

Author responses to comments of referee #1

We would like to thank the referee for the effort and time he/she put in to review our manuscript. We are grateful for his/her careful and considered comments and will make every attempt to fully address these comments in the revised manuscript.

In the following list, the points raised by the referee are written in **bold** characters, whereas our responses are shown in blue characters.

Overall, I find this to be useful work. Exercises such as this are not done as often as they should be. However, I am concerned about the model calibration and the overall message of the manuscript. I am unsure what overall message the authors are advocating. They do a comparison of the different parameterizations in a 1D model and leave it at that. The manuscript also has organizational issues which make it difficult to follow. To make this work more impactful, I suggest a section on modeling advice.

Please do not be discouraged by this review. I feel this work can be useful with some reorganization and reframing of the overall message. I very much look forward to reading a revised version.

Our study is essentially a gas exchange model intercomparison study. The performance of the four parameterizations of CO₂ transfer velocity applied in this study has been estimated in prior studies by comparing the results to directly measured CO₂ fluxes and gas transfer velocities. In our study, we performed a corresponding comparison between the CO₂ transfer velocities and fluxes obtained through (a) lake model simulations and (b) calculation using measured CO₂ concentrations and other relevant variables. In addition, the aim of our study is to assess the capability of the lake model MyLake C to simulate lake inorganic carbon cycling especially under the conditions of high simulated CO₂ effluxes.

We will restructure the Methods section according to the Referee's suggestions and include further discussion on modeling advice and the overall conclusions of the study regarding the modeling of lake carbon cycling.

Specific Comments

I am concerned about the model calibration. During the calibration step, the entire ecosystem is changed for each parametrization. I understand the calibration was intended to capture the surface CO₂ concentration. I would consider tuning the model capture some aspect of the ecosystem such as chlorophyll concentration.

We performed the model calibration against water column CO₂ concentration, and the aim of the calibration was indeed to optimize the simulated near-surface CO₂ concentration because air-water CO₂ exchange is governed by the air-water CO₂ concentration difference.

In the study, four different individual calibrations of the model application were performed. Because the lake model simulates a rather complex coupled physical-biogeochemical system and the statistical inference method used in the calibration tries to find an optimal parameter set using a relatively high number of free parameters, the individual parameter sets often tend to differ from each other. In other words, each statistical calibration yields a unique description of the lake carbon cycling. This is one of the reasons why the aim of the calibration procedure was not to try to reproduce the actual in-lake carbon cycling but rather to compare different possible ways to generate an optimal water column CO₂ concentration.

There are many possible drivers of in-lake CO₂ concentration variation, for example, phytoplankton processes, microbial degradation processes, and external loading of carbon species. Because phytoplankton is only one of the contributing factors and it is known that MyLake is not highly capable of simulating short-term phytoplankton dynamics correctly, different factors

were considered equal in the calibration. In addition, comprehensive data on water column CO₂ concentration were available, whereas chlorophyll *a* measurements had not been performed in the lake during the study period.

It was also not clear why these specific parameterizations were chosen. Some rationale for choosing these specific parameterizations is needed. Admittedly, I am not familiar with most of these parameterizations, so the modeling community could benefit from a description of each. I suggest a section on “gas exchange parameterizations” where you start with a paragraph stating the gas exchange parameterizations and the parameters that go into them. I suggest putting all the parameters in a table with units. Additional sections can be descriptions of each parameterization and where it is currently being used (ie which models use them and which studies use them). Lastly, why wasn’t Wanninkhof 1992 used in this comparison? Wann.1992 is the parametrization incorporated into ocean models such as the CESM and MITgcm. MITgcm has been used to in studies of the Great Lakes. Also, the chosen parameterizations are completely different from those used in marine environments (for example, Wrobel and Piskosub *Ocean Sci.*, 12, 1091–1103, 2016). I can’t think of any reason why there are different parameterizations for freshwater and marine systems.

We selected the four parameterizations, or gas exchange models, because the performance of these parameterizations has been assessed against direct CO₂ flux measurements in Lake Kuivajärvi in previous studies by Heiskanen et al. (2014), Mammarella et al. (2015), and Erkkilä et al. (2018). Consequently, also the simulations performed in our study could be indirectly compared with direct measurements in the study lake. We will include the reasoning in the text. The model by Wanninkhof (1992) is a wind-based parameterization, similarly to the parameterization by Cole and Caraco (1998). We chose to select the simple parameterization by Cole and Caraco (1998) to represent the parameterizations that are based only on wind speed in our study. Wind speed-based parameterizations have been shown to be inadequate in small lakes, and the use of more sophisticated parameterizations has been recommended (Heiskanen et al., 2014; Erkkilä et al., 2018).

Different parameterizations of air-water gas exchange for freshwater and marine systems are needed because the main drivers of near-surface turbulence are different in these systems. It has been shown that thermal convection is a larger source of mixed-layer turbulence than wind shear especially in lakes with a small surface area and a sheltered location (Read et al., 2012). (This may be the case also in oceanic regions with low to intermediate winds and strong insolation (see McGillis et al., 2004).) Thus, it has been suggested, for example, in the two aforementioned studies, that parameterizations relying only on wind speed may be insufficient under such conditions. Many boreal lakes are small in area, and also convective processes may have an essential role in air-water gas exchange in these lakes. By contrast, all the parameterizations in Wrobel and Piskosub (2016) are based solely on wind speed or include also gas transfer by bubbles. We will clarify the limited applicability of wind-based parameterizations in the Introduction:

“Buoyancy flux is relatively more important in small, wind-sheltered lakes, and parameterizations of the gas transfer velocity that are based solely on wind speed may not be applicable under such conditions (Read et al, 2012).”

We will revise the manuscript to include a more detailed description of the applied parameterizations and related parameters according to the Referee’s suggestions. The main parameters will be tabulated and their descriptions will be given, followed by the descriptions of the gas exchange models and their prior usage. However, as far as we know, the models by MacIntyre et al. (2010) and Tedford et al. (2014) have not been widely used in other studies, let alone having been integrated into biogeochemical models. Furthermore, we consider that the model

by Cole and Caraco is so well-established and widely known in the field that there is no need to review its usage further.

I suggest a section providing modeling advice. Differences in gas transfer velocity and CO₂ flux using each method are mentioned, but there is no consensus on which parametrization the community should be using. I also suggest highlighting more the impact the choice of these parametrizations has on global efflux from lakes.

We have concluded that it is not a trivial task to judge which parameterization is most suitable for integration into MyLake C. None of the four model versions with different gas transfer velocity parameterizations surpassed the other ones in the study because of the complex interplay between the near-surface water CO₂ concentration and air-water CO₂ flux in the simulations. However, many experimental studies have shown that traditional, wind-based parameterizations often yield too low fluxes when compared to estimates based on direct measurements. Thus, we find that it is recommended to strive to use the more sophisticated gas exchange models provided that the lake biogeochemical model can be made adaptable to higher CO₂ losses and that the parameters related to the convection-based parameterizations can be simulated correctly.

We will include a more detailed discussion on our recommendations on the selection of the gas exchange model. We will also state more clearly also in the Discussion that the estimates of global gas efflux will be higher if more correct gas exchange parameterizations are used.

Technical corrections

- Make it clear GEM stands for gas exchange model. It took me a minute to realize this.

GEM is actually intended to stand for an individual MyLake C version that uses one of the four different gas exchange models/parameterizations. When the actual model/parameterization (that is, the formula) is discussed, the phrase “gas exchange model” is used in the text.

We will clarify the usage of the abbreviation GEM in the manuscript. The abbreviation has been defined at its first occurrence in section 2.2.4. We will also repeat the definition of the abbreviation at the beginning of the Results and Discussion sections.

“Even though the differences between the formulations of the gas exchange models incorporated into MyLake C are rather notable, the resultant CO₂ concentrations did not differ substantially between the GEMs, that is, between the simulations with the MyLake C versions using different gas exchange models (Fig. 1).”

“There was less variation between the air–water CO₂ fluxes simulated with different GEMs, that is, simulated with the MyLake C versions using different gas exchange models, than between the CO₂ fluxes calculated with the corresponding different gas exchange models on the basis on measured surface heat fluxes and air–water CO₂ concentration gradients (Table 3).”

- Add a table stating all the parameters with units used in each GEM

We will add the table as suggested.

- Figures 3 and 5 I suggest a cross plot off to the right with a list of summary statistics (correlation, bias, RMSE, Nash-Sutcliffe efficiency, etc.)

We will include a cross plot containing also some summary statistics along with each subplot in Figures 3 and 5.

- I suggest a paragraph of modeling advice. How does this work advance modeling of the carbon cycle in lakes?

We believe that this work addresses the issue of the selection of the gas transfer parameterization to be used in biogeochemical lake models. It is clear – on the basis of many previous

experimental studies – that traditional wind-based gas exchange parameterizations tend to give too low gas fluxes and that more advanced models with higher flux estimates should be used. However, our study suggests that it is not a straightforward task to simply use a better gas exchange model in a lake model because it may bring about difficulties in the simulation of in-lake carbon cycling, particularly in the generation of sufficient gain of CO₂ in the water column. Thus, we conclude that further development related to the mathematical description of in-lake carbon processes and to the modeling or other kinds of estimation of external inorganic and organic carbon loading are still needed.

Please see also our answer to the prior Specific Comment that is related to this comment.

- In the last paragraph of section 2.1.1 make it clear where the temperature dependent solubility comes into play. For this section I suggest looking at Wanninkhof et al. 2009 in annual review of marine science voll:213-244.

We agree that there are different ways to describe the air-water concentration difference at equilibrium by using different solubility coefficients (for example, Henry's law solubility constant, Ostwald solubility coefficient, and Bunsen solubility coefficient). We will clarify that we meant the temperature dependence of the Henry's law solubility constant K_H :

“[...] where K_H is the temperature-dependent aqueous-phase solubility (also known as the Henry's law constant) of CO₂ at surface water temperature, [...]

“[...] and the temperature dependence of the aqueous-phase solubility K_H is calculated according to Weiss (1974).”

- In section 2.1.2 It is unclear where the approximation $U_{10}/U_{1.5}=1.22$ is used

The gas exchange models by Cole and Caraco (1998) and MacIntyre et al. (2010) use the wind speed at 10 m as input, whereas the model by Tedford et al. (2014) uses wind speed at 1.5 m. In the calculations, wind speed measurements performed at 1.5 m height (which is, by chance, also the height used in the model by Tedford et al. (2014)) are used, and the conversion between wind speeds at 1.5 and 10 m is performed using the approximation $U_{10}/U_{1.5} = 1.22$. The measurement height used in this study, 1.5 m, is stated later on in the text in section 2.2.2; however, the conversion factor is included in the MyLake C model code and is thus not only a study-specific value. As the structure of the manuscript will be modified, we will move the statement to an appropriate location and restate the statement in the description of model assessment data in section 2.2.3.

- in section 2.2.2. When you say the model was calibrated against daily averages of automatic CO₂, does this simply mean the parameters in the model were tuned to match observed CO₂ concentration? Please be clear about this.

This is exactly what we meant. The calibration procedure is explained in more detail later on in the text, in section 2.2.4. In this section (2.2.2), the measurements used in the calibration procedure are described. The calibration method was statistical, and it would be imprecise to merely state that the model parameters were tuned to match the observed CO₂ concentration, to minimize the root-mean-square error (RMSE) between the simulated and measured concentrations, or to improve the fit between the simulated and calibrated values.

We will clarify the statement as follows: “The model was calibrated against the daily averages of the automatic high-frequency CO₂ concentration measurements: an optimal set of selected model parameters were estimated so that the simulated CO₂ concentration time series matched the corresponding measured CO₂ concentration time series as well as possible. The estimation was performed using a statistical inference algorithm.”

- In section 2.2.3 : please provide a rationale for this choice “Missing relative humidities

were replaced by a value of 75 % in the calculation of the water-side friction velocity”

Many half-hour values of k_{TE} could not be calculated because of missing data on surface heat fluxes (described in section 2.2.3) or other variables, and the omission of periods with missing relative humidity would have further decreased the number of calculated k_{TE} values. Thus, we chose to approximate the missing values of relative humidity.

Furthermore, relative humidity has a very small effect on the gas transfer velocity calculated with the parameterization by Tedford et al. (2014) (k_{TE}), and it is not included in the other parameterizations. The Air-Sea Toolbox utilized by MyLake includes a formula that calculates air density ρ_a on the basis of air temperature, relative humidity, and air pressure. The variation of air density at different relative humidities compared to the value of 70 % is, at maximum, of the order of 0.1 %. Because k_{TE} is proportional to $\rho_a^{3/8}$, the corresponding variation of the gas transfer velocity is even smaller. However, we chose to include the statement on the relative humidity for completeness.

The mean value of the SMEAR measurements of relative humidity during the period May–October 2013 was 72 %. Platform measurements of relative humidity were relatively well applicable during the period May–August 2013. During May–August, the average values for the SMEAR and platform measurements of relative humidity were 66 % and 68 %, respectively. Thus, the average relative humidity can be assumed to be slightly higher over the lake than at the SMEAR station. Consequently, we find that a value of 75 % is a relatively good as a rough approximation for the average relative humidity at the platform during May–October. We will explain the choice in the text:

“In the calculation of the water-side friction velocity, missing relative humidities were replaced by a value of 75 %, which is close to the average of the SMEAR II measurements of relative humidity in May–October 2013, 72 %. The corresponding averages over the period May–August 2013, for which platform measurements were rather well applicable, were 66 % and 68 % for the SMEAR II and platform measurements, respectively. Thus, the relative humidity can be assumed to have been slightly higher over the lake than at the SMEAR II station.”

- In section 2.2.4 : All the summary goodness-of-fit statistics (NS, B*, URMSE’*) can be displayed nicely in a target diagram. See Jolliff et al. 2009 “Summary diagrams for coupled hydrodynamic-ecosystem model skill assessment”

All the statistics are presented in tables in the Supplement. Because the manuscript already contains a rather high amount of figures, we consider that additional target graphs would not provide much additional value to the manuscript.

References

Cole, J. J. and Caraco, N. F. (1998), Atmospheric exchange of carbon dioxide in a low-wind oligotrophic lake measured by the addition of SF₆, *Limnol. Oceanogr.*, 43, 647–656, doi:10.4319/lo.1998.43.4.0647.

Erkkilä, K.-M., et al. (2018), Methane and carbon dioxide fluxes over a lake: Comparison between eddy covariance, floating chambers and boundary layer method, *Biogeosciences*, 15, 429–445, doi:10.5194/bg-15-429-2018.

Heiskanen, J. J., et al. (2014), Effects of cooling and internal wave motions on gas transfer coefficients in a boreal lake, *Tellus B*, 66, 22 827, doi:10.3402/tellusb.v66.22827.

MacIntyre, S., et al. (2010), Buoyancy flux, turbulence, and the gas transfer coefficient in a stratified lake, *Geophys. Res. Lett.*, 37, L24 604, doi:10.1029/2010GL044164.

Mammarella, I., et al. (2015), Carbon dioxide and energy fluxes over a small boreal lake in Southern Finland, *J. Geophys. Res.-Biogeosci.*, 120, 1296–1314, doi:10.1002/2014JG002873.

McGillis, W. R., et al. (2004), Air-sea CO₂ exchange in the equatorial Pacific, *J. Geophys. Res.*, 109, C08S02, doi:10.1029/2003JC002256.

Read, J. S., et al. (2012), Lake-size dependency of wind shear and convection as controls on gas exchange, *Geophys. Res. Lett.*, 39, L09405, doi:10.1029/2012GL051886.

Tedford, E. W., et al. (2014), Similarity scaling of turbulence in a temperate lake during fall cooling, *J. Geophys. Res.-Oceans*, 119, 4689–4713, doi:10.1002/2014JC010135.