Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-271-AC1, 2019

Biogeosciences
Discussions

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

M. Graham Clark et al.

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Received and published: 29 November 2019

The following response is structured in the order upon which the comments were made by RC1.

Specific comments:

We agree with the reviewer, their wording clarifies the percentage as a proportion of samples, not total emission. We made this first suggested correction verbatim.

We rewrote the sentence to reflect the clarifications suggested by the reviewer. The sentence now reads "Using principal component (PC) analysis, methane fluxes had a significant positive correlation to the leading PC which was associated with increasing ammonium, iron, and manganese availability and deceasing sulphur availability ($r = 0.31$, $p < 0.001$). "

We added inorganic to "alternative inorganic electron acceptors" as suggested for clarity.

On line 21 we updated reference and value to the fifth assessment report standards as suggested by RC1.

Comments for lines 50-61: The literature is limited in comparison to undisturbed sites. To highlight how much methane undisturbed sites produce, we changed a sentence to show their impact on the global methane budget. In the prior paragraph, where discussing wetland emissions, we made the following changes: "Methane emissions from wetlands are highly variable in space and time (Moore et al., 1998), but are significant sources of atmospheric methane. Globally wetlands represent 32% of the total sources of atmospheric methane (IPCC 2013)." We also added the following sentence at the end of the paragraph in question: "The few studies which compare emissions from rewetted and undisturbed wetlands (supplementary Table S1) show a wide range of results with rewetted wetland emissions <1% (Juottonen et al., 2012), 19% (Beetz et al., 2013), 43% (Urbanová et al., 2012), and 127% (Christen et al., 2016) of the emissions observed in undisturbed wetlands."

The note on line 83 was moved as suggested.

RC1 questioned the changes in grouping as outlined on lines 125-126. Five of the sites did change at various times. In 2013, when the water table was lower and heavily managed four sites moved between the saturated and unsaturated groups, but methane was almost absent in that year. In 2014 and 2015, the switch occurred in early spring (first week of June) before methane production increased as discussed. So we believe that this categorization had little impact on the results of this paper. More recent work by Dale Vitt and Jeremey Hartsock in the Sandhill Fen (personal communication) is demonstrating that PRS probes tend to reflect the ions towards the end of their burial period. This makes us confidant that the early spring change of group likely did not

impact the ion results as they likely were biased towards ion fluxes at the end of the burial period when the collars were stable in their grouping. We added the statement, “Only 5 lowland plots switched categories in 2013 and early spring 2014 and 2015, all periods when CH4 emissions were uniformly low with no discernible differences among the groups.”

Moved the comment “with a surface area of 0.07 m2” up to address bring all size discussion to line 128, as suggested.

In response to why we didn’t use a larger chamber: It wasn’t a regular occurrence and we didn’t have the capacity on hand to build new chambers, so it was either trim or lose the data for that whole season. Since it was only relevant at a few sites, we decided to trim the vegetation and save what data we could.

Clarified this sentence on line 166 to be “These anomalies included isolated large decreases in concentration or a return to ambient concentration or isolated or unsustained large increases in concentration and represented fluxes from a leaking chamber or an ebullition event (Tokida et al., 2007).”

Changed the language around correlating PC’s to “Using Pearson correlation, the relationship between the leading two principal components and 0.2 m REDOX measurements were assessed. Pearson correlation was also used to assess the relationship between the leading two principal components and the logarithm transformed burial period averaged CH4 flux (transformed to account for skew).”

We agree the sentence on line 242 was unclear. Since fluxes were very small in general, we intend to highlight occasions were relatively more CH4 is emitted including through ebullition. We have revised the description as, “This included occasions with ebullition or when fluxes were greater than 0.5 mg C-CH4 m-2 h-1, which is equivalent to ten times the maximum CH4 uptake rate observed at this site and is in the upper range of average uptake rates observed in grassland and forest soils around the world (Yu et al. 2017).”

RC1 asked about the fate of S in the system. That was a good question. I don’t think we can say. The sentence has been changed to “. . .mobile S appeared to be declining in abundance as the presence of mobile metals (Fe and Mn) increased.” so as not to over-interpret the results.

Thank you for bringing the Gao et al. 2019 paper to our attention. Very interesting, we added the comment as suggested.

Good idea, the long sentence was changed into the two following two sentences: “For example, Kreiling et al. (2015) found that precipitation of Fe2+ and H2PO4- lead to non-linear trends in Fe2+ ion adsorption to ion exchange resins despite increasing time in anoxic conditions. Kreiling et al. (2015) demonstrated that the precipitate removed waste products and maintained the system’s relative abundance of oxidized iron (Fe3+), thereby maintaining favourable conditions for forward reactions within Fe reducing metabolic pathways.”

Add the following sentence to speculate on future emissions as requested by RC1. “Therefore, without any other processes limiting production, CH4 emission may increase in the future.

Technical Corrections Thank you RC1 for catching these. We made all the technical corrections.

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