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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Multimodel simulation of vertical gas transfer in a temperate lake" *by* Sofya Guseva et al.

Anonymous Referee #2

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The present study is a lake model intercomparison exercise conducted in a temperate lake in Canada with a focus on biogeochemical processes in lakes and more precisely carbon dioxide and dissolved oxygen modeling. The study first focuses on the simulation of thermal stratification and ice cover, and then vertical diffusion of gases which are key elements for the vertical transport of greenhouse gases in lakes. Although five models are involved in the intercomparison, only two of them, of higher complexity, are able to model carbon dioxide and oxygen concentrations evolutions. The subject is of real interest to study in details CO2 and O2 dynamics in lakes.

General comments

It is stated that the study is a continuity of LakeMIP exercises accounting for biogeochemical processes comparisons. Although the first part of the paper is dedicated to

thermal stratification and ice cover study, and involves the five models, the fact that only two of them have the possibility to simulate O2 and CO2 dynamics, indicate that this is not a real LakeMIP type exercise, and therefore constitutes a limitation in my view to be considered as a true intercomparison model experiment for vertical gaz transfer.

In the first LakeMIP exercise (Stepanenko et al., 2010) the sensitivity of lake depth has been studied and the experiment setup accounted for simulations with maximum depth, local depth and average depth. And this is crucial especially (maybe only) for FLake model as it was demonstrated that an average lake depth was necessary for FLake simulation in order to be conservative in terms of energy. In the current setup, the maximum lake depth is used for all models and this is contradictory with a correct use of FLake which should be run using the LDSim configuration. It could be interesting to compare in the same graph RefSim and LDSim at least for FLake.

As a consequence, the sensitivity test on light extinction for FLake is not relevant since the thermal profile cannot be well simulated, and therefore should be conducted with a depth of 13.32m.

Specific comments

Looking at the bathymetry indicates strong gradients from the shoreline to the point of maximum depth. There are probably 3d-circulations that take place when dense waters flow along the bottom slopes. And these circulations are not accounted for in the 1d simulations. What is in your view the potential impact on these circulations on the thermal stratification and ice cover? And on modeled vertical transport? It would be interesting to add a discussion on that particular point.

The abstract mentions the need to improve biogeochemical processes in lake models to enhance weather prediction and climate projection capabilities. I'm not convinced improving biogeochemical processes will improve weather prediction. What is crucial in weather prediction or climate modeling is to simulate a correct surface temperature and fluxes because these are the variables that will be used in the coupling to the

atmosphere. That's true that in climate simulations the knowledge of carbon dioxide or methane emissions are of high interest, however to my knowledge only the LAKE model offers the capability to simulate CO2, O2 and CH4 dynamics and be coupled to a climate model. Please add a discussion on that point to also highlight the difficulty to increase the complexity of lake models and ensure a correct coupling to an atmospheric climate model.

Ice cover is a key variable for vertical transfer of gases. It has been shown that freeze-up or brake-up presented delays of several weeks potentially. Don't you think more effort should be put on the representation of ice and snow over ice in lake models, especially when working in NWP and/or climate contexts?

It would also be of interest to have a comparison of surface temperatures observed at 10cm to the model simulations to be sure that the daily cycle of temperature is well reproduced. This is a key feature for any further coupling to an atmospheric model. Could you add such a graph in the revised manuscript?

A comparison of methane profiles for ALBM and LAKE would also be interesting (climate change context, . . .) even if no observation is available.

Technical comments

Page 4 line 12: change – by of

Page 6 line 5: summarized

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