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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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While there is a growing number of CO2 and CH4 studies from natural ecosystems, relatively few studies come from urban wetlands. Hence, this paper caught my attention as being a potentially important, new and novel contribution.

What does the term urban wetlands mean and why may greenhouse gas exchange to and from it differ from other wetlands? To my mind, I would expect urban wetlands to be recycling water from urban uses and be subject to runoff from urban landscapes, which may have elevated levels of N applications, herbicides, oil runoff from roads etc. So. these factors may affect the redox ladder and alter methane fluxes compared to those from more remote wetlands. Let's see what the authors find.

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Discussion paper

I suspect the definition of an urban wetland is overly broad and more specification may be needed. In this case the authors are studying a constructed, stormwater wetland. I suspect there are many other types of urban wetlands, just look at the urban LTER in Baltimore, MD and Phoenix, AZ as a comparison. So building a database on how they may differ or be similar should be a long term goal, initated by a project like this. It would be nice to frame this urban wetland in Finland in context to those in wetter/drier and warmer worlds.

A limitation of this study is the time scale..

'The measurements were commenced the fourth year after construction and lasted for one full year and two subsequent growing seasons'.

This study is missing many of the important pulses after construction to truly understand the dynamics of this system. This aspect is one of the greatest weaknesses of this work. But given so little data on this topic, I decided it is not a fatal flaw, in this instance. But I would not view future studies of this type that miss the dynamic of the restoration pulse viable.

The authors report:

The annual NEE of the studied wetland was 8.0 g C-CO2 m-2 yr-1 with the 95 % confidence interval between -18.9 and 34.9 g C-CO2m-2 yr-1 and FCH4 was 3.9 g C-CH4 m-2 yr-1 with the 95 % confidence interval between 3.75 and 4.07 g CH4 m-2 yr-1.

I must admit I am surprised how tiny the fluxes are, given it is a wetland, even if in Finland. I would expect a stronger sink, but granted this would be conditional of what is in the flux footprint. So careful correspondence between fluxes and footprints are key to interpret these data.

As I read on I take home the key point that it is a weak sink for only 2 months and a slow C source rest of the year. Guess in hindsight it all makes sense.

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Discussion paper

As I read the introduction, I am finding necessary conditional information. For example, open water is just not always open water. With N inputs there can be other life forms. Here the authors note

'At open-water surfaces, the net production of CO_2 is a result of photosynthesis by algae, cyanobacteria as well as submerged aquatic plants, respiration of organic carbon and oxidation of CH_4 produced in the water.'

This conditions meets with some of our experiences where we see azola and other aquatic plants in the open water sections. It has changed my perspective and open to this observation. The authors will need to be careful as they evaluate their 'open' water data and inform the reader if it is or not truly open water.

Glad to see citation to the work of the Estonian team of Mander et al, as they are among the few teams looking at this problem. I would also double check literature by Bill Mitsch. Their wetlands in Ohio may qualify as an urban wetland as it was close to the University in Columbus OH. Recent reports of methane fluxes come from Gil Bohrer's group, Morin et al and others.

Glad to see the authors are clued in about the key role of flux footprints. As we bend the rules of eddy covariance and ask contemporary questions and problems, we will need footprint models to partition the heterogeneity of the landscape.

Materials

The wetland is over 500 ha. This is a good size field for this work. Standard and well vetted eddy covariance is used by experts in the field who know how to carefully interpret the data. Closed path CO_2, Licor, and TDL is used to measure methane fluctuations. Given the cold, wet environment I think closed path is best for this work. The authors have looked at cospectra to ensure filtering is limited or appropriated corrected for. Good micromet protocol.

Standard neural networks are used to gap fill. The methods are described in great detail and proper attention to nodes, validation data, etc are made.

Overall I am confident about these measurements as this team has a long history of well vetted studies. The paper needs an assessment, map of the heterogeneous fetch and the flux footprint. I did not see this in the material. It is in the supplement, but it may be better placed in the paper. Starting to lose track of what is a paper vs supplement.

This paper is novel with water ch4 sensors to apply the diffusion model. First time I have seen these sensors. Bravo/brava/bravum.

The authors try to partition fluxes by the veg water fraction. I realize this is a legitmate quest and one with good intentions. We have tried this approach in the past and failed. We used multiple towers to close the system of equations with water/veg fractions. But my student, Jacyln Hatala Matthes found that the fractal dimension of the patches was key. So be careful in your partitioning.

Matthes, Jaclyn Hatala, et al. "Parsing the variability in CH4 flux at a spatially heterogeneous wetland: Integrating multiple eddy covariance towers with highâĂŘresolution flux footprint analysis." Journal of Geophysical Research: Biogeosciences119.7 (2014): 1322-1339.

Use of the Kljun model is good. It has evolved as one of the better and most widely used.

With this the authors calculate the veg water fractions. But I must confess I don't have confidence in these numbers, especially from one tower. The reason we tried to use two towers was to get different fractions of water and vegetation with two equations and two unknowns.

I'd like to have the authors discuss the uncertainty more and critique the pros and cons of their method.

The reporting of flux reports is straight forward and standard. I have no critique or suggestions for this part.

What interests me is information on controls and processes. Here the paper has an advantage with measurements of the fluxes from the water section. But we have to be careful here. If the water is open then simple models will work. But with urban systems the N inputs can green up the water and the presence of green material will cause the diffusion models to be invalid. I need to hear more about this. So first confirm if the open water is open or is it clogged.

The controls need a bit more information on N load of the water. What is the nitrate or phosphorus levels. If there is runoff P and NO3- may affect the CH4 fluxes.

The control and process section is very simple and using correlations. I does not go into great enough detail and I am not sure if it makes a dent in our ignorance. I like methods using information theory at different time scales, I continue to worry about the roles of photosynthetic inputs to prime archaea. We learn that at different time scales temperature control may be dominant and photosynthesis may at others. Water table is important, but if it does not vary much it will not be a notable factor, yet we know mechanistically it is and if water table dropped below ground level one would see the effect.

Glad to see the authors using sustained warming potential method of Neubauer and Megonigal. I just reviewed another wetland restoration paper and they Did NOT use this method and it was a criticism of mine Methane emissions are not a single pulse, like used with the old method. It is key to use a sustained emission method.

Discussion

The authors do a nice job putting this work in context and reviewing the literature. I don't want to micromanage as there are many ways to go. I do like the discussion on O2 consumption. This is a nice angle and looks at mechanisms.

I do like seeing a bit of advice on how best to design these systems. What are the pros and cons of different water/veg fractions and what can one do to minimize methane emissions or what are the effects of nutrient inputs on the greening of open water spaces.

In closing this paper has some novel aspects and I think it will merit publication. I do think it has some lingering issues that need to be resolved. Most seriously fraction of the water and vegetation and the modeling of fluxes from the water portion if the water is not pure. The other limitation is the time scale. It misses critical dynamic of the pulse and recovery after the wetland has been developed. This is a hole that cannot be filled.

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