Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-116-RC1, 2019

Interactive comment on "The ratio of methanogens to methanotrophs and water-level dynamics drive methane exchange velocity in a temperate kettle-hole peat bog" *by* Camilo Rey-Sanchez et al.

Anonymous Referee #1

Received and published: 4 June 2019

The authors study methane fluxes and their drivers in a kettle bog in Ohio in 2017 and 2018. They present monthly chamber fluxes and porewater concentration profiles from transects starting at the central open water area and ending at the upland. In addition, they sampled leaf level CH4 fluxes in 2018 and took cores to determine the relative abundance of methanogens and methanotrophs (via 16s rRNA sequencing). Their objectives are to estimate the growing season budget and to analyze biotic/abiotic drivers as well as the relationship between microbial population and the observed fluxes. The study site incorporates a restoration site, which has also been sampled and differ-

ences between undisturbed and restored sites are discussed. They find temporally highly variable fluxes and the integrated budget indicates rel. high fluxes compared to other boreal bogs (possibly due to the location at the southern border of the boreal zone, and thus higher temperatures). They find water table level a month prior to flux sampling as most important driver. Both methanogens and methanotrophs are most frequent at sites that are permanently water saturated. The authors calculate a 'methane exchange velocity' based on top soil methane concentration and the measured fluxes and can correlate this with the ratio of methanogens and methanotrophs (rel. abundance). They conclude that microbes and intermittency of the water table are important drivers for methane cycling in bogs. Methane fluxes are variable in space and time. To a large degree, the associated uncertainties come from dynamics related to the two microbial processes involved (methane production and oxidation) as well as the different transport mechanisms (diffusion, plant-mediated, ebullition). Often, water table level is used as a rough threshold between anaerobic (i.e. methane producing zone) and aerobic (i.e. methane oxidation zone). Thus, water table has often been found as important driver in small-scale flux studies. To me the most interesting part of the study is the attempt to link fluxes and microbial populations and I am intrigued by the approach to estimate the 'methane exchange velocity'. However, I think that this approach needs a more theoretical base and a discussion of its possible advantages and limitations. 1) Methane exchange velocity: The authors do not give any information about the assumptions that go in equation (1). It seems that a) ebullition and plant-mediated transport have to be excluded and b) the peat structure and water/air content has to be the same for all sites (i.e. diffusivity is identical as well). Thus, by default, the only remaining factor to explain fluxes is the net methane production (i.e. microbial processes). And that is indeed, what the authors find. Only after reading the whole manuscript, it becomes clear that assumption a) is fulfilled (although the high fluxes in summer 2018 are unexplained). 2) Microbial populations and activity: The author correctly state, that their analysis only indicates the presence of microbes, not their activity (i.e. gene expression, as was done in the Lee 2014 paper, which is cited here). However, this makes the interpretation of Fig. 8 more difficult. I would like to point out FSL-S: Fig. 7 shows that at FSL-S, very close to the top soil, methanotrophs dominate. But for the relation with 'methane exchange velocity', only top soil ratio of methanogens/methanotrophs is used (Fig. 8) – where FSL with both high ratio of methanogens/methanotrophs and high (but variable) methane exchange velocity is clearly needed for the correlation. Given these assumptions, I wonder whether the monthly porewater concentration profiles (Fig. 6 only shows the overall mean profiles) contain more information about production, oxidation and diffusive transport (i.e. the shape of the profile). If so, this can be used as further support of microbial activity as most important driver. Minor comments Page 7: Considering the high fluxes in summer 2018, I wonder whether the starting point may already have been high (i.e. an ebullition event early on)? Could be helpful to include the graph in the Appendix. Page 9: I understand why and how you do the up-scaling of chamber fluxes. However, there really is no way of evaluating that number and given the temporal variability there is the possibility that the large integrated flux is due to that (but temperature as discussed is possible as well). Page 17: Is there an explanation for the result, that the instantaneous water table does not have a significant effect, but the one a month earlier has? Page 19: Given the importance of the methane exchange velocity, I would move the figure from the appendix up to the main text and also discuss its error (from the figure it looks like that only for FSL and TMW the estimate is significant?).

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