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Biogeosciences Discussions

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

Anonymous Referee #2

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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 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). 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.

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.

Minor comments

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

L58: The Kyoto protocol is obsolete, we've moved on to the Paris Agreement.

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

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”.

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.

L83: “the situation are”

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

L108: Please provide a reference.

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

L236: 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).

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.

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.

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

References

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