

Geosci. Model Dev. Discuss., referee comment RC4  
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## Comment on gmd-2021-432

Anonymous Referee #4

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Referee 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-RC4>, 2022

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### Summary

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.

### 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  $k - \epsilon$  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.

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

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.

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

#### Minor Comments

- Fig.3 caption differs from annotation: I assume RMSE is red, as stated in caption
- Fig. 6 caption states RMSE but the figure seems to show bias

- 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.