

# ***Interactive comment on “Variations of dissolved greenhouse gases (CO<sub>2</sub>, CH<sub>4</sub>, N<sub>2</sub>O) in the Congo River network overwhelmingly driven by fluvial-wetland connectivity” by Alberto V. Borges et al.***

## **Anonymous Referee #2**

Received and published: 9 April 2019

### General Comments:

This study examines geochemical dynamics, with a focus on greenhouse gases, in the Congo River. The manuscript presents an impressive amount of data and has unprecedented spatiotemporal coverage in a globally important, yet understudied river network. However, the amount of data presented makes the manuscript very hard to read. Further, the study’s main conclusion, and title of the paper, “Variations of dissolved greenhouse gases (CO<sub>2</sub>, CH<sub>4</sub>, N<sub>2</sub>O) in the Congo River network overwhelmingly driven by fluvial-wetland connectivity” is based on some major assumptions that

[Printer-friendly version](#)

[Discussion paper](#)



are not adequately addressed in the manuscript or data analyses. The data and discussion driving this conclusion is presented in essentially one paragraph buried in a 34 page manuscript. The manuscript has 21 Figures, many of which are difficult to read because of numerous panels and large ranges in axis scales, so it is difficult to evaluate the data.

I suggest that the authors significantly refocus the manuscript to tell a more concise story and perhaps consider separating this large dataset into several manuscripts. With the current data that is presented, a more appropriate title would be something like: “Geochemical dynamics in the Congo River.” A conclusive statement should not be made in the title, as there is little quantitative evidence for the conclusion that has been made other than some simple mass balances with considerable assumptions.

Specific Comments:

P5, L31: Much of the information in this paragraph is perhaps more suitable as a “site description” at the beginning of the methods section

P8, L13: Did the depth of the pump adequately prevent aeration while the ship was traveling at high speeds? Also, please describe how the underway data was post-processed to exclude erroneous data related to factors such as aeration, etc. It is also very difficult to evaluate the quality of data in the figures because of the amount of information displayed in each.

P11, L5: This methods section is poorly organized. Consider breaking into additional subsections. For example, flux calculations are mentioned here, then how  $k$  was calculated is not mentioned until the next section, which refers readers to the supplement for the actual details.

Methods: What statistical tests were used to evaluate the data? For example, throughout the results, the word “significantly” is used, and P20, L30 says “The  $p\text{CO}_2$  values were statistically higher. . .”

[Printer-friendly version](#)

[Discussion paper](#)



Results and discussion: Perhaps consider a separate results and discussion section. Considering the large amount of data presented, the discussion points get buried in the weeds.

P15: While this information provides interesting information about the Congo River, the volume of information distracts from the overall story about GHG cycling and makes the manuscript difficult to read.

P22, L30: Does the presence of high CH<sub>4</sub>:CO<sub>2</sub> ratios in the wetlands fit with the hypothesis that wetlands drive CO<sub>2</sub> emissions in the basin?

P23, L20: Was depth-integrated community respiration calculated? This is not mentioned in the methods. It would also be useful to indicate how much lower CR was from FCO<sub>2</sub> rather than simply saying it was lower. Further, only an average value was reported for CR. Please indicate the range of observed values and where these values were observed. This data is perhaps the most important contributor to the main conclusion of the manuscript, but is only described in a few sentences.

P23, L30: It is unclear why a shorter incubation time would alleviate the need to disturb the sample in any way. In biological sciences it is well-known that agitation significantly influences biological oxygen demand compared to stagnant conditions, and that microbial reaction kinetics occur on the time scale of seconds to minutes. For example, see the following studies:

Al-Homoud, A., M. Hondzo, and T. LaPara. 2007. Fluid dynamics impact on bacterial physiology: biochemical oxygen demand. *J. Environ. Eng.* 133: 226–236.

Coleman, M. E., M. L. Tamplin, J. G. Phillips, and B. S. Marmer. 2003. Influence of agitation, inoculum density, pH, and strain on the growth parameters of *Escherichia coli* O157: H7—relevance to risk assessment. *Int. J. Food Microbiol.* 83: 147–160.

Perhaps consider describing this methodological constraint as one factor leading to uncertainty in your conclusions rather than making an excuse and writing off the re-

[Printer-friendly version](#)[Discussion paper](#)

sults of the Richardson and Ward studies. The Ward et al., 2018 study showed that bottle effects are also a factor leading to underestimates, and that rotation velocity also influenced respiration in clearwater rivers with little suspended sediment load. This statement “Nevertheless, it seems unrealistic to envisage an under-estimation of CR by an order of magnitude that would allow reconciling the CR (and NCP) estimates with those of FCO<sub>2</sub>” does nothing to contribute to the advancement of aquatic sciences by ignoring efforts to improve mechanistic understanding and methodological biases.

P24, L5: The primary conclusion that the authors make, and the title of this paper are based on these several sentences rather than robust quantitative conclusions, and is buried in a 35 page manuscript. The title is not appropriate for this reason.

P34, L10: See previous comment. If this is the main conclusion that is made, the authors should focus much more detailed evaluation to this conclusion rather than diluting the manuscript with massive amounts of unrelated data.

P34, L25: What is meant by “. . .organic and inorganic CO<sub>2</sub> inputs?” Do you mean to say organic matter and CO<sub>2</sub> inputs from riparian wetlands.” By invoking inputs of wetland OM into rivers as an important pathway for CO<sub>2</sub> emissions, then you must also consider that river respiration of this organic matter drives a large fraction of river CO<sub>2</sub> degassing. When evaluating how much total CO<sub>2</sub> efflux from river channels is from inputs of CO<sub>2</sub> from wetlands to the river, the residence time of CO<sub>2</sub> must also be considered, i.e. how much respiration is needed to sustain high levels of CO<sub>2</sub> downstream of floodplain inputs?

---

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-68>, 2019.

Printer-friendly version

Discussion paper

