

Interactive comment on “A coupled two-dimensional hydrodynamic and terrestrial input model to simulate CO₂ diffusive emissions from lake systems” by H. Wu et al.

Anonymous Referee #1

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GENERAL COMMENTS The integrated modeling of watershed and lake processes is a major research frontier in ecosystem ecology. This manuscript describes this goal well in the abstract and introduction. Unfortunately, I don't view the described model and calibration exercise as a significant step towards that ambitious goal.

The CO₂ model is a useful addition, but relatively outdated given recent work on the importance of convectively driven gas exchange (Read et al. 2012, Vachon and Prairie 2013).

It seems watershed loading of inorganic carbon was not considered. This can be an important component of the carbon budget for a lake (McDonald et al. 2013), and

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makes the inference about terrestrial DOM support of lake CO₂ efflux questionable.

The number of samples available to calibrate this model seems untenable. It is difficult to constrain parameter values of a model of this complexity with large amounts of data (e.g. sensor network data). I would argue that although the model output at times captures the observations this by no means suggests that the model as parameterized provides any real information about the processes going on in these lakes. It would seem to me that a multitude of parameterizations could reproduce the minimal observations.

DETAILED COMMENTS Pg 3513, line 11: It seems a bit odd to tack on a previously published CO₂ flux model, and rebrand the model as your own, including giving it a new name.

Pg 3517, line 14-16: Seems like these differences in cell dimension might impact numerical solutions and overall dynamics.

Pg 3517, line 20: How far is this distance?

Pg 3517, line 22: A brief description of TRIPLEX-DOC model would be helpful.

Pg 3517, line 22: Is there any knowledge of the hydrologic connections between the lakes and their watersheds? Are their inlets that dominate surface water inputs? What role do groundwater inputs or outputs play in the hydrologic budget?

Pg 3518, line 1: A blue-green algae seems a bit of an odd choice for the single phytoplankton species if these systems are truly pristine.

Pg 3518, lines 6-13: Please clarify what is meant by “kinetic coefficients”. Are the authors referring to production and consumption of carbon constituents here?

Pg 3518, lines 14-24: How many sites were used during this sampling are the sample numbers e.g. “(16 sampling in 6 days)” indicating 16 time points or 16 total samples including multiple sites in the lake?

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Pg 3521, lines 5-7: I'm unclear how this can be the take home message given all of the issues reproducing the small amount of observation data described in the paragraphs preceding this sentence.

Pg 3521, line 15: Figure 6 does not convince me that this statement is accurate.

Pg 3522, line 15: No measurements of pCO₂ or flux were made near ice off. What is constraining the model during these time periods, and how do you know the model captures the dynamics of the lakes?

Pg 3524, lines 3-9: What supports net heterotrophy if there is no input of terrestrial DOM?

Section A2- The carbon cycle submodel This does not describe DOM or inorganic carbon inputs and outputs explicitly as far as I can tell.

LITERATURE CITED McDonald CP, Stets EG, Striegl RG, Butman D. 2013. Inorganic carbon loading as a primary driver of dissolved carbon dioxide concentrations in the lakes and reservoirs of the contiguous United States. *Global Biogeochemical Cycles*, 27: 285-295.

Read JS, Hamilton DP, Desai AR. et al. 2012. Lake-size dependency of wind shear and convection as controls on gas exchange. *Geophysical Research Letters*, 39: L09405.

Vachon D and Prairie YT. 2013. The ecosystem size and shape dependence of gas transfer velocity versus wind speed relationships in lakes. *Canadian Journal of Fisheries and Aquatic Sciences*, online early.

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