Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-68-RC1, 2019


# *Interactive comment on* "Variations of dissolved greenhouse gases ($CO_2$, $CH_4$, $N_2O$) in the Congo River network overwhelmingly driven by fluvial-wetland connectivity" *by* Alberto V. Borges et al.

**Anonymous Referee #1**

Received and published: 8 April 2019

This manuscript is based on data from10 field expeditions carried along different parts of the lowland Congo river. Measurements of water concentrations of CO2, CH4, and N2O is in focus, but in addition many more variables including temperature, conductivity, pH, turbidity, suspended solids, DOC, POC, cations, dissolved silicate, nitrate, nitrite, ammonia, chlorophyll-a, primary productivity, pelagic respiration, and dissolved O2, methane oxidation, stable isotopic compositions – 13C in CH4, DIC, POC and 18O in water. In addition to the 10 specific expeditions, there were continuous or regular multi-year monitoring of selected variables on a few sites. Therefore, the data reported



is exceptionally rich for any river and even more impressive given that they regard the Congo river. This in itself bring a very high value to the scientific community and we should be grateful for the author's persistence and long-term dedication for studying this very important river basin.

The patterns emerging from this data is very interesting and the authors bring forward a several aspects under the umbrella of a greenhouse gas study. In my view this study contains much more than just greenhouse gases and a general comment would be to broaden the umbrella and title to better reflect all topics actually covered – also those that are not strictly greenhouse gas-related.

In general I find the text well-written and interesting, but I have some questions and comments outlined below that I think need consideration.

General comments:

- The manuscript is long and in some parts a bit hard to follow when it combines many aspects and variables in the same paragraphs. I think all information given is valuable so I am not asking for removing any of them, but if possible to provide a more clear structure and to perhaps shorten the text a bit that would make reading and understanding easier.

- In some cases when trying to explain observed patterns/correlations, it seems single cause-effect patterns are suggested, while other alternatives may also be possible but not mentioned as the main explanation. . . .and sometimes these other alternatives are brought up later in the text, separated from the first explanation. This regards e.g. to what extent patterns observed in the river depend on in-river processes or on what is brought to the river from the soils. Clearly the authors have expert awareness about all possible explanations, so this is a request to clarify that several alternative explanations are possible when relevant.

- I miss method evaluations, sensitivity analyses, reliability checks, and attempts to validate estimates regarding the greenhouse gas emissions. This is very important because the suggested extrapolated emissions of $CO_2$ and $CH_4$ are exceptionally large – much larger than reported from any other river worldwide - including the Amazon. There are many potentially very large sources of uncertainty in the flux estimation and its upscaling that is not mentioned nor analyzed, and single approaches from the literature are simply accepted without critical discussions and testing other alternatives.

More details on the two last general comments are provided in the detailed comments below.

Detailed comments:

P2 L15: Please clarify how % values link to atmospheric concentrations, eg is 0% or 100% in equilibrium with the atmosphere?

P2 L19-24. Are these explanations for the observed patterns the only options? Could patterns not also simply correspond to the relative input from anaerobic soil water (higher such input would correspond to lower $O_2$ and $No3^-$ but higher $NH_4^+$). Is the abstract the best place for explanations of correlations if there are several alternatives? Is it not better to simply report the correlations in the abstract and, unless reasons are very clear, leave the discussions on explanations to the discussion section?

P2 L32-33: As in the above comment, I wonder if the proposed direct link between $pCO_2$, $CH_4$ and DOC variability via organic matter processing is the only option. Can transport patterns including the balance between input and output be excluded? It seem reasonable that soil water bringing lots of DOC from surrounding soils could also bring lots of $CO_2$ and $CH_4$, and that this could explain correlations even if carbon processing in the river itself was not the reason, which is also supported by the following text in the manuscript. Therefore, I find this sentence emphasizing a link between $pCO_2$ and $CH_4$ and in-river DOC confusing and also potentially misleading and I suggest to remove it.



P5 L24-26: This sentence claim half of the global wetlands in the tropics and 40% in the northern temperate zone, leaving only 10% for the peatlands in the boreal and subarctic zones. This does not fit with e.g. N. C. Davidson A B D , E. Fluet-Chouinard C and C. M. Finlayson A. 2018. Global extent and distribution of wetlands: trends and issues. Marine and Freshwater Research 69(4) 620-627 https://doi.org/10.1071/MF17019., or with several other wetland extent studies. Please check and revise sentence.

P8 L6-14: Please give more details on the equilibrator system to prevent dependence on access to key method papers (not easy to access papers for all readers worldwide). What was the water residence time relative to the gas exchange equilibration time in the equilibrator?

P10 L12-13: Was linear models best for estimating methane oxidation? Sometimes exponential decay model can work better.

P11 L31: The used model was developed for closed systems. Why was not the open-steady state system model tried (e.g. Happell, J. D.; Chanton, J. P.; Showers, W. S. Geochim. Cosmochim. Acta 1994, 58, 4377-4388.)?

P14 L26. Please provide the equations used. Readers from all countries may not easily get access to other papers and can then not adopt or evaluate this study as should be possible.

P18 L2-18 and elsewhere: Some of the discussions on reasons for observed patterns seem to focus entirely on potential explanatory processes in the river channels, while alternative but not always mentioned explanations could relate more to the balance between different compounds entering the channel from the surrounding soils. Please check for possible alternative explanations to the observed patterns and report all relevant alternatives if not clear that a single explanation is most likely.

P29-34 Section 3.4. The mean concentrations found are very high – at least for $CH_4$. Mean values reported here are more than 2-fold higher than other recent estimates from the Congo basin by Upstill-Goddard et al 2017 (doi.org/10.5194/bg-14-2267-2017). Further, the extrapolated $CO_2$ emission is 251 TgC yr-1, i.e. 0.25 PgC per year which is more than 10% of the total global estimate of inland water C emissions (2.1 PgC yr-1; Raymond et al 2013 Nature). This value is also 3.3-fold higher than the total terrestrial NEE of the Congo basin (77 Tg C yr-1 as reported by the authors). Hence, given that not all this terrestrial NEE will be exported laterally to the river, degraded there and be emitted as $CO_2$, it means that wetlands in the basin not being a part of the terrestrial or the river network carbon balances, presumably would need to have an NEE of > 200-300 TgC or even more, and it is unclear how reasonable this. It would be good to discuss this and make a reliability check from a catchment carbon mass balance perspective.

Another concern with the extrapolated values are that a rigorous self-evaluation of the methods used regarding flux estimates seems to be missing. What are the main uncertainties? How large are the uncertainties? How robust are the area determinations? What possible area range is there? How about the k estimation? How reliable is that? There are different models to determine k in rivers. Was several of them tried? Do single k models really work across all stream/river orders? Were there any tidal influence that could have affected k in the most down-stream river sections? What ranges were derived? Were there any real in-situ flux measurement that can be used to validate the k estimates? If not, can k or flux estimates be validated in other ways? How about the observed concentrations? In rivers/streams it is often found that water concentrations are regulated by the emission rates so that the highest water concentrations are found were k values and fluxes are lowest, while concentrations are lower where fluxes are high which keep concentrations at low levels. How was this considered in the upscaling? Please add a careful method evaluation and examine and discuss the flux extrapolation critically.

Figure 2. Detailed maps with green borders to the right seem not correctly positioned on the central big map.



Figure 7. The outliers indicated as red dots do not seem to deviate so much from the other nearby dots. How much difference did removal of them make and is such a removal needed for some good reasons?

Tab S1-S3 in Spatial Analysis supplement: Is it realistic that the velocity was lowest in the highest slope low order streams? I realize this is the consequence of Equation S2 and that the river cross section dimensions change much more than the slope and drives this pattern… but is it realistic and was this validated? One could imagine a positive correlation between velocity and slope and that velocity-slope relationship eventually break down or change character for higher order rivers.

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