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Biogeosciences Discussions

Interactive comment on "Carbon leaks from flooded land: do we need to re-plumb the inland water active pipe?" *by* Gwenaël Abril and Alberto V. Borges

Anonymous Referee #2

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This paper is a timely contribution to the discussion about the role of inland waters in the global C cycle in that it connects two important aquatic elements (emergent wetlands and rivers/lakes). I found the paper to be provocative, rigorous and insightful, and thus look forward to its publication. I have several comments on the paper that are mostly second-tier issues (i.e., none challenge the core arguments, just more minor details of those arguments), as well as several editorial suggestions.

1) The equations provided are useful, but there a a few issues that the authors could consider to augment. Principal among these was the utility of a master equation that connects Eq. 1 and Eq. 2. The text is full of compelling subleties about where NEE

departs from NEP, and where NEP departs from NECB, and these are central to the overall argument. I think that returning to this master equation for each section (aquatic, terrestrial and wetland) would integrate the narrative more clearly. When the equations for the wetland budgets are presented, new terms (alpha, beta) are introduced. Alpha is described, but beta is not directly (i.e., proportion of aquatic CO2 that is transported laterally). The general use or not of subscripts to denote fractions (e.g., E[subCO2]) is not general (e.g. ARw and ARs).

2) The authors do a really nice job integrating inorganic C and organic C into the narrative. That said, I feel like there is a missed opportunity in the wetland and stream sections to actually enumerate the relative importance of the two modes of C transport. In the area where I work, DOC overwhelmingly dominates mass transport (mean DOC $\sim$ 40 mg C/L, mean excess DIC $\sim$ 3 mg C /L), suggesting a slightly different emphasis than the current paper takes (for which the focus is mostly on DIC transport). One place this manifests as an issue is L441-443. There the authors assert that if E is large (in this case not parsing organic and inorganic species), then GPP, NPP and NEP will be overestimated and ER underestimated. This is ONLY true if E is principally DIC. If, however, most of the lateral flux is DOC then the opposite would be true.

3) Throughout the paper, the authors assume strong hydrologic connectivity between wetlands and other inland waters. This is true, at least episodically, for many riparian wetlands, particularly those along large rivers. This raises an important issue, however. Many many wetlands are NOT well connected, but rather venues of distributed water storage that connect only for short intense periods, but where otherwise water flowpaths to convey C are neither rapid nor volumetrically significant. This storage is, indeed, the main reason that wetlands provide flood attenuation services. While I cannot claim experience with wetlands in all areas, the wetlands that I do work in are mostly disconnected expect via slow groundwater flowpaths except during short bursts of event driven connectivity. As such, the C budget in those flooded lands is mostly entirely vertical (during periods of weak connectivity) except when connectivity enables

transport. I think this means that for many of the wetlands that I know well, the lateral fluxes are likely to be very small for most hydrologic conditions.

4) Building on #3, the time variation of lateral fluxes and respiration pathways is not clearly considered. On P24, the authors assert that nearly all of ARs and HR occur sub-aqueous. This is not the case, unless one assumes that wetlands are permanently inundated. Most wetlands are not permanently inundated, and have sometimes prolonged periods when soils are no longer saturated, during which there is frequently a significant pulse of respiration with the atmosphere serving as the destination. This is both ARs and HR. I admit that this is a relatively minor nuance, and does not change the overarching assertion, but measuring reasonable values of alpha and beta (not to mention ARs and HR) requires that our conceptual model remain faithful to the actual processes. The timing issues are important not just because they apply to nearly every "plumbing" flowpath, but also because time variation in concentrations is informative about sources. In particular, the fact that DIC, DOC and POC all generally increase with increasing discharge suggests sources that are activated during events (consistent with the wetland source narrative). If the source were principally terrestrial, it seems to me that dilution signals would be more frequently observed.

5) The thought experiment on P24 and 25 to estimate the proportion of respired CO2 that is exported has some flaws. First, the gas transfer velocity is way too high; 1 cm s-1 is about 3000 times larger than values that we measure. I checked the Ho et al. (2018) reference and their gas transfer velocities are 1 cm hr-1, which aligns with measurements we've made, and likely represents a unit error. This should greatly INCREASE the fraction of CO2 that is laterally exported, except that the velocities that are used are (at least for the wetlands that I work on) way too high. As I mentioned above, velocities are typically $\sim$0 except during event driven connectivity periods (neglecting the very modest, but non-zero, velocities associated with groundwater transport). Even the lotic wetlands have maximum velocities of 1 cm s-1. These two assumptions cancel out, possibly rendering the ultimate inference sound. I'd add that even if the beta values are

far lower (say 5%), this is still a significant mass flux. One convenient way to frame this argument is using the Damkohler number relating advection to reaction (in this case gas exchange). I could imagine a typological synthesis across wetlands that highlights very low Damkohler numbers for distributed depressional storage, and higher numbers for riparian and littoral wetlands.

A few more minor comments:

- As I mentioned above, wetlands do not always exhibit "strong hydrological connectivity" (L27).

- I am not convinced that the authors have sufficiently demonstrated the primacy of wetland C loading in the tropics. The mechanisms are general, and the absence of direct data to support that the role of wetlands is disproportionately important in the tropics makes inclusion of that conclusion in the abstract a little bit of a stretch.

- phrasing on L168/169 is very odd. "intermittent and/or vegetated flooded land" makes no sense to me as a wetland scientist.

- The structure of the text on P8-9 yields a "Second,..." on L176 that it took some hunting to find the accompanying "first". Consider revising.

- L199 -is there a citation for this?

- I feel as though the emphasis in L286-287 is so much on DIC that DOC fluxes are getting lost. DOC fluxes can be significant in numerous low-relief settings, even from uplands.

- The mass budget closure of terrestrial systems was pretty compelling, but the conviction of the narrative on L321-323 seemed strong given how close those numbers get. The NEE vs. NECB difference could plausibly be as large as 0.7 Pg yr-1, which is in the range of OC burial + export rates.

- L396 - extra "has"

- L474 - I didn't understand the "Albeit..." part of this sentence.

- L483 - Plants transporting gases from the soil to the air (and vice versa) is NOT why wetlands are generally hypoxic. They are hypoxic because the gas exchange of oxygen is slow compared to consumption. I wonder if there was another sentence there in an earlier draft.

- L504 - should be Mitsch et al. (2013), or another reference that is missing from the list.

- The controls on beta are as described (depth, gas exchange) but also critically hydrological connectivity (which is not a constant property).

- Fig. 3 is great. It's worth noting that the wetland depicted is one modest subset of wetland area (i.e., those with perennial connections to other inland waters).

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