

## ***Interactive comment on “CO<sub>2</sub> and CH<sub>4</sub> fluxes are decoupled from organic carbon loss in drying reservoir sediments” by Tricia Light et al.***

**Anonymous Referee #2**

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‘CO<sub>2</sub> and CH<sub>4</sub> 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

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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?

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?

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?

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

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

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

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.

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7) Section 4.4 is missing table and figure numbers.

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

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