Report on manuscript titled "Multiyear methane ebullition measurements from water and bare peat surfaces of a patterned boreal bog".

General comments

I found this paper well written and containing useful results. Design of the study is optimal for chosen object and helps to represent studied phenomena. Methods were described good enough. Especially I appreciate honest remarks on several methodological details. Literature was presented comprehensively, all important available studies were cited and used for comparison. It is very nice that photo of studied object was given in the paper (but in comments below I strongly recommend to give more photos to give a reader as much as possible information on your object). Nevertheless I recommend to improve this manuscript in several directions (they are described in specific comments section). After that I think this paper should be published.

Specific comments

1. Lines 54-55. It seems to me that in these lines it was pointed out that during methane transport in vascular plants methane is not consumed by methanotrophs. I think this is wrong because there are a lot of papers showing presence of methanotrophs in plant tissues (for example, Bao et al., 2014; Doronina et al., 2004; King, 1994). Am I right?

2. Lines 110-115. I think your object should be described in paper text more comprehensively. I understand that there are a lot of papers about Siikaneva station peatland. But more information important for YOUR study should be given. In general you describe your research sites as peatland. But you also compare methane fluxes from your site with fluxes from ponds and lakes. It seems to me that there is an ambiguity. Do you think that your objects are something between shallow lake and peatland?

It is very important for future reader to understand what EXATLY is your object. That is why more information on factors of methane emission should be given in the paper text. What was the water depth in all three site types (OW, EW and BP)? Why bare peat is bare? It is not typical for natural intact wetlands in Canada or Russia, where water table depth is about 0 cm or higher and moss cover is continuous. Why these parts of peatland are so wet (or so submerged)? Does peat on your sites removed by erosion (or by any secondary process, but not in inherent peatland development)? If so it decreases methane emission (because relatively young and rich in substrates peat layer was removed). Or your sites are in an inner water channel (stream) inside the peatland?

I strongly recommend to add photos of bare peat surface and water's edge (EW in your terms) sites and probably small map of your peatland to see where your sites are situated.

3. Lines 127-128. How do you define where is open water (OW) and where is water's edge (EW)? Based on water depth? It is important because methane emission is known to be WT-dependent (see Wik's papers from your reference list for example).

4. Lines 137-139. I have several questions to discuss on using triggered ebullition gas concentration for calculation of the total emission.

a) As I understand bubbles release to the surface from the peat when methane concentration in them reaches a certain threshold. If it is right, triggered ebullition gas concentration is lower than "real" ebullition gas concentration because gas concentration in triggered bubbles did not reach a certain threshold. Hence methane and CO_2 concentrations and their fluxes are underestimated using your methodology (not by much I think). If I am right please mention it in the paper text.

b) Methane is poorly dissolved in water. And concentration of dissolved methane in peatland water is usually high and close to saturation level. That's why I think that methane concentration in funnels during a week (actually it is less than a week, because bubbles do not release right after funnel installation) can be more or less constant and not decreased by diffusion. Did your compare methane concentration in funnels after week of exposition and methane concentration after triggered ebullition in the end of this week? I have never read about methodology you used and think that it is novel. Any novel methodology should be assessed. For example, Martin Wik (see papers cited in your manuscript) use methane concentration in funnels after week (or couple of weeks) of exposition. It is always risky to use not *in situ* concentration in such heterogeneous environments as peatlands.

5. Line 158. Where exactly your water table sensor was placed, in what site? As I see on Figure 3 you use the same WT data for prediction of fluxes from all three types of sites. But I think it is not correct because each site type has own WT mean level and WT seasonal dynamic. Anyway it must be mentioned in a paper text.

Bao, Z., Okubo, T., Kubota, K., Kasahara, Y., Tsurumaru, H., Anda, M., ... & Minamisawa, K. (2014). Metaproteomic Identification of Diazotrophic Methanotrophs, and their Tissue Localization in Field-grown Rice Roots. Applied and environmental microbiology, AEM-00969.

Doronina, N. V., Ivanova, E. G., Suzina, N. E., & Trotsenko, Y. A. (2004). Methanotrophs and methylobacteria are found in woody plant tissues within the winter period. Microbiology, 73(6), 702-709.

King, G. M. (1994). Associations of methanotrophs with the roots and rhizomes of aquatic vegetation. Applied and Environmental Microbiology, 60(9), 3220-3227.