMSE into bias and other components in Fig. 4 but the analysis is incomplete in that the sign of the bias is not analyzed. The 8 lakes examined in the figure seem to generally show a positive LSWT bias – is that correct? Is that the trend globally? If so that could point to a systematic error in FLake. A hint is shown in Fig. 5 and in the second last paragraph of §4.2.2, but it would be very interesting to show bias results in a figure like Fig. 3, for example, or some kind of table.

Response: YES we obtain a systematic positive bias in our simulations, and this is shown in the MSE decomposition results and in the SB component. The interpretation of these biases is not straigthforward since it varies a lot among the different lakes and climates. Depth differences (between the observed lake and the modeled tile) seem to explain most of the errors for some of the specific lakes we have examined more deeply. Some pathways to better understand and possibly improve these errors are given in the discussion part, and require future work.

It is somewhat oversimplified to state that the lake freezing process is determined by lake depth (Line 507);

Response: this is the case in FLake (even though the fetch parameter depends on surface area in our case) and it was what we wanted to say. But we agree that in the real world, the lake extent and wind stress are also very important parameters to take into account.

lake surface area is an important factor in ice mechanics (*e.g.* Leppäranta and Wang, 2008, *Hydrobiologia*). For example, the mean depth of Lake Erie is only about 19 m but it rarely freezes solid. You also suggest (§5.2 second paragraph) that snow conductivity and transport might be important processes for EOF results – what about snow-ice production? Is all of the ice produced by FLake through congelation (*i.e.* black-ice) processes? Note that even a "perfect" snow model can always be tuned to improve your ice phenology statistics (*e.g.* reducing albedo to accelerate ice-off). I think it is preferable to consider missing processes before adjusting physical constants whenever possible.

Response: we agree, and try to better explain these missing processes in the discussion part, see lines 549 - 555.

Minor Comments

- Fig.3 caption differs from annotation: I assume RMSE is red, as stated in caption

Response: YES, the figure was corrected, sorry for this mistake

- Fig. 6 caption states RMSE but the figure seems to show bias

Response: what has been plotted is the dispersion of the bias among all the lakes observed, we corrected the legend in the revised version.

- Line 516: you state that SOF corresponds to the first occurrence of ice but elsewhere in the paper, as well as in the documentation for the observations (https://nsidc.org/data/G01377/versions/1) SOF corresponds to the first day of complete ice cover. Please clarify. This is an important distinction – the ice-on process *i.e.* the period from first appearance of ice until 100% ice cover can last many weeks for larger lakes (with some lakes never freezing completely at all). This period is not analyzed at all in your study.

Response: we agree that because FLake do not simulate the partial ice coverage, the first occurence of ice in FLake is probably in advance for many lakes compared to the observations that report only the complete ice coverage. We discussed this point more deeply in the discussion part and proposed ways of improvement to simulate this process in the following.