

Interactive comment on “CO₂ and CH₄ fluxes are decoupled from organic carbon loss in drying reservoir sediments” by Tricia Light et al.

Anonymous Referee #3

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General comments: I think this paper needs serious reframing, as the current narrative about drying is completely unsupported by the data. The title of the paper—that CO₂ and CH₄ fluxes are decoupled from OC loss—seems appropriate. However, I have serious concerns about the conclusions presented in the abstract and discussion. As I see it, the narrative presented is a) gaseous C fluxes are higher from drying cores; b) OC is lost from drying cores, but at insufficient rates to account for the fluxes, and c) carbonate dissolution may account for the difference. However, only the first point is well-shown and supported. On the second point, the fact that wet-drying and wet incubations saw an identical decrease in sediment OC (as well as the fact that the wet and dry initial cores had identical OC) suggest that drying in and of itself may not actually result in sediment OC loss in situ. The abstract statement, “Organic carbon

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content at the end of the incubation was lower in drying cores...” is misleading in this sense.

****My most significant concern is related to this issue: if the drying core is missing a C sink (because gaseous C flux < OC loss), then the deficit must be even larger in the wet incubation core, implying there should be even more carbonate dissolution in the wet core. Data for the wet incubation core are selectively not discussed, however. *****

Potential experimental artifacts from the incubation (affecting both wet and wet-drying incubations) might be explored to explain the OC loss. Finally, the mineralogical data, as presented, do not quantitatively show that carbonate dissolution occurred in the wet-drying cores. No mineralogy data or statistics are shown, apart from a PCA in the main manuscript, that suggests (non-quantitatively) carbonate loss. In the supplemental Fig 4, it is not clear that carbonate is actually (statistically) lower in the wet-drying cores. I have not done the math on this, but could you calculate how much C is missing from the gas flux and OC loss balance, and whether that amount could be measured in carbonate loss? Furthermore, if calcite dissolution explained the missing C flux, you might expect to see a relative increase in alkalinity in the wet-drying core compared to the wet core (especially given similar loss in OC), which was not observed. Finally, along the same lines, the discrepancy between the loss of sediment OC and CO₂ flux may also be explained to some extent by the decreased alkalinity in both wet treatments (compared to the wet control). I have not done the math on this, but it should be considered.

While the paper’s title still seems applicable, the presented narrative about OC loss and carbonate dissolution due to drying is not strongly supported by the data. The same conclusions could be made about the wet core, which overturn the narrative. In my opinion, the abstract and discussion overstep the interpretation of how C processes proceed during in situ drying, and lead to misleading conclusions.

That said, this is an interesting and valuable data set, and is a great example of how

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organic and inorganic carbon processes may be important in understanding fluxes. I commend the authors for taking both into account. I think the manuscript may potentially be improved by a) being more upfront about wet and wet-drying OC similarities; b) presenting more mineralogy data (not just the PCA) to show a loss of carbonate (if there is any); (and if not) c) scaling back the conclusions (i.e., by potentially not pointing out carbonate dissolution in the abstract). Again, the discrepancy between fluxes and OC loss is interesting, but—considering the data provided—the rest of the conclusions, especially those about drying specifically, are speculative and should be discussed, but not “concluded.”

Specific comments: It needs to be discussed further why the dry core had similar OC to the wet control core, even though the experimental drying process resulted in significant OC loss. Could this be due to potential differences in the sample collection sites (which should be described in more detail)? Perhaps more recent OC accumulation in the dry core site? Just a thought; perhaps there is literature on this. The wet incubations were not stirred (P2, L30) so a significant boundary layer could have built up, preventing diffusion and limiting benthic biogeochemical processes. Can you explain/justify that this was not a significant issue, and/or represents in situ conditions? The presentation of data in the results section is thorough, but could be more streamlined. A lot of info is provided, but it's not always clear what data are important to the argument, so the reader gets lost in the p values. It would be much better to include statistics in a table and present the data in a more clear way. P1, L6: This is not accurate / misleading. OC content was not “lower in the drying core” compared to the wet incubation core. P1, L11: I would say “may be” instead of “was”. The data don't show this very conclusively. P3, L2: Can you expand on “periodically” sampled? It looks (from Fig 2) that the wet incubations were only sampled a few times. P3, L9: Can you say where the cores were collected in proximity to each other? I.e., how close? And were they collected randomly? P3, L15: Maybe I missed it, but it seems that the in situ CO₂ measurements were never mentioned again / data not shown? P4, L6: Can you say more about the accuracy of the CO₂ sensor? Were standards

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used? Is the 1% accuracy based on performance in your lab over time? This is very important since your big conclusions are comparing different C measurements taken with different methods. P4, L16: What volume of water was taken for the samples, and how was it removed without introducing an empty headspace? Also, how were the samples stored / what vials / etc? The discussion of headspace equilibration samples is also unclear (and perhaps improved if explained that it's for CH₄ in the first sentence). What is the "headspace"? Just ambient air? P5, L30: Do you have a reference for measuring pH and alk on thawed samples? I'm not familiar with this method, so wonder if the freezing/thawing process may affect these. P6, L1: Was mineralogical analysis only done for the incubation treatment samples (and not the controls)? P6, L5: Was aragonite measured? If not, you might want to say why not. While it may not be important in the bedrock, it might form from recent processes? P6, L10: I wonder if frozen/thawed samples give an accurate representation of biological activity? I imagine there is some loss from cell lysis. P6, L11: explain how samples were quantified in 5 mL aliquots. Loosely packed? P6, L12: You might want to explain briefly how the CO₂ fluxes were measured. These are not the same cores + chamber lids as for the incubations, so "as described above" isn't accurate. How was the sensor inserted into the airtight seals? How long were the measurements taken? P6, L32: Why were data transformed to arcsin/sqrt(x) instead of the straight % abundance? P11: Why not include a table of mineral concentrations, or provide % values for calcite, at the least.

Technical corrections: General observations: *In general, the reference list is a bit short, and I found several of the citations weak. It would be better to have general background info with many references than a few specific examples with single citations (P2 L14-18). A stronger reference list, and more/appropriate in-text citations would be helpful. *References could use some organizing. E.g., Donchyts et al doesn't list the journal name or number (DOI alone is insufficient). Also, in-line citations seem to be out of order in many places. (I assume they should be in alphabetical order). I recommend using a citation management software (e.g., Mendeley) if you don't already. *The writing could be tightened for efficiency and to improve readability. I recommend

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flipping through “Writing Science” by Joshua Schimel for guidance on efficient science writing.

Specific observations: Table 1: What is the unit of alkalinity? I don’t understand. Figure 2: It’s not clear what the different lines (solid, dashed, dotted) correspond to. Figure 3: Are there any points for the “initial” cores? Figure 4: The point sizes in the legend and figure don’t match (there are small points on the figure, but not the legend; maybe needs rescaling. . .

*P1, L1: A helpful suggestion: the abstract can be easily tightened for ease of quick reading: As examples: “Meanwhile, reservoirs frequently go dry due to drought and/or water management decisions. Nonetheless, the fate of organic carbon buried in reservoir sediments upon drying is largely unknown.” Could be combined into “Reservoirs frequently go dry due to drought and/or water management decisions, and the fate of organic carbon buried upon drying is largely unknown.” *P1, L9: You only need to write “mean +/- SE” for the first use. Later, it’s implied. *P2, L1: Are you referring to total burial or burial rates? (I assume the latter.) Reservoirs can have similar OC burial rates as coastal wetlands (which are arguably ocean sediments), so it might be good to mention them. Similar comment for the next sentence about eutrophic reservoirs. What is “high” burial? A value might be useful. *P2, L5: Given the ubiquity of reservoirs, “thousands” is insignificant. Consider rephrasing. Also, both the Donchyts and Ragab&Prudhomme references don’t seem totally on point; neither reference reservoirs specifically more than a couple of times. They are really about global freshwater trends, which is a different issue. I know there are other references describing annual, if not longer-term drawdowns in reservoirs. *P2, L20: “thought” implies the scientific community thinks this is the case based on evidence. “Commonly assumed” might be a better choice here. *P2, L25: “shifts in the equilibrium” is unnecessarily vague. Why not just say “precipitation or dissolution”? P3, L3: Again, “equilibrium” is a confusing word choice. *P3, L17: Was the 10L water filtered? Clarify. *P3, L23: Missing close parenthesis. *P3, L26: How was the water drained? (Drained through the bottom,

suctioned off top, etc?) *P10, L10: The alkalinity for incubation:wet doesn't seem to be the highest in Table 1. *P15, L3: The entire first paragraph belongs in results, not discussion. *P15, L13: any more references to include here? One reference doesn't seem sufficient to make the conclusion in the next sentence. *P15, L15: This could use more discussion *P15, L22: The wet-drying core appears no different than the wet incubation core in either Table 1 or Fig 3. *P16, L5: You might want to reiterate the "previously discussed" evidence here. It's not clear what you're referring to. *P16, L6: I was waiting for this to be pointed out. :) *P16, L8: But there is not large variability within replicates (i.e., errors are low relative to differences between treatments in Table 1). Thus, this explanation for why the dry control has similarly high OC doesn't make sense. I have to disagree that your data don't actually, "supports the presence of an enhanced mineralization process during drying." *P16, L15: I don't think it's a fair conclusion to say that drying results in an OC loss, because you see the same loss in the wet incubation. *P16, L17: These numbers might be better presented in mmol m⁻², so as to match the plots and earlier discussion. It took me some mental gymnastics to realize that you're now referring to the actual amount emitted from the 28 cm² core surface. *P16, L30, L32: Supplementary figure #s missing. *P17, L1: Did you measure Ca²⁺? I'm confused. *P18, L8: This could be worded better. Yes, bicarbonate is not a long-term sink (geologically, relative to silicate dissolution). But the bicarbonate won't just "transform to CO₂", unless via a) carbonate reprecipitation; or b) entering a low pH environment (wherein it still functions as alkalinity). *P18, L10: The link to forest soils is not clear. We're not talking about carbonate dissolution here, are we?

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