Biogeosciences Discuss.,
doi:10.5194/bg-2016-122-RC2, 2016

Biogeosciences
Discussions

Interactive comment on "The role of *Phragmites* on the CH_4 and CO_2 fluxes in a minerotrophic peatland in Southwest Germany" *by* Merit van den Berg et al.

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Received and published: 9 July 2016

Van den Berg Biogeosciences Biogeosciences Discuss., doi:10.5194/bg-2016-122, 2016

The research aim was to clarify (1) how this plant-mediated gas transport influences the CH4 fluxes, (2) which other environmental variables influence the CO2 and CH4 fluxes, and (3) whether Phragmites peatlands are a net source or sink of greenhouse gases. CO2 and CH4 fluxes.

The authors used direct eddy covariance method to conduct their study. The method makes direct, but net flux measurements between the ecosystem and the atmosphere. To my mind it is the best technique for studying ecosystem greenhouse gas fluxes given the new generation of laser spectrometers and open path systems that run off

solar panels so scientists can study remote wetlands where there is no ac power. This is an emerging topic and field and ripe for studies like this with new data and new interpretations.

There are some noted strengths of this work including a year's budget of greenhouse gas fluxes from a peatland. These are important greenhouse gas hot spots and only in the past few years have there begun to be continuous records of fluxes from these systems. In the past most of the work was with chambers that were episodic in time and confined to small areas and sampled periodically. The eddy covariance method gives these investigators the ability to measure net and gross carbon fluxes and then relate gross carbon assimilation with methane fluxes. How cool can this be towards addressing attributions of biophysical mechanisms towards understanding methane production and transport.

With methane exchange it is important to know if the flux is due to bubble transport, diffusion through the water column or xylem transport. Here the authors attempt to study the route by which methane enters the atmosphere. And give us insight on the dominate mechanism for transport.

Introduction

The authors do a nice job reviewing the literature and capturing key papers like those by La Mer and Lai. There is also nice work by Moore (Moore, 1994), Megonigal (Megonigal et al., 2003) and by Brigham (Bridgham et al., 2006). But it may not be necessary to cite everyone.

Methods

The authors use the eddy covariance and open path licor 7700. This tunable diode laser system is state of art, works off line and has been well vetted, so I have confidence in fluxes exceeding 10 nmol m-2 s-1, as long as the correct density fluctuations corrections are made. I would like to hear more about flux detection limits, calibration and errors. They do a good job applying the standard tests for interpreting fluxes, following work by Foken and co workers.

As for gap filling while the Falge methods are standard for CO2 fluxes other methods should be applied for methane flux gap filling. Look to the work of Sigrid Dengel et al. and others for methane gap filling. I suggest the use of artificial neural networks to gap fill methane fluxes. This approach is gaining popularity by groups such as those led by Gil Bohrer and another by the author Cove Sturtevant, in JGR Biogeosciences.

The simultaneous measurements of carbon dioxide and methane and the partitioning of NEE into GPP is what I like about this paper. There is much strength and potential of doing such coupled research. The authors are using appropriate methods to partition NEE into GPP and Reco with qualifications.

The authors compute one Q10 value through the whole data set and come up with a non biological and indefensible Q10 value greater than 2 and near 4. While this may be ok for gap filling, it is wrong fundamentally and can be misused by modelers who may look for a Q10 from these data. From lab enzymatic studies we know Q10 value is near 2. We have learned from CO2 studies that the Q10 will be artificially high when and the basal rate of respiration changes with the season. So the basal rate must be adjusted with time; this is the main lesson from the Reichstein (Reichstein et al., 2005) paper and Mahecha (Mahecha et al., 2010) paper.

There may be difficulty in interpreting methane fluxes as the source distribution may be heterogenous. The authors need to supply us with information on the flux footprint climatology.

The authors use a biserial and muli correlation method to infer that stomata control the transport of methane because light is the strongest driver. While this is plausible and possible, they do not exclude the alternative hypothesis that photosynthesis primes methane production through root exudate to the rhizosphere. Remember we are looking for a balance between production and transport in interpreting fluxes.

The authors should look at both photosynthesis and transpiration as potential drivers of their methane fluxes, too. Many are showing that exudate from photosynthesis primes microbes that produce methane, so fluctuations in light could affect photosynthesis and methane production at certain time scales. This is an important alternative or complementary path and production mechanism. While I like much of the analysis and data I feel the authors need to do more than their simpler statistical analysis to nail down the answer. They have a rich and high quality dataset that merits deeper scrutiny. Doing this and it will be a first rate paper.

In sum I am worried about the attribution of causation of the plausible hypotheses. The authors inappropriate use linear regression models for a complex, nonlinear and multifactorial process is my biggest criticism. They are bound to misinterpret their data with such an antiquated statistical method. The Gil Bohrer team fitted their data with neural networks and looked at partial derivatives with environmental drivers to explain methane fluxes. More recently Sara Knox in a paper in JGR Biogeosciences used this method to study the controls of the environment on methane fluxes. The method seems to have much power. She and colleagues found superior description of their data using neural networks compared to a simple stepwise multi-linear regression model. At least the authors should do this. Remember we are trying to tease out the controls on fluxes that are modulated at different time scales by an array of different biophysical factors to different degrees. So the problem needs to be tackled with the best and most appropriate statistical methods. In addition, the field has advanced by introducing such methods as Granger Causality and Transfer Entropy to do a better job at linking methane and carbon fluxes with drivers such as light, temperature, humidity and photosynthesis (Hatala et al., 2012; Ruddell and Kumar, 2009). The authors have the dataset to apply these methods and I feel the work and interpretation would be stronger if they used them. These methods and tools are shared on the internet through MATLAB so they don't have to reinvent the wheel.

A nice side of this work is the computations of annual sums of carbon and methane fluxes.

In sum I recommend publication after major revision. I think it has much potential to answer the important question they ask. The current paper is a good start. I only want them to aspire for better. Good luck.

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