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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COMMENT- 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.

ANSWER- We thank the reviewer for her/his interest and detailed reading of our MS, her/his overall positive evaluation of our paper and encouraging and constructive comments. The reviewer has raised very relevant points and asked important questions concerning our understanding of carbon exchange between wetlands and inland waters; on several aspects, the reviewer highlighted some limits in our reasoning, particularly when considering wetlands only when connected with inland waters. Owing to the general limited knowledge on these topics, it is obvious we could not provide a definitive answer to all his/her five major comments; this is why we acknowledge the reviewer when he/she recognises our contribution insightful and rigorously and when she/he mentions that his/her comments are second-tier issues. Nevertheless, in our revised MS, we will temple some of our statements in the light of these comments, give more evidence on the mentioned facts based on new references to literature, put more emphasis on the remaining uncertainties, and we provide some additional quantitative information useful to support our views, particularly those on the relative importance of tropical regions. These changes will improve our MS by giving more emphasis on current gaps and the necessity to fill the gaps between land and water in the future.

COMMENT- 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 subtleties 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.

ANSWER: we will do our best to follow this excellent but very challenging suggestion; please note that in fact, no single universal equation connects Eq.1 and Eq.2, exactly because in almost all cases, all terms are significant and NEE departs from NEP and from NECB. However, it is indeed possible to connect Eq. 1 and Eq.2 if the objective is to illustrate some assumptions commonly made by ecologists, and the consequences of these assumptions on C fluxes conceptualization.

In aquatic systems, the general case is a positive NEE (CO2 source), a negative NEP (heterotrophy), a negative E (import from surrounding land) NECB can be positive (if burial of terrestrial C exceeds heterotrophy) or negative and and F_{other} can be reasonably neglected if CH4 emissions from open waters only are considered (L222-223); thus Eq.1 and Eq.2 can be combined to: -NEE = NEP + E_{CO2} Consistent with the term "internal" (-NEP) and "external CO2" (-E).

We will insert this simplified equation together with few explicative sentences in the aquatic section after L286.

"In inland waters, Eq.1 and Eq.2 are generally combined to a simplified equation that only considers inorganic C :

$-NEE = NEP + E_{CO2}$ Eq. X

with NEE positive, NEP negative (heterotrophic metabolism), and E_{CO2} negative, as rivers and lakes receive more dissolved CO_2 from groundwater than they export downstream."

In terrestrial systems, the general approach based for instance on eddy-covariance CO2 fluxes often consists in neglecting F_{other} and E; thus Eq.1 and Eq.2 are combined to: -NEE=NECB=NEP=GPP-ER; we will insert this statement in the terrestrial section L370:

…"by considering the loss of CO_2 that dissolves in groundwater as negligible or within the error of estimation of metabolic flux at the ecosystem scale. In other terms, classical approaches in terrestrial ecosystems consisted in neglecting F_{other} and E and combine Eq.1 and Eq. 2 to:

-NEE=NECB=NEP=GPP-ER Eq. Y"

In flooded systems, similar hypothesis cannot be made to link Eq.1 with Eq.2. In fact the relationship between fluxes in and out of the wetland (Eq.1) are linked to metabolic fluxes (Eq.2) according to the series of Eq. 14 to 16. Eq. 16 is the correct equation processes in the flooded area atmospheric exchange and is more complex for many reasons

$NPP+\beta ARw+\beta ARs-(1-\beta)HR = B+F_{CO2}+F_{CH4}+E$

In the revised MS, we will modify the last sentence of the paragraph L546-547 "*The three terms ARw and ARs and HR together with the E term, are generally neglected in wetland C budgets that quantify only NPP, FCO2, FCH4 and B (Mitsch et al. 2013; Sjögersten et al. 2014).*"

COMMENT- 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).

ANSWER- We will define beta in the text and use subscripts for all terms of the equations

COMMENT - 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 40 mg C/L, mean excess DIC 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.

ANSWER- we recognize that we have put more emphasis on inorganic lateral fluxes rather than organic fluxes, probably because of our own experience in tropical floodplain systems, where export of excess DIC apparently predominates. In our revised

MS, we will put more emphasis on the fact that some wetlands like peat bogs for instance exports DOC rather than DIC, citing:

Freeman C., Evans C.D. and Monteith (2001) Export of organic carbon from peat soils. Nature 412: 785

Clark J.M., Lane S.N., Chapman P.J. and Adamson J.K. (2008) Link between DOC in near surface peat and stream water in an upland catchment. Science of the total environment 404: 308-315

The sentence L441-443 was: "if wetland E is ignored but significant, GPP, NPP, NEP, and NECB deduced from the diurnal changes of eddy CO_2 fluxes (Lu et al. 2016) would be overestimated and, inversely, ER would be underestimated (Eqs.1-6)". We agree with the referee that this sentence deserves more attention and must be moderated in some way. With the eddy covariance technique, NEE is measured throughout day and night; ER is assumed equal to night-time NEE, ER is correlated to temperature; GPP is calculated as daytime NEE minus ER re-calculated for daytime temperature; NPP is calculated from the diurnal integration of GPP minus ER. Thus, in the revised MS, we will first limit our reasoning to GPP, NPP and ER. If the wetland exports DIC, then this DIC will be missing in the night-time positive NEE and its relationship with temperature that are used to calculate ER; thus ER will be underestimated, and GPP and NPP will be overestimated. If the wetland exports DOC, this organic carbon will not be respired within the ecosystem and thus the assumption that ER equals nighttime NEE will remain valid. Finally, we agree with the reviewer's comment that "this is ONLY true if E is principally DIC", but we disagree with the statement that "if however, most of the lateral flux is DOC then the opposite would be true" because in theory, DOC export would have no effect ER, GPP and NPP deduced from eddy covariance; If DOC export is significant, NECB will be overestimated; however, NECB cannot be derived directly from the eddy covariance method (reference to Lu et al. 2016). In the revised MS, we will specify that the sentence is true if E is principally DIC and remove "NECB" from the sentence above.

COMMENT- 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.

ANSWER- it is true that many wetlands are only temporally connected to inland waters; as a corollary, only wetlands strongly connected with inland waters will contribute disproportionally to the inland water C budget. We will make this point clearer in our revised MS by inserting the appropriate statements in sections 2 and 5. Because of the complexity of processes and the spatial and temporal variability of wetland ecosystem functioning, in order to apprehend land flooding as mechanism for lateral C transport, we use land flooding as operational criteria to delimit continental areas (based for instance on remote sensing) as a major process that contribute to the wetland to inland water C flux. Thus, as the reviewer emphases, wetlands almost permanently flooded will contribute continuously, whereas wetlands episodically flooded will contribute only during short periods, although C lateral fluxes during these short periods can still be significant in the annual C budget.

Two major reasons justify to put more emphasis throughout our MS on riparian and littoral wetlands: firstly, these type of wetlands directly connected to river and lake water bodies will exchange more C with inland water; secondly, these types of wetlands predominate in tropical regions owing to the specific hydrological features (dry and rainy seasons), and according to Raymond et al. (2013) and Lauerwald et al. (2015) almost 80% of CO2 emissions from inland waters occur at latitude lower than 25°. In sections 2 and 5 of the revised MS, we will put more emphasis on the difference between swamps, bogs and marshes on the one hand and riparian and littoral wetlands on the other hand. A special paragraph will appear in the section 6 ("what tools do plumbers need?") on the absolute necessity to build a global typology of wetlands that adequately address the question of hydrological connectivity with inland waters.

COMMENT- 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.

ANSWER- Yes, we agree that our reasoning do not explicitly consider wetland C balance during prolonged emersion; indeed, during these periods, C fluxes are mostly vertical, with lateral C fluxes occurring only as subterranean flow, similarly to drained land. Lateral fluxes induced by flooding follow seasonal cycles and can occur as regular flood pulse such as in many tropical wetlands, or as flash flood events. The proportion of wetland GPP exported annually to streams and rivers can be highly significant in both modes; however it would make sense if regular flooding export mostly DIC whereas flash flood export mostly DOC and POC. Probably because remote sensing tools permit their clear delimitation in space and time, aquatic scientists use to consider wetlands as the flooded area only, even if this area changes with time. This is why we preferred to use "flooded land" and not "wetland" in the title of the MS. We do not neglect the emerged period of wetland, but we consider it belongs to the terrestrial domain. We believe we can assume this limitation because our main focus is the description of the C transport mechanism induced by flooding, and not the net C balance of wetland ecosystems as ecological entities. We agree with the reviewer that we must make it very clear in the MS, particularly in sections 2, 5 and 6. In the revised MS, more emphasis will be given to temporal variation of flooding and export. Section 6 ("plumber tools") will contain a short paragraph on the importance of building a global typology of wetlands that include hydrological connectivity and other ecosystems specificities such as productivity, CH_4 fluxes, etc… In order to mitigate this defect in our MS, we will also change "wetlands" to "flooded land" when appropriate.

We thank the reviewer for sharing constructive ideas on the analysis of concentration versus discharge patterns. We agree with the idea that positive slopes suggest activation of a wetland source and a larger export of wetland C at high discharge.

COMMENT-5) The thought experiment on P24 and 25 to estimate the proportion of respired CO_2 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 CO_2 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 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.
ANSWER- we apology for the mistake in the unit. In our calculation, we have used a gas transfer velocity of 1 cm hr-1 and not 1 cm s-1. As mentioned above in previous answers, the focus of our paper is to discuss land flooding as a process that transport C to inland waters, and this is relevant only when flooding occurs and when surface water velocities within wetlands are non-zero. A water velocity of 1 cm s-1 is indeed classical for lotic wetland. However, in floodplains of large tropical rivers which are significant contributors of tropical and global CO_2 and CH_4 budget, water velocities inside a flooded forest can reach several dozens of cm s-1 during the months of maximum flood, when dissolved CO_2 concentrations are maximum. 10 cm s-1 can be assumed as a maximum velocity value. This observation also contributes to highlight the importance of temporal changes in the lateral fluxes between wetlands and rivers (comment 4 and corresponding answer).

A few more minor comments:
COMMENT- As I mentioned above, wetlands do not always exhibit "strong hydrological connectivity" (L27).
ANSWER- the sentence will be changed to "Contrarily to well-drained land, many wetlands combine a strong and dynamic hydrological connectivity with inland waters, high productivity…"

COMMENT - 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.
ANSWER- we agree with reviewer that the mechanisms we describe are general; however, their quantitative significance at the global scale is probably much more important in the tropic, where 80% of global river CO2 degassing occurs. We will mention this fact in the abstract.

COMMENT- phrasing on L168/169 is very odd. "intermittent and/or vegetated flooded land" makes no sense to me as a wetland scientist.
ANSWER- we will remove this definition probably useless for our main message.

COMMENT- 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.
ANSWER- We will write L153 "As a first step, an adequate conceptualization…" and L176 "As a second step, our conceptual model should be two-dimensional…"

COMMENT- L199 -is there a citation for this?
ANSWER- We will cite Chanton et al. (1995) here

COMMENT- 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.
ANSWER- We will put more emphasis on the importance of DOC export from some wetlands such as peats citing Freeman et al. (2001) and Clark et al. (2008), in this paragraph of the MS as well as in the latest section 6.

COMMENT - 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.
ANSWER- Indeed, we will modify this sentence to state that difference between NEE and NECB is still is the range of OC burial+export.

COMMENT- L396 - extra "has". - L474 - I didn't understand the "Albeit..." part of this sentence.
ANSWER- Will be re-phrased in the revised MS

COMMENT - 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.
ANSWER- We agree with the reviewer. In the revised MS, will remove the words "This is why"

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

COMMENT - The controls on beta are as described (depth, gas exchange) but also critically hydrological connectivity (which is not a constant property).
ANSWER- in our beta is also a function of water velocity, that is of hydrological connectivity; we will mention that in the revised MS.

COMMENT- 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).
ANSWER- We thank the reviewer for the compliment. We will change the title of the subsets delimiting ecosystems type; change "land" to "drained land" and "wetland" to "flooded land". We will mention in the legend that with such delimitation, many wetland ecosystems are temporarily in both categories; this was the best functional definition we found to conceptualize the main message of the MS.