Review of: High organic carbon burial but high potential for methane ebullition in the sediments of an Amazonian reservoir

By Quadra et al.

General Comment:
This is an interesting study on an important topic and for an understudied set of ecosystems (tropical reservoirs). I was impressed by the efforts at taking and analyzing more than a hundred sediment cores for a single reservoir and applaud authors for such a system focused analysis – there is still too few comprehensive studies based in a single system. I, however, also have some reservations that preclude immediate positive recommendation: first I was confused by your method description, particularly your estimate of sediment accumulation rates and your interpolations. This needs to be clarified. Along the same lines, I understand the need for fast publication but this dataset requires/deserves some more consideration – you mention pre and post-flooding intervals but there is little mention on this in the discussion. You basically focus your entire discussion on comparison to previously published data with many detailed numbers provided – I do not think this necessary. The ms would be much stronger if you would make a clear case that large inputs from a highly productive forest produce large C burial and CH4 emissions. Massive CH4 production and emission in sediments supplied with lots of OM and particularly in hot tropical conditions is a stand-alone argument. . This is also, as far as I see, not so much an oxygen/stratification driven effect but rather effect of high productivity – which is, in fact, interactive. You do not need to compare your findings to those from many other reservoirs and if you decide to do so, focus rather on processes and ratios than on absolute numbers.

The manuscript is well written but some sentences are a bit too complex and should be re-written.

I suggest that the entire text would benefit from a careful editing (I spotted some minor typos) and streamlining.

Specific comments:

Introduction

However, most of the CH4 is emitted from reservoirs via ebullition (i.e., gas bubbles), which is very difficult to measure due to its strong variability in space and time (McGinnis et al., 2006; 20 Deemer et al., 2016).

This is a very generous statement about ebullitive fluxes but nor necessarily correct. Some compact sediment do not allow for large bubble accumulation despite high methane concentrations. Ebullition is not always major emission pathway. Please re-phrase this sentence.

Methods

Measurements with a multiparameter sonde (YSI 6600 V2)… showed that the relatively shallow water column (mean depth: 6 m) is generally well mixed.

I am not sure whether YSI profiles can give you a good measure of stratification/mixing and besides this is a discussion already. Please state you results and revise the text.

In each of these cores, the first and second layer (0 to 4 cm deep), the last sediment layer above the pre-flooding soil surface, and about one sample every 8 cm in between were analyzed.

Why these intervals? Briefly explain or clarify sampling design.

Using a core liner with side ports, 2 ml of sediment were collected using a syringe with a cut-off tip, added to a glass vial with 5 ml of distilled water, and closed with a 10 mm thick butyl rubber stopper

We use similar method to evaluate sedimentary CH4 but samples are killed with concentrated NaOH solution, how does it work with DI water?

The CH4 concentration in pore water was measured by an Ultraportable Greenhouse Gas Analyzer (UGGA, Los Gatos Research) with a custom-made sample injection port, and the peaks were integrated using an R script

I believe that what you want to say here is that headspace concentrations of CH4 were measured with UGGA and the re-calculated to pore-water concentrations

Assuming that a CH4 concentration >80% of saturation concentration is indicative of a sediment layer prone to contain a gas bubble; this assumption mirrors the potential loss of gas from the sediment during coring and sampling.

Any literature to support this?

The average sediment accumulation rate (SAR; cm yr-1) was obtained by the ratio of post-flooding sediment thickness and the reservoir age.

Same here. Do you have any support for this method to estimate sediment accumulation rate? What about movement of sediments or turbidities? Do you have a photo of your sediment cores? Did you try to date them?

SAR was positively correlated to OC burial rate in the sites

Isn't this implicit from the method you used to calculate SAR and OC burial. You used total OC – which is clearly a function of sediment thickness to calculate burial and then you used thickness directly to calculate SAR? Either I am confusing something or both of these functions use the same dataset.

used to estimate the OC burial rate (g C m-2 yr-1) from SAR for the coring sites where OC content was not analyzed.

Ok. I am a bot confused here. Please indicate here for how many sites you have the data and how many were treated to this interpolation.

Results and Discussion

I think that this section can be much reduced by clearly discussing new findings and possibilities as well as perhaps some more quantitate analysis of inputs, Currently there is too much comparison to previous research and too little insight into the implications of this study. The work is valuable and has a potential for impact but more work in this section is needed.

Figure 3: perhaps a mass balance for sedimentary CH4 would be informative here?