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Reply on RC1

Brian Scott et al.

Author comment on "Quantification of potential methane emissions associated with organic matter amendments following oxic-soil inundation" by Brian Scott et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-182-AC1, 2021

General response.

It is apparent from the comments that the scope and inference of the paper is not clear. To address this, we will make several modifications. First, we will modify the title to:

Quantification of potential methane emissions following oxic soil inundation with organic matter amendments

This also addresses a remark Commenter #1 about the title.

Also, the main point of the paper is the segmented gas production pattern, and there is a marked increase after the breakpoint. This was the case even without amendments, so inundation duration could be a means of controlling methane, which we will emphasize further.

Commenter #1

In this manuscript, the authors conduct microcosm experiments with two wetland soils, a sandy loam and a sandy clay loam, to explore how different organic amendments (from fresh to cured organic matter) affect CH4 emissions and Fe reduction. The paper addresses a topic of great interest to the biogeochemistry community, especially to those interested in mitigation efforts in wetlands. Amending soils with organic matter to increase soil carbon stocks is generally considered a key mitigation practice, so exploring systematically how different amendments affect emissions is important. The paper is also easy to follow, especially as it adopts a very simple structure. Overall, I believe the paper can be an important contribution, and I recommend publication after addressing some points of concerns described below. Generally, I think these points can be addressed by expanding the discussion and/or elaborating more on the methodology.

- There needs to be more connection between the experiments being done and the type of wetland (and location within the wetland), for which the results are relevant. For instance, the experiments are conducted under anaerobic conditions, but this is not always the case in wetland soils, as some soils are affected by tidal fluctuations or are not necessarily inundated (e.g., peatlands). In non-inundated wetlands or seasonal wetlands, there might be an interplay between methanogens/methanotrophs and between different metabolic pathways to decompose carbon. So, it seems that the experiments are more relevant to saturated/inundated wetlands (e.g., marshes or small lakes). I think that it would be important to read the authors' perspective on this.

The commenter makes makes several good points with respect to scope of inference, which we will need to clarify. First, although we tested two soils, both were sandy Atlantic coastal plain soils. We have discussed sandy soils specifically, that sandy soils may be more prone to higher methane emissions with OM amendments, but we agree additional discussion would be helpful. Second, we believe the gas production pattern would be common to many soil types (including peat) and the important factor is the duration of inundation after a dry period. Our experiment specifically excludes the activity of methanotrophs and we are only considering a continued inundation condition. Commenter #2 also pointed this out and we will address further. Another point we need to emphasize is the general pattern – inundation after a dry period - may only be applicable to certain wetlands without human intervention. However, there are instances (e.g., rice cultivation) where hydraulic control may be used, given the information here, to control methane.

- Another important point is related to the overall conclusion of the study. That CH4 and CO2 emissions generally increase upon organic matter addition is expected, I would say. But how much do emissions increase relative to the amount of C provided? The authors should consider studying the emissions normalized by the amount of C added. This normalized measure could also be more relevant in the context of wetland management and restoration. Overall, if we add organic matter to wetland soils, we should expect an increase in emissions. But the questions are: how much of this organic matter ends up being emitted as CH4? How much as CO2? And how much will it be converted into stable organic carbon? Isn't it this partitioning that ultimately helps us decide whether adding a specific organic matter (and how much) is an effective mitigation/restoration or not?

We agree with the commenter that organic amendments, and hypothesized that both CO_2 and CH_4 would increase with OM amendments (an expected outcome). This paper quantifies the increase by amendment type and dose, which we will emphasize further in the title and discussion. We did provide the amendment dose; however, correlating methane emissions based on dose is problematic because are multiple factors at work, including: amendment type, amendment dose, pH and whether the material was composted. In the case of hay, floating completely altered methane production. Also note that some amendments decreased emissions. So, in terms of management, composting and selecting an amendment that decreases soil pH is as important as dose. We will further clarify this in the discussion. We have made an effort to quantify the relative contribution of each factor, within the limits of data available. It was our intention to focus on methane emission potential, and did not discuss CO_2, or CH_4 : CO_2 ratios, but the other commenter also suggested this and we will add a table with these ratios and some discussion. With respect to partitioning of OM into soil, this analysis was problematic. Our microbial biomass numbers were nonsense, likely because the amendments add microbial biomass and the inherent variation is large. Also, the amount of added carbon used in a

60-day incubation was < 1%, well below the range of statistical variability. We are including an analysis of carbon fate in a companion field study, to be published separately, which includes the important contribution of plant roots.

- There is a lot of material in the supplementary information, which could be included in the manuscript. Right now, as soon as one starts reading the results, one needs to stop and look for the supplementary Figures and Tables to be able to follow. If they are important for understanding the analysis and findings, they should be included in the main manuscript.

In addition to adding a table with CO_2:CH_4 ratios, we will move the following supplemental figures into the main text: S4a, S5, and example from S6, and possibly S10.

- The observation that sandy loam has higher emissions than a sandy clay loam might seem trivial, if it is not discussed in more depth. Because of the higher clay content, I would assume that this is due to the higher specific surface area that tends to retain more carbon. If this is the reason, then this is well known. If there is more, then why do you suggest that they sandy loams are more vulnerable here? For example, in lines 235-239.

Adding figure S4a, along with additional discussion, should address this issue.

- The implications of the study seem important, but the authors could elaborate more on them. I suggest the authors discuss more the implications, perhaps with some rough numbers estimated from their analysis. For example, the authors mention the design of systems that regulate flooding depending on the breakthrough time. I am surprised the authors are not mentioning/citing work on rice cultivations, where this technique of managing inundation to reduce emissions is widespread. In this regard, it seems that using organic amendment with long breakthrough times can be very important in rice fields. However, in rice fields, farmers tend to use rice straw as amendment, because of course it is readily available. What would be the implications for other wetland systems? Going back to point 1, linking the analysis to wetland type can be an important point of improvement.

This is an excellent point. We could have pulled more from rice cultivation studies. Fortunately, in the intervening time from submission, there has been a key publication, Souza 2021, which mirrors the findings of this lab study in a field setting. This reference will be included and discussed, along with several other relevant publications.

Minor comments

In the introduction, the different paragraphs are not well connected with each other. There is background material without explicit link to the overarching question. I suggest reframing a bit the introduction so that the research question is clear and so is the link to the background material in the various paragraphs.

Point taken. The abstract has a clearer flow, introducing methane emissions first, then elaborating. We will modify the introduction by starting each paragraph with a reference to methane emissions. This will mostly require a re-organization of the paragraphs with little change in content.

The title mentions that adding organic material is not needed for hydric soil development, but this question is poorly discussed throughout the manuscript, so there seems to be a mismatch between title and manuscript. In my view, either the authors address this more explicitly in the manuscript, or they remove it from the title.

Agreed. We will remove this from the title. Still, it is an important point because classic redox ladder thinking is not useful in understanding our results. We will include several recent publications to clarify this point.

Line 44: what do the authors mean by "couple it"?

We will replace the text and associated reference. We mean to state that iron reduction can participate in interspecies electron transfer (Tang et. al, Secondary Mineralization of Ferrihydrite Affects Microbial Methanogenesis in Geobacter-Methanosarcina Cocultures, AEM, 2016). In that sense iron reduction facilitates methanogenesis.

Fe reduction also depends on the amount of readily available Fe oxides and is not necessarily limited by available C. Did the authors consider this? Also, Fe reduction can be important in systems that experience oxic/anoxic fluctuations (or saturated/unsaturated conditions), because Fe reduction is very fast and if there is not an oxidation step where Fe2 is oxidized back to Fe3, then Fe reduction quickly stops. So, in what wetlands or wetland position do the authors think that this part of their analysis is important?

We saw, in several cases, that iron reduction did not "quickly stop", as the commenter points out, and continued even after methane generation increased. This was an unexpected finding. The fact that we observed a pattern, contrary to the commenter's point, is why we think this is important. With respect to redox fluctuations, many other studies address fluctuating redox conditions and we did not address that here, only the length of inundation following an oxic condition.