

Author responses to comments of Anonymous Reviewer #2 regarding Biogeosciences manuscript bg-2019-128 “CO₂ and CH₄ fluxes are decoupled from organic carbon loss in drying reservoir sediments”

By Tricia Light, Núria Catalán, Santiago Giralt, and Rafael Marcé

We sincerely thank this anonymous reviewer for their thoughtful and constructive comments. We are confident that their feedback has helped us improve our manuscript during our revisions.

Reviewer comments and our responses are presented below. Reviewer comments are in red, our responses are in black, and direct quotes from our manuscript are in blue. Revisions are italicized.

‘CO₂ and CH₄ fluxes are decoupled from organic carbon loss in drying reservoir sediments’ by Light et al describes the contrasting roles of organic carbon metabolism and chemical weathering in reservoir sediments under contrasting hydrological conditions. This manuscript adds to a growing body of work that quantifies the joint effects of organic and inorganic carbon cycles on carbon emissions from freshwater ecosystems. Uniquely, this manuscript describes the contribution of calcium carbonate weathering to carbon emissions in a reservoir experiencing a long-term draw down.

General comments

Greater consistency in naming conventions would improve readability. For example, the names Incubation: drying and Incubation: wet-drying are inconsistently used throughout the paper. Also, using calcium carbonate instead of calcite (if there is a reason to use both terms, an explanation would help!). I appreciate that the authors’ address the high variability among replicate cores and that they call for greater spatial sampling across the reservoir. To address the limitation of only sampling within one small region of the reservoir, I would like to see some more information about the location in the reservoir the cores were collected (inflow, transition zone, or outflow). In the Siurana Reservoir is there a transition from more inorganic, watershed derived sediments near the inflow and more organic sediments near the outflow? How does that relate to your findings?

Thank you. We agree that greater consistency would improve readability, and we will revise the manuscript to consistently use treatment labels such as “Incubation: Wet-Drying”.

We will also revise our manuscript to clarify our use of both “calcium carbonate” and “calcite”. We will modify Page 6 Line 5 to read “*Albite, calcite (a polymorph of calcium carbonate), clinochlorite, dolomite, gypsum, kaolinite, microcline, muscovite, and quartz were identified and quantified.*” We will change “calcium carbonate” on Page 16 Line 29 to “*calcite*” because this sentence specifically discusses XRD results, which differentiate between calcite and other calcium carbonate polymorphs. We will add the following text to Discussion Section 4.4 to clarify our later use of calcium carbonate in place of calcite: “*Nearly all calcium carbonate in these sediments is likely to be in the form of calcite, since the Siurana Reservoir’s Ca/Mg ratio is too high to promote water column aragonite precipitation. Therefore, we will refer to calcium carbonate for the remainder of this publication.*”

Lastly, we will improve our site description by adding the following text to Methods Section 2.1: *“Exposed and submerged sites were as close to one another as possible in the lacustrine zone of the reservoir. The exposed site had sandy sediment and little visible vegetation.”* We will discuss the implications of this site selection in the second paragraph of Discussion: Drying sediment carbon loss as follows: *“Considering the large variability among replicate cores collected from the same location (which would therefore be more accurately described as pseudo-replicates), greater spatial replication within the reservoir would likely be necessary to resolve differences in sediment carbon content between wet and dry sites. Wet and dry sites were selected as close to one another as possible to allow for a drought gradient while minimizing differences in organic matter composition, but lacustrine environments can display significant spatial heterogeneity in organic matter even over short distances (Cardoso-Silva et al. 2018; Downing and Rathe 1988; Mackay et al. 2012; Pittman et al. 2013).”*

Specific comments

1) Page 2, line 24. Can you expand on the environmental conditions in which the equilibrium reaction of calcium is important and how those conditions relate to the environmental conditions in reservoirs?

Thank you. We will add the following on Page 2 Line 30: *“Additionally, shifts in the calcium carbonate equilibrium can be induced by changes in sediment moisture content, temperature, and oxygen availability, all of which are expected to occur during water reservoir drying.”*

2) Page 3, line 10. How certain are you that the wet sites have been consistently wet for the last 2 years? How would intermittent drying affect your results?

The reservoir water level at the time of sampling was the lowest it had been in more than three years, according to data from the Spanish Ministry of Agriculture, Food, and Environment (https://sig.mapama.gob.es/93/ClienteWS/redes-seguimiento/default.aspx?origen=1008&nombre=ROAN_ESTACION_AFORO_EMBALSES&claves=COD_HIDRO%7CCOD_SITUACION_ESTACION&valores=9868%7C2, accessed 1-10-19). Thus, while intermittent drying might have affected our results, we can be very certain that our submerged site has been consistently wet for at least two years and no intermittent drying has occurred.

3) Figure 1. Are the DIC-method and flux methods generally consistent for the Incubation: Wet treatments?

DIC measurements were conducted in solution while flux measurements were conducted in gas phase, so we believe grouping these two distinct analyses as one in this figure might be misleading.

4) Page 6, line 10. What are your findings? Did you look at biological activity in Incubation: wet-drying treatments?

Thank you. We will revise our results on Page 7 Line 12 to clarify that carbon dioxide uptake increased after sterilization. We will also note that the following sterilization experiment data is in Table S7 of our supplemental:

	Pre-Sterilization CO ₂ Flux (ppm CO ₂ s ⁻¹)	Post-Sterilization CO ₂ Flux (ppm CO ₂ s ⁻¹)
Replicate 1	-0.00769	-0.03729
Replicate 2	-0.00308	-0.03750
Replicate 3	-0.00500	-0.03974

We did not look at biological activity in the Incubation: Wet-Drying treatment. We certainly should make this more explicit, so we will modify Page 7 Line 11 as follows: “Post-incubation analysis of sediment from a randomly select “Incubation: Dry” core showed that CO₂ influx to the sediment increased after sediment sterilization (Table S7).”

5) Page 15, line 13. Can you expand upon the mechanisms suggested by Marcé et al 2019 and relate them to your system?

Yes, we will add the following to this section: “*Marcé et al. 2019 suggested that variability is driven by variations in moisture, temperature, distance from water-line, soil type, topography and organic carbon as well as their interactions. Thus, fluxes measured in different reservoirs could strongly vary as a function of different catchment geology and productivity.*”

6) Page 15, line 22 and the rest of this paragraph. I found this paragraph difficult to follow. Does this paragraph only consider what is happening in the top 5 cm of the core? If so, I suggest adding additional columns to Table 1 (this table currently shows that organic carbon is higher in Incubation: wet-drying) to describe the average characteristics of the upper 5 cm. The depth profile (Figure 3) also does not make it clear that wet and wet-drying differ from each other. If the analysis are for the whole core and the table/figure are correct, I do not think the evidence supports your conclusion.

Thank you for this feedback. Yes, this paragraph is only referring to what happened in the top 5 cm of the core. We will clarify this by changing the second sentence to read “*Both “Incubation: Wet” and “Incubation: Wet-Drying” cores displayed lower organic carbon content in the upper 5 cm than “Initial: Wet” cores (Table 1, Fig. 3), which is consistent with organic carbon decomposition during the incubation.*” We will also add the following row to Table 1 displaying organic carbon content for the top 5 cm of all cores:

	Initial: Control Wet	Initial: Control Dry	Incubation: Wet	Incubation: Dry	Incubation: Wet-Drying
<i>Top 5 cm Organic Carbon Content (g/gdw %)</i>	<i>4.28 ± 0.26</i>	<i>3.41 ± 0.16</i>	<i>2.81 ± 0.21</i>	<i>3.72 ± 0.07</i>	<i>2.86 ± 0.33</i>

7) Section 4.4 is missing table and figure numbers.

We apologize for this oversight. We will correct the missing references to reflect that mineralogy data is in the Supplement Table S10.

8) Page 17, line 13. In what way is climate not relevant to this study?

Thank you, we agree that this sentence is poorly phrased. We intended to refer to the increase in precipitation observed in that study, so we will change “climate” to “*increased precipitation*”.