Interactive comment on "Carbon dioxide and methane fluxes from different surface types in a created urban wetland" by Xuefei Li *et al.*

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The reviewer's comments are shown in blue and our responses in black.

Major comments

"Li et al. report a data-set of CO2 and CH4 fluxes measured by eddy-covariance (EC) in an artificial wetland in Southern Finland. The topic of the study is to quantify air-water and air-vegetation CO2 and CH4 fluxes in wetlands which is very interesting as well as extremely challenging, and rarely investigated. However, the analysis relies heavily on data gap filling, and data are reconstructed up to $>$ 70% for the first year and up to $>$ 50% for the second year. I'm aware that there is commonly a very substantial data rejection for EC measurements, and that data filling is a common and accepted practice in studies of terrestrial ecosystem fluxes. However, in terrestrial ecosystem flux studies, data filling relies on relations that make sense such as primary production vs PAR and respiration vs temperature that are based on robust biological principles. Here, the authors used correlations with the dissolved CO2 concentration to data fill the EC CO2 fluxes, which does not necessary make sense specially for the air-vegetation fluxes (because some of the CO2 signal must come from hydrological input and is independent from wetland metabolism)."

We thank the reviewer for the time and effort used to our manuscript.

The wetland ecosystem in our study is comprised of both open water and vegetation surface type, both of which contribute simutaneously to the EC measurement. As the dictinct processes involved in each surface type, the relationships between environmental variables and EC fluxes are very complicated, which makes the gapfilling using traditional process-based method difficult. Therefore, we gap-filled the EC data using an artificial neural network (ANN) model. ANN is essentially a empirical non-linear regression model (Papale & Valentini, 2003), which is a data-based model rather than process-based models such as Michaelis-Menten light response function for photosynthesis. ANN is known for its capability of modelling complex relationships (Moffat et al., 2007; Richardson et al., 2008). The input parameters of the model are chosen to maximize the model accuracy in keeping with the principle of parsimony. The dissolved CO_2 and CH_4 concentrations are chosen in the model as they greatly increased the model precision (see Figure S2 from supplement material). This is also reasonable because a fraction of the flux measured by EC tower comes from the diffusive fluxes from the open water which is linked to gas concentration in the water.

"Furthermore, the authors use the CO2 concentration to compute the air- water CO2 fluxes that are then used in a more detailed analysis in conjunction with the EC CO2 fluxes to discuss the relative contribution of air-water and air-vegetation fluxes. So, the same variable (CO2 dissolved concentration) is used to compute two variables (air-water CO2 and EC CO2 fluxes) that are subsequently treated as independent, when they are obviously not. This, in my opinion, strongly weakens the analysis and conclusions of this study."

We do not fully understand the comment on the independency of the variables. We did not assume air-water CO_2 and EC CO_2 fluxes to be independent quantities. Rather, they are interlinked as EC flux is comprised of air-water flux and air-vegetation flux. We calculated the air-vegetation flux (f_{veg}) using eq. 7:

$F_{EC} = F_{water} \times f_{water} + F_{veg} \times f_{veg}$

We then simply added up the numbers to obtain annual balance of the flux. We did not apply any statistical model to the calculated variables where the independency of data is required.

“My other concern is that the air-water CO2 fluxes were computed from a gas transfer velocity parameterization, when it could have been relatively easy and inexpensive to measure it directly with floating domes. While it is not necessarily very constructive to point out what should have been measured, I have also some strong concerns on the choice of the parameterization. The gas transfer parameterization of Cole and Caraco (1998) was developed for large lakes, and is most probably inadequate for very small water bodies (such as the one in the present case) that usually have much lower gas transfer velocity values (Holgerson et al. 2017). The gas transfer velocity in small water bodies are even less constrained than in larger water bodies, and are bound to lead to a large source of uncertainty for computation of the fluxes that will propagate into the additional analysis based on these fluxes. Turbulence (hence gas transfer velocity) in small water bodies is mainly related to convection and less to wind speed (Holgerson et al. 2016), so wind speed based parameterizations are inadequate for small water bodies.”

We are fully aware of the limitation of using Cole-Caraco parameterization to estimate air-water fluxes from small lake (discussed in Line 232- 239). To quantify the potential uncertainty, we have calculated the gas transfer velocity using another model which takes heat flux into account (Heiskanen et al., 2014). However, due to the shortage of the incoming shortwave and incoming longwave radiation data, applying Heiskanen model has created much larger gaps. Especially for CH_4, the spring peak in air-water CH_4 flux would then be completely missing. Additionally, unlike ponds surrounded by the forest (Holgerson, Farr, & Raymond, 2017), the water body in this study is located in an open area where the contribution of wind shear to the turbulence in the surface mixed layer should be relatively high. Like in our study, the parameterization of Cole and Caraco has been similarly applied to connected small open-water pools in a restored wetland which found reasonable agreement between the model estimation and the measurements (McNicol et al., 2017). Furthermore, the estimated air-water fluxes of CH_4 and CO_2 based on the current model were well within the range of the diffusive gas fluxes over small lakes from other studies (Erkkila et al., 2018; Mammarella et al., 2015). Therefore, we decide to continue using Cole-Caraco model to estimate diffusive fluxes from the water, bearing in mind that the calculated fluxes can be underestimated.

Minor comments

“L 51: What “UN report” ? Please provide a reference.”

The reference is : United Nations, Department of Economic and Social Affairs, “Global Sustainable Development Report 2016”, New York, July, 2016.

“L58: The Kyoto protocol is obsolete, we’ve moved on to the Paris Agreement.”

We will change “Kyoto protocol” to “Paris Agreement”.

“L62-66: Are these hypothetical or based on prior studies?”

1) is based on the knowledge of vegetation dynamics. We will spell it out in the text: "When a urban wetland is newly created by rewetting the landscape, it takes time for the vegetation to establish itself in the new environment. The low coverage of vegetation at the initial phase of wetland establishment can lead to low CO_2 sequestration on a ecosystem scale." For 2), the high nutrient level in the receiving water into the urban wetland was observed by multiple studies. We will add references to back up this statement (Lu et al., 2009; Vohla, Alas, Nurk, Baatz, & Mander, 2007; Valkama et al., 2017). And for 3), we agree that natural wetlands can also exhibit large spatial heterogeneity in vegetation and hydrology, thus we will remove this sentence.

“L 66: Does this mean you assume “spatial heterogeneity” of artificial wetlands to be stronger than natural ones ? Why ? Natural wetlands also have “different processes of production and transportation of GHGs”

We will remove this sentence as mentioned above.

“L68: dissolved CO2 concentrations are usually orders of magnitude larger than CH4 concentrations, so CH4 oxidation plays a negligible role in the balance of production and uptake of CO2.”

We will remove the “oxidation of CH4 produced in the water”

“L83: ‘the situation are’ ”

We will change it to “the situations are ”

“L107: Might be useful to provide nutrient and chlorophyll levels to characterize the eutrophication of the lake.”

The level of total phosphorus and NO_3-N are provided now in Fig. 1. Chlorophyll level was not measured, unfortunately.

“L108: Please provide a reference.”

The following references will be added to the manuscript:

Varis O, Sirvio H, Kettunen J. 1989. Multivariate analysis of lake phytoplankton and environmental factors. Arch Hydrobiol. 117:163-175.

Salonen V-P, Varjo E. 2000. Vihdin Enäjärven kunnostuksen vaikutus pohjasedimentin ominaisuuksiin [The effects of restoration actions at the Lake Enäjärvi in Vihti, Finland on bottom sediment characteristics]. Geologi. 52:159-163. Finnish.

“L201: Part of the Reco signal is due to hydrological input of CO2, and does not equate with ecosystem respiration.”

We will remove the section of NEE partitioning (Line 201-214).

L 236: A nine year old paper is not a ‘recent study’. There are numerous other studies that show a disagreement between floating chamber and other methods, for instance Vachon et al. (2010). Conversely, there are numerous studies that report gas transfer velocities in lakes that diverge from the parameterization of Cole and Caraco (1998) such as Jonsson et al. (2008) and MacIntyre et al. (2010). This is particularly the case in small water bodies where turbulence is largely unrelated to wind (Holgerson et al. 2016).”

See the corresponding responses above. While we acknowledge that both wind shear and convection have significant contributions to turbulence in the surface mixed layer above small water bodies, but we think the current method is sufficient to capture the basic patterns in the diffusive fluxes.

“L240: The Fveg term also includes the CH4 ebullition component, however the fveg term for CH4 only corresponds to the vegetation, so when ebullition occurs (most of the time probably) the Fveg term is over-estimated.”

We acknowledge that Fveg term can be over-estimated as we did not have independent measures for ebullition. We have discussed about it in the text as one of the potential uncertainties in our

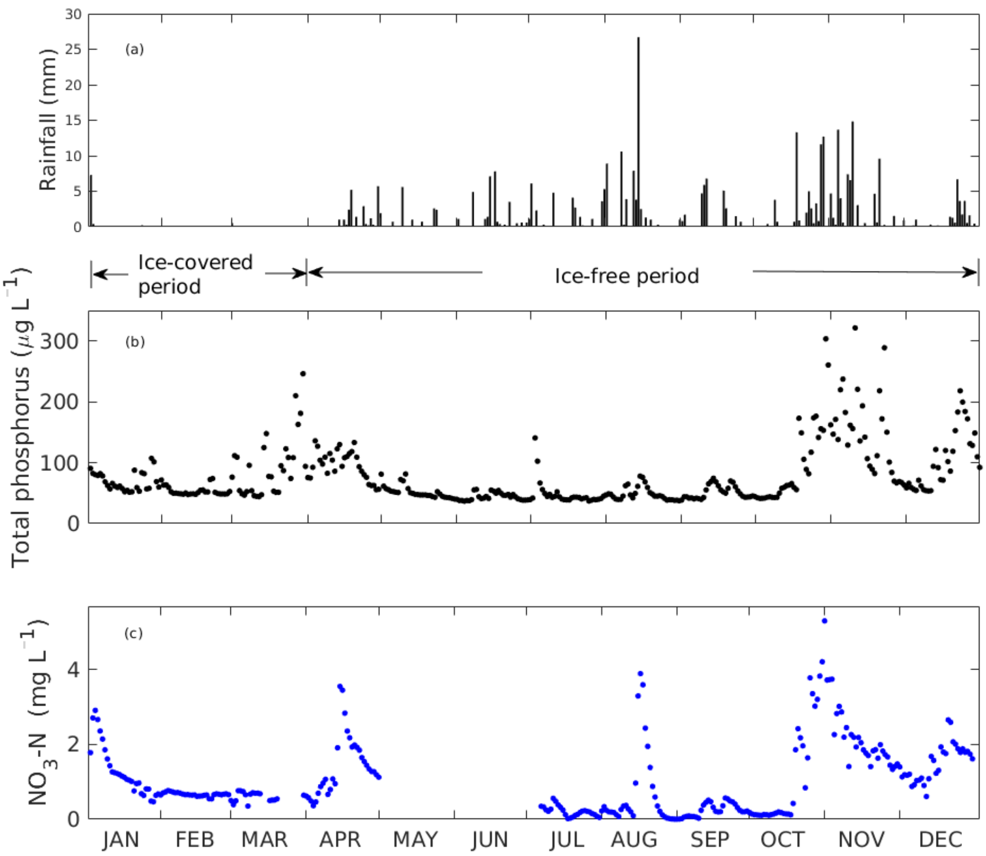

Figure 1: The daily average of (a) rainfall, (b) total phosphorus concentration and (c) NO_3-N concentration measured at the outlet monitoring station in year 2013. The lake was covered by ice from January to March and it was free of ice after the end of March.

study (Line 478-485). Furthermore, in a recent study where ebullition was measured with chambers in a restored wetland, ebullition from the open water was shown to have only minor significance accounting for 4.1% of ecosystem CH_4 flux (McNicol et al., 2017). We think that our ignorance of ebullition would not change much of the general conclusion of our study.

"L 262: This GWP value is much higher than the one proposed by the IPCC that is unanimously used. For consistency with the rest of the literature it could have been wiser to use the IPCC values."

We used sustained global warming potential with a 45 as the CO_2 equivalents of CH_4 fluxes (Neubauer & Megonigal, 2015) because greenhouse gas emissions are not single pulses, so it is reasonable to "use a sustained emission method" as mentioned by Reviewer #1. But for a easier comparison with other studies, we now also calculate CH_4 fluxes as CO_2 equivalents using a global warming potential (GWP) of 34 following the 5th Assessment Report of IPCC (Myhre et al., 2013). The GWP of CH_4 fluxes from ecosystem, water and vegetation are 0.177, 0.077 and 0.195 kg CO_2-eq m^{-2}, and they will be added to the result section.

“L 302: ppm unit in aquatic GHG literature relates to a partial pressure of CO2 and not the concentration of CO2 as stated.”

ppm unit will be converted to µmol/L using Henry’s law.

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Erkkila, K.-M., Ojala, A., Bastviken, D., Biermann, T., Heiskanen, J. J., Lindroth, A., ... Mammarella, I. (2018). Methane and carbon dioxide fluxes over a lake: comparison between eddy covariance, floating chambers and boundary layer method. *BIOGEOSCIENCES*, *15*(2), 429-445. doi: 10.5194/bg-15-429-2018

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