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Biogeosciences Discussions

Interactive comment on "Low methane emissions from a boreal wetland constructed on oil sand mine tailings" *by* M. Graham Clark et al.

Anonymous Referee #1

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General comments

This paper presents results from a three year study of methane (CH4) emissions across a constructed watershed in the Athabasca Oil Sands region. The watershed contains both upland and lowland areas and was constructed on a former mine site. The authors measured low rates of CH4 emission, even at lowland plots that were frequently flooded. However, at these saturated sites, CH4 emission increased over the three years of the study. This was linked with a shift in availability of inorganic terminal electron acceptors as measured by resin probes. Therefore, the authors conclude that CH4 emission at the site is limited by the competition with metal and sulphate reducing bacteria with continued reduction of electron acceptors over time leading to the observed increase in CH4 emissions.

As there are few studies of biogeochemistry in constructed landscapes, particularly peatlands in the oil sands region of northern Alberta, Canada, this study provides new insight to carbon cycling and greenhouse gas exchange in the region. The work was carefully conducted and the results are clearly presented. The discussion is clear and concise. My only substantive comments is the use of the word ebullition and clarity in the methods on how this was defined. I have highlighted a few questions about this in specific comments, but overall, I was unclear when it came to the results, exactly what "ebullition" was referring to and how this was determined. This should be solved with some clarification in the methods.

Specific comments

Abstract – often ebullition is reported as a % of emission and even though you state that it is ratio to total number of sampling events, might be clearer to reword here as "Ebullition events occurred in 10% of measurements in 2013, increasing to 21 and 27% of measurement in 2014 and 2015, respectively at the plots with saturated soils"

I suggest rewording the results for the PCA to something like: "Using principal component (PC) analysis, methane fluxes significantly correlated to PC1 that was associated with ammonium, iron, manganese and sulphur availability". But can you also specify the direction of the correlation?

You should clarify – "alternative inorganic electron acceptors"

Line 21: 20 is really a low GWP for CH4. The Fifth Assessment Report (IPCC 2013) gives 28-34 depending on whether climate-carbon feedbacks are considered. This would be a more appropriate value to cite.

Lines 50-61: This is all fine, but it would also be good to have some overview of how rewetted peatlands compare to undisturbed sites. This sort of makes it sounds like rewetting is creating a source of CH4 on the landscape (because of course flux is higher than from drained sites), but in reality it is likely just returning the flux back to

what it would have been prior to the drainage disturbance.

Line 83: I suggest that you incorporate the note (known to contain salts and naphthenic acids) up with the definition of process-affected waters on Lines 80-81. This will help to avoid interjecting many sets of brackets.

Lines 125-126: So did inclusion in the saturated/unsaturated groups change over time, i.e., if a plot was saturated one week and then not the other, did it move between groups? What implications would this have on the analysis?

Line 128: Can you also state the horizontal dimensions of the collar here?

Line 133: So why not use taller chambers or bigger collars?

Line 166: How did you define "spike", especially at the start of the measurement? Was there a concentration threshold? A change in slope or first difference threshold?

Line 207: By saying the leading two principal components were correlated, it sounds like you are already stating a result. I know what you mean, but I think it should be reworded. For example, you could say "We used Pearson correlation to assess relationships between the first two principle components. . ."

Line 242: So these ebullition events – is this based on the "spikes" discussed in the method, or something else, based on the magnitude of flux.

Line 334: Is it cycling out of the system or out of solution? Do you think there is now a large reduced sulphur pool that could be remobilized if the site becomes drier? Or is the S lost as a gas?

Line 364: And more recently, the solid organic matter itself has been shown to be an important electron acceptor in peat soils (Gao et al. 2019).

Lines 379-383: This is a really long sentence. I suggest breaking it into two sentence to improve readability.

Line 397: So, what implications might this have on future CH4 emissions?

Technical corrections: Line 5: Typo in reference – should be Environmental

Lines 15-16: I don't think page numbers are needed in the referencing for the journal.

Line 20: its not it's

Line 159: Should be linear not liner.

References Gao C, Sander M, Agethen S, Knorr K-H. 2019. Electron accepting capacity of dissolved and particulate organic matter control CO2 and CH4 formation in peat soils. Geochemica et Cosmochimica Acta, 245, 266-277.

IPCC. 2013. Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker TF, Qin D, Plattner G-K, Tignor M, Allen SK, Boschung J, Nauels A, Xia Y, Bex V, Midgley PM (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 1535 pp.

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