Anonymous Referee #2

Reviewer: 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 (CO2, CH4, N2O) in the Congo River network overwhelmingly driven by fluvial-wetland connectivity" is based on some major assumptions that 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.

Reply: We thank the reviewer for her/his comments on the previous version of text that stimulated us to clarify the presentation and discussion of our data.

Regarding comments on length of the paper and suggestion to rewrite the data-set into 3 separate shorter papers: Young scientists are pressured nowadays into producing as many papers as possible for the advancement of their career, to comply with the "publish or perish" model. They are further pressured to publish in "high-rank" journals that have very strict limitations in terms of word-counts and number of illustrations. This of course encourages slicing data-sets to produce as many as possible short papers, and in recent years this has become common practice. However, this should not forbid long papers presenting large and broad data-sets, unless there is a size limit imposed by the journal, which is not the case for *Biogeosciences*. The Associate Editors of BG are required to carefully evaluate the manuscript before they are published in the *Biogeosciences Discussion* forum, which is an indication that for our present submission the Associate Editor decided that the length of text and number of figures were acceptable for an article in *Biogeosciences*. Furthermore, the conciseness of papers does not guarantee quality, and conversely long papers are not automatically of poor quality. In fact, the contrary seems to be objectively true, longer papers tend to be more cited, we refer to Fox et al. (2016) and other similar studies cited therein.

In our particular case, we have gathered a large data-set on the biogeochemistry of the Congo basin that we think deserves to be presented in sufficient detail, since this is the first time such a comprehensive data-set was gathered for this relatively unexplored river basin. Our personal perception is that we present a self-contained and consistent "story" based on several variables that were chosen to help understanding the patterns of spatial and temporal variations of GHGs, and that for a reader interested in understanding our dataset, the full dataset should be discussed in its totality rather than in separate manuscripts.

To conclude, we feel that slicing the data-set into separate shorter papers might make it easier to digest but this does not outweigh the scientific merit of keeping the data in a single manuscript.

Regarding the title of the paper we agree to remove the "conclusive" element of the title, but want to avoid a more general title along the lines of "Geochemical dynamics in the Congo River" that would in our opinion lead to a loss of visibility and attractiveness of the paper. Dynamics of GHGs are the central topic of the paper, while other data gravitate around this central topic. We think that the primary readership of the paper will be specifically interested in GHGs (rather than geochemistry at large).

Specific Comments:

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

Reply: this paragraph provides information on the importance of C cycling in the Congo basin and at the same time the paragraph highlights the near lack of information/data. This message is introductory and helps to motivate and justify our research and goes beyond a simple "site description". As such we kept the paragraph in its original location.

Reviewer: 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 postprocessed 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.

Reply: We adjusted the depth of pump at the start of the cruise to avoid bubble entrapment (cavitation) and this was sufficient for the rest of the cruise. Our system is quite robust and rugged based on experience from more than 20 years of field deployments, and during the cruises the system was supervised quasi-constantly, so there was no need for a particular post-processing of the data. There were some occasional problems on rare occasions (clogging of pump or power shortage) that were identified rapidly and logged, and the data were processed (filtered out) accordingly. Should the reviewer want to evaluate the quality of the pCO_2 data, the comparison of discrete and continuous measurements of pCO_2 is given in Abril et al. (2015, doi: 10.5194/bg-12-67-2015).

Reviewer: 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.

Reply: We have added some sub-section titles and we have moved the text giving the parameterization of the gas transfer velocity.

Reviewer: 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 pCO2 values were statistically higher: : :"

Reply: Statements on statistical significance in text refer to figures, where the statistical significance is shown with symbols and the type of test is explained in the figure legend. We preferred to alleviate the main text from names of statistical tests and the coefficients of statistical significance. We nevertheless added the reference to corresponding figure to the main text each time the word "significant(ly)" was used. We also added a few words on the statistical methods in the Methods section.

Reviewer: 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.

Reply: While elaborating the manuscript we spent a lot of time contemplating the best way to articulate the text, and we concluded that a joint results and discussion section was the best option since it allows the "story" to flow from the most descriptive aspects (spatial variations) to the most integrative aspects (discussion of fluxes at the scale of the basin) to end with the most conceptual aspects (implications of the comparison with terrestrial NEE). Also, separating results and discussion sections would further increase the length of paper (need to add text to recall results in the Discussion). Hence, rather than splitting Results and Discussion, we have addressed the underlying concern by adding several new sub-section headings, and a paragraph at the start of the results-discussion section that explains the "road-map" of the paper. This should help the readers to pick directly the main points they're interested in.

Reviewer: 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.

Reply: Virtually no information is easily available in the literature on the variations of basic limnological variables in the Congo River, so we think it is important to present these data, which we admit makes the section a bit descriptive. Whether or not this information is a distraction is fairly subjective, and some readers might be extremely interested by more basic limnological information such as pH or conductivity or more advanced variables such as $\delta^{18}O$-H_2O.

Reviewer: P22, L30: Does the presence of high CH4:CO2 ratios in the wetlands fit with the hypothesis that wetlands drive CO2 emissions in the basin?

Reply: We do not see a contradiction here. The high CH_4:CO_2 ratio means that CH_4 is relatively higher than CO_2, it does not necessarily mean that CO_2 is lower in absolute terms. Figure 12 shows that CO_2 is higher in rivers draining the Cuvette Centrale Congolaise (CCC), so it does fit with the hypothesis that wetlands drive CO_2 emissions in the basin. This means that both CO2 and CH4 increase in the CCC, but the increase of CH4 is relatively more important.

Reviewer: P23, L20: Was depth-integrated community respiration calculated? This is not mentioned in the methods.

Reply: We added to the methods the following sentence: "Depth integration was made by multiplying the CR in surface waters by the depth measured at the station with a portable depth meter (Plastimo Echotest-II)."

Reviewer: It would also be useful to indicate how much lower CR was from FCO2 rather than simply saying it was lower.

Reply: This information was already given in the original submission P23L22-24.

Reviewer: 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.

Reply: We have added the range of CR values to the text. Since the reviewer acknowledges the CR data are (the most) important, we further analyzed these data. We found no correlation with most variables expected to have some explanatory power (TSM, POC, Chl-a) but we found a relation with DOC that is now included in the manuscript. We also discuss a decreasing relation between the FCO_2:CR ratio and stream order. We added the following text: "The $F$$CO_2$:CR ratio was higher in lower order streams than higher order streams, with median values ranging between 21 and 139 in stream orders 2-5 and between 3 and 17 in stream orders 6-10 (Fig. 16). This indicates a prevalence of lateral CO_2 inputs either from soil-water or riparian wetlands in sustaining $F$$CO_2$ in lower order streams than higher order streams where in-stream CO_2 production from net heterotrophy is more important. These patterns are in general agreement with the conceptual frame developed by Hotchkiss et al. (2015), although lateral CO_2 inputs were exclusively attributed by these authors to soil-water or ground-water inputs and riparian wetlands were not considered. These patterns are also in agreement with the results reported by Ward et al. (2018) who show that in large high-order rivers of the lower Amazon, in-stream production of CO_2 from respiration is sufficient to sustain CO_2 emissions to the atmosphere."

Reviewer: 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: H7âˇAˇTrelevance 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 results 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 FCO2" does nothing to contribute to the advancement of aquatic sciences by ignoring efforts to improve mechanistic understanding and methodological biases.

Reply: We have the impression the reviewer misinterpreted our discussion. We do not have the intention to dismiss the work of Ward and of Richardson, on the contrary, we mentioned upfront that our CR measurements might be under-estimated based on these publications. We have rephrased the sentence as: "We acknowledge that our CR measurements might be under-estimated due to bottle effects and lack of rotation, 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_2." We stand by this statement. It is unlikely that our measurements are 10 times too low, when the experiments of Ward and Richardson show at most an underestimation of a factor of 2. We have removed the statement regarding the incubation time that was initially motivated by the Richardson study for which the experiments were 40 days long, and major differences are only apparent in data from 15 days onwards, from the inspection of their Figure 2. We cannot check the effect of TSM on respiration since Ward et al. (2018) did not explicitly report TSM data at their sites, but it is now well established that the Congo is much less turbid than the Amazon (e.g. Descy et al. 2018) and that effect of rotation on respiration should be linked to the presence of particles. Accordingly, the median of TSM in our data-set (14 mg/L) is more than 2 times lower than at the sites studied by Ward et al. (2018), as reported by Ward et al. (2015).

Reviewer: 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.

Reply: Our conclusions are built on several lines evidences gathered from a combination of metabolic measurements, stable carbon isotope ratios of DIC, patterns in the spatial variations of CO2, and the comparison between CO2 emissions and terrestrial NEE.. Note also that the primary conclusions of our work are highlighted in the "Abstract" and "Conclusion" so we disagree with the reviewer's comment that they are buried in the text. Nevertheless, we now added several new sub-section headings and added a paragraph at the start of the results-discussion section that explains the "road-map" of the paper. This should help the readers to pick directly the main points they might be interested in.

Reviewer: 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.

Reply: See previous replies regarding subjective appreciations on the length of papers. We do not see how the other data would be unrelated.

Reviewer: P34, L25: What is meant by ": : :organic and inorganic CO2 inputs?" Do you mean to say organic matter and CO2 inputs from riparian wetlands." By invoking inputs of wetland OM into rivers as an important pathway for CO2 emissions, then you must also consider that river respiration of this organic matter drives a large fraction of river CO2 degassing. When evaluating how much total CO2 efflux from river channels is from inputs of CO2 from wetlands to the river, the residence time of CO2 must also be considered, i.e. how much respiration is needed to sustain high levels of CO2 downstream of floodplain inputs?

Reply: Yes, we mean OM inputs from wetlands, we have clarified this in the revised version ("by organic matter inputs as well as direct CO2 inputs"). However, the fact that organic inputs contribute does not necessarily make these to drive a large fraction of river degassing – our comparison of CR and FCO2 addresses this point adequately, we feel. Residence time needs to be accounted when making detailed budgets at small scales, for instance, over a given short stretch of a river. We do not have the data to make such budgeting exercises. We looked at riverine and terrestrial CO2 fluxes integrated at the scale of the entire basin, for which the residence time of water is irrelevant.

References used (not cited in manuscript)

Fox, C.W., Paine, C. E. T., and Sauterey B.: Citations increase with manuscript length, author number, and references cited in ecology journals, Ecology and Evolution, 6, 7717–7726, doi:10.1002/ece3.2505, 2016