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Interactive comment

Interactive comment on "The influence of reservoir traits on carbon dioxide emissions in the Belo Monte hydropower complex, Xingu River, Amazon – Brazil" *by* Kleiton R. Araújo et al.

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Dear referee 1, We would like to thank you for the detailed comments, they were very constructive. Please find our answers below after each referee comment.

Referee 1: This paper is about the CO2 concentration and emissions from a newly created hydroelectric reservoir complex in the Amazon area. Given that particularly Amazonian reservoirs have been pointed out as high emitters of greenhouse gases, and since emissions typically are higher the first years after flooding, this study is certainly valuable and interesting. In particular since the new reservoir is a run-of-the-river type, which is supposed to result in lower emissions than storage reservoirs. The study

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seems to be well-conducted, based on standard methods. However, the presentation severely lacks focus and clarity. I will give in the following a few idea on how the paper can be improved, but I really want to urge the senior authors of this paper to support and help the first author, who is apparently a MSc student and writes his/her first paper (it says in the Acknowledgements). It also takes a thorough revision of English language use and style.

Response: Thank you, we hope to help clarify the role of run-of-the-river dams on CO2 emissions, particularly in the Amazon. We have modified the manuscript based on your suggestions and all authors have carefully reviewed the manuscript for style and clarity.

Referee 1:What makes this study interesting is that it studies the Belo Monte hydroelectric complex, a all-new installation in the Amazon (it's not even up at full capacity yet), the biggest in the Amazon so far, and one of the biggest in the world, and one that was heavily disputed and criticized. This is not mentioned at all in the paper! I could imagine that the story could be built around the case of this new and huge installation. New reservoirs typically have elevated emissions, but here apparently biomass was removed before flooding, at least partially. Is this visible in the data? One of the reservoirs is run-of-the-river, does it really have lower emission than the storage reservoir? These questions could be formulated as hypotheses, addressed with the data (i.e. figures should illustrate data in a way that relates to these hypotheses), and then explicitly answered in the Discussion. This would give the study a much-needed 'read thread'.

Response: We agree that we did not convey the controversy surrounding the Belo Monte hydropower operations in the original manuscript. We have now added a brief discussion of this topic in the Introduction. The Belo Monte hydroelectric complex is the largest hydroelectric in power capacity (11,233 MW) in the Amazon, but not the largest regarding area of reservoir. Among all the new constructed or planned dams in the Amazon, Belo Monte is in fact the most efficient in terms of energy production per km2 of reservoir (see Faria et al 2015). Despite complete forest removal, plant-derived material still remained in the Intermediate Reservoir (IR). In the Xingu Reservoir (XR), forest removal were done only in some large islands. However, 42 % of the area of this reservoir represents the previous river channel where the riverbed consisted of bedrock and sand. Therefore, lower emissions were expected for the XR in comparison with the IR. Nevertheless, our results show higher CO2 fluxes in the IR only during the low water season. A possible explanation for the lack of difference in CO2 fluxes among the reservoirs at the high water season could be related to the shorter residence time, the primary productivity not necessarily heterogeneous and an influence of algal bloom in the IR. This was perhaps unclear in the original manuscript, which we have improved in the revised manuscript, including the addition of the hypotheses and answers as suggested. Thank you for this useful feedback.

Referee 1: It will take a thorough rewriting of the manuscript before it may become acceptable, but since it seems to be good data from a understudied site of high interest, I think in the end this could become a valuable addition to Biogeosciences.

Response: Thank you for your comments, they've helped to shape a stronger manuscript. We have worked hard to improve the manuscript quality and hope to contribute to the knowledge around tropical run-of-the-river reservoirs.

Detailed comments: Title: the influence of reservoir traits is not explored to any greater depth. Which traits? I'd suggest to change the title accordingly, maybe "CO2 concentrations and emission in the newly constructed Belo Monte hydropower complex in the Xingu River, Amazonia".

Response: We intended to use the word "traits" in the title to describe our comparison of storage and run-of-the-river reservoir types. However, we agree that this title was a bit unclear. We have directed the hypothesis and discussion to better explore and clarify our title.

L41. The inland water area number seems wrong. See Verpoorter et al. 2014 GRL

Response: The inland water area value number is related only to rivers and streams (i.e., not including lakes and wetlands) based on Raymond et al. 2013. We updated that information with the lake surface area estimated by Downing et al. 2006 and Verpoorter et al.2014. In addition, fluxes information was corrected and influx data was added based on Drake et al. 2018.

L42. Only the Raymond study gives a global estimate, the other citations are regional scale.

Response: The other citations were removed from this sentence to adopt only the global estimate of Raymond et al. 2013.

L45-54. There's a lot of detail here that is not addressed by this study and could be removed here, e.g. microbial community structure or priming.

Response: The goal of this paragraph was to address the factors involved in CO2 production. We agree that this section was perhaps too detailed. We have modified this section to be more brief and concise in the revised manuscript.

L70. While emission are typically high, the lifetime emission of a reservoir is probably rather a function of the long-term emission level, and the short initial emission pulse may have less influence.

Response: We have modified this sentence to point out that emissions during the initial years are typically highest and the most uncertain, but that sustained long-term emission rates are likely important with respect to the overall carbon balance of the system over its lifetime.

Study Area: This must mention that the installation is new, and it must describe in how far and where vegetation was removed before flooding, and when the flooding took place.

Response: Thank you, we have made this change.

L106 and 114. The water retention times are very short, even for the storage reservoir it's only 1.5 days. Are these numbers correct? If so, these reservoirs, given their size, must be characterized by quite strong water flow, and thus the gas exchange velocity is probably hardly related to wind speed, but rather to water speed.

Response: Our residence time (RT) calculations were based on Faria et al. (2015) and environmental impact study (EIA) published by Norte Energia (Eletrobrás, 2009b). Since RT was underestimated we made new calculations based on Water Agency of Brazil (ANA) discharge data at the Altamira station. The corrected RT was 20.2 and 3.4 days for IR and XR, respectively, using the average discharge historic series to whole year. We have corrected the RT in the manuscript.

L112. 97% of the capacity are at the Belo Monte dam, so the ROR dam only produces 3% of the energy even though it contains one third of the number of turbines?

Response: The difference among both dams is not only in size and turbine number. The turbine model also differs between dams, which influences the generating power. The main power house is equipped with 18 turbines Francis type with active unit power of 611.11 MW. As complementary powerhouse has 6 Bulb type turbines that are considerably less potent with active unit power of only 38.85 MW.

L116. Where is the hypolimnion typically starting? Did you do any depth profiles of T and/or DO? If so, please show and report! If not, please cite a study that states that the thermocline is typically at >20 m.

Response: During our samplings the water column had a well-mixed pattern without variation in DO in most of the reservoir's area. The hypolimnion was only apparent in the IR, close to the dam, where DO decreased drastically at approximately 50 m from a total depth of 58 m. However as observed on Faria et al (2015) its formation is not expected on Belo Monte Reservoirs. Therefore this sentence was altered and hypolimnion information was withdrawn. We have now included depth profiles of variables such as DO, temperature, etc. in the supplementary material.

Section 2.2. It would be more easy to understand if you first described your sampling campaigns, and then tell about any gaps.

Response: We have made this change

L144. Why was 60% of water depth chosen? Seems arbitrary. Also, it would be good to know the actual depth at these sites. A raw data table should be submitted alongside with the paper.

Response: 60% depth was chosen as a mid-depth sampling point to compare surface and bottom waters. In deeper sites the three depths (surface, 60% and near-bottom) were sampled due the variation in water velocity. Our goal was to sample depths with different organic and inorganic matter due water flow transport. We have added depth information to Table 1.

L150. How good was the evacuation? In my experience, it's very difficult to get a good vaccum, but probably 10% or more atmosphere will remain, which may dilute or contaminate your samples. Was this checked?

Response: We are confident in our sample storage methods, which our team has extensive experience with. A vacuum pump was used to create a vacuum, which was confirmed since the volume of gas pulled from the syringe into the vial was similar to the vial volume without the needing to manually depress the syringe's plunger. We have not added these details to the manuscript, as transferring gas to vials is a common method.

L154. Start this paragraph with saying "Diffusive CO2 emission was measured with floating chambers". Also, please give the dimensions, shape and type (transparent / opaque) of the chamber.

Response: We have made this change. Two different chambers were used to measure gaseous CO2 emissions during whole sampling campaigns. Both chambers were round and opaque covered with reflexive aluminum tape, differing only on the dimensions (high water chamber: capacity - 7.7 L; area - 0.08 m2; height – 11.7 cm/ low water chambers: capacity - 6 L; area - 0.07 m2; height – 10.5 cm).

L161. I guess you mean logging frequency, not time.

Response: Exactly, we have modified this.

L168. Atmospheric pCO_2 of 380 ppm seems like an outdated value, or are these your own measurements in air?

Response: We agree that this is an outdated value and we have re-checked our database and changed the text. For the new version, measurements were discarded when the R^2 of the linear relation between pCO_2 and time ($\delta pCO_2/\delta t$) were lower than 0.90 ($R^2 < 0.90$) or in cases where we measured negative FCO_2 when the surface water pCO_2 was higher than the atmospheric pCO_2 based on measurement done at the same site. However, this happened only two times and could be attributed to some source of CO_2 contamination when placing the chamber into the water. Thus starting with a higher pCO_2 than the water.

L184. This sentence seems unnecessary

Response: We have removed it.

L191. A station is stationary. You probably mean a handheld meter or device?

Response: Updated as suggested.

2.5. Statistics. I did not know Permanova, so this should be better explained. Is it a parametric method? Because it is stated that the data did not follow normal distribution. However, later in this paragraph, you mention some data were normally distributed and used t-test; this is confusing. Also, in the entire paper, report the actual p values, not just if p is lower of higher than 0.05.

Response: Agreed, the method was superficially mentioned in the manuscript. PERMANOVA is a multivariate variance analysis to compare variability between and within groups using permutation to obtain p-value. Due the different hypothesis tested the data set had to be adjusted and consequently altered the data distribution. In the case of T-Test, sites located on “outside reservoirs” and “downstream of the dams” were not considered and also season. Related to p value, we have now reported all the p-values accordingly to the real value obtained from the statistical test. PERMANOVA analysis was better detailed in the methods section as suggested. We have removed T-Test analysis since it is related to a descriptive result.

Results. In general, this section describes many findings and patterns, but it does so in a quite unstructured way, and is therefore difficult to follow. I really think it would help this paper if only the results were presented that are relevant to the hypotheses or research questions. Also, the language describing the patterns should be improved. For example, it needs to explained what numbers are given (e.g. L208, is this the mean $\pm$ standard deviation, or something else?), and comparisons between two groups describe a difference and not a variation (L208). Also, increase and decrease (e.g. L245 and L249) refer to a change over time and thus some form of time series data, while this study has data for two discrete sampling occasions, and thus can only speak about differences. It should also always be very clear what exactly was compared. For example, in L213, it was unclear what was tested here, the variability in pCO_2 within and environment, or between environments?

Response: Thank you, your comments were very constructive especially to this section. In line L208 and throughout the whole text we presented values as mean $\pm$ 1 standard deviation and indeed we were using the term “variation” when we were meaning “difference”. Some of the comparisons were unclear due the writing style and language, but in the line L213 tested the pCO_2 variability between environments. We have restructured this section as suggested and paid extra attention to the language.

Again concerning statistics, it is unclear to me how a comparison between two groups can render a R^2 value, but maybe that’s a part of the PERMANOVA, and should in that case be better explained in the Methods.

Response: Our statistics description did not detail PERMANOVA properly in the previous version of the manuscript, as so this test became unclear to the reader. PERMANOVA analysis tests similarity using a Euclidian distance index through permutations. The R2 value is generated by permutations. As mentioned above, PERMANOVA analysis was better explained in the methods section as suggested.

L215. Here you speak about spatial variability, but do you mean differences of means between different environemnts, or the variability of measurements within one environment type?

Response: Here the test is to evaluate if the different environments (reservoirs, downstream the dam and outside the reservoir) presented different fluxes in each season. Temporal trends sometimes may mask some spatial patterns that only become visible when seasons are treated separately. Therefore, here we refer to a PERMANOVA test similar to the one mentioned on L213, comparing pCO2 between environments.

L219. “Outside reservoir areas” is not a very illuminating term. Could choose another name?

Response: We agreed and replaced it to “unaffected river channel”.

L224. 281 μatm at 60% depth, how much is that in meters? And how can deep water be undersaturated in oxygen? Typically it is oversaturated. Or was this above a macrophyte bed?

Response: The total depth of this site is 7.5 m (Table 1), the sampling depth was 4 m. This sentence describes pCO2, not dissolved oxygen. The value of pCO2 equal to 281 uatm was observed in the undisturbed river channel with significant current and no macrophyte bed. This value corresponds to undersaturated pCO2 with respect to the atmosphere, and therefore likely oxygen levels above atmospheric saturation, indicating net primary production. We are not sure what you mean by the question of how deep water can be undersaturated in oxygen. It is quite common for dissolved oxygen levels in river water to be below atmospheric in the case of net heterotrophy.

L231. Here it says the the data from the two seasons were pooled, but L237-241, the seasonal data are discussed separately. This is confusing.

Response: Thank you for this comment. Our FCO2 data is related to a time period of two years, comprehending three seasons (2016 high water, 2017 high water and 2017 low water)(L124). The data pooled are from the same season, both high water, sampled with the same equipment and they were not statistically similar (L228). High and low water were measured with different equipment due technical issues and treated separately (L154 and L158).

L246. The seasonal difference in IR was very small, certainly not a "pronounced difference". Interestingly, FCO2 was very different between seasons in spite of similar pCO2, which indicates a strong variability in k. Was this the case?

Response: Very true, related to k, no statistically significant variation was observed between seasons (L273 – L275). We have removed the word "pronounced" and updated this line as suggested.

L250. What kind of spatial analyses? Comparison of the means for different environments?

Response: PERMANOVA was used to compare simultaneously the variation of FCO2, pCO2 and k600 between both reservoirs. This analysis did not generate difference of means, but the dissimilarity within versus and between groups through distance measures.

L251. "evaluated together", is this warranted? Were these two groups similar?

Response: Good point. Yes, our results indicate that they are similar. We have checked it by changing river channel category. Only flooded areas represented the reservoir emission, nevertheless, same results were reached. In case of dissimilar data a different classifying reveals overlapped patterns, which was not the case of river channel and flooded areas.

L256. “Pasture” is a new and undefined category.

Response: Upland forest and pasture were the main land cover in the areas flooded by the reservoirs as described in the description of the study area and measured sites (Table 1). They are not a new category and may be classified as a flooded area subgroup.

L262. What’s the measure of variability? It seems that in this study, you mostly compared means, but if you want to address the variability, you maybe want to look at relative standard deviations, interquartile ranges or something similar. If you want to stick to comparing means between environments, please formulate this explicitly in the text.

Response: There was some confusion with the term from our part. Our analysis describes difference by a distance matrix that calculates the similarity within and between groups, not variation. We assume that the poor statistics section may have complicated much of the reading. We have rewritten that section and replaced “variation” by “difference” in the whole manuscript.

L263. Varied significantly between what?

Response: The FCO2 differed significantly between XR and IR reservoirs during the low water season. This sentence has been modified accordingly in the revised manuscript.

L266. The 90 km downstream site is so far away it’s not even on the map. I wonder in how far it is relevant to this study at all, or could safely be omitted.

Response: Thank you for the observation. That was a mistake in the writing; the 90 km site is downstream of the Pimental dam (P20 site – Fig. 2), not Belo Monte. This site is relevant because of its location downstream of the Volta Grande do Xingu (Xingu Great Bend) region and a few kilometers upstream to where the Belo Monte dam discharge back into the original river. This information was properly corrected.

L270-273. Go straight to the results instead of first describing what was not done.

Response: We have made this change.

L275. The relationship between k600 and wind speed is very weak. At any wind speed, k can vary with a factor of 2-4. This is quite often the case, and maybe even expected in such system where water moves fast, and thus water turbulence is quite independent of wind speed.

Response: Thank you for this comment. We agree, particularly considering the residence time of the Belo Monte system. Since there was no significant k variability, the water turbulence must be the major factor driving CO_2 diffusion.

All in all, the Results give many comparisons, What about making matrix tables where you can give test statistics for each comparison?

Response: Thank you that was a great suggestion. We have added such a table.

3.3. Did you ever measure depth profiles? Would be very interesting to show these data, to asses if really the turbine intake is in the epilimnion, and to assess the potential outgassing through turbine passage.

Response: Yes, depth profiles were made for temperature, pH, O_2 and conductivity. CO_2 was measured at the bottom, 60% of site depth and at the surface (0.3 m) during high water campaigns. Probably, there was no hypolimnion close to the Pimental dam due the water column uniform oxygenation. Nevertheless, its intake is on the bottom, where even with high O_2, the pCO_2 is higher than on surface. In Belo Monte dam pCO_2 follows the same pattern, although the O_2 decrease drastically at approximately 50 m (as mentioned above). As so, Belo Monte intake is in the O_2 rich zone. We have created a supplementary material file and the depth profiles were added to it.

L292. This is not one of your results.

Response: Removed.

L296. The Discussion should start with your most important finding, not with citing other studies.

Response: Updated as suggested.

L303. This seems to be an important finding. Could you make a figure that illustrates this finding, to make it visible and convincing?

Response: Thank you for the suggestion. However, we chose to keep this comparison on table 2.

L309-326. This discussion is very hypothetical and not much related to your data.

Response: Thank you for this comment. We have deleted this paragraph.

L327. Not really. In your own data, there is an example of differences in k producting very different emission fluxes in spite of similar pCO2 (see my comment above).

Response: We have added the clarifying statement "...although we did observe some specific examples of differences in k producing different emission fluxes even when pCO2 was similar"

L328-334. This may be the main message of this paper. It would be good if you produced a Figure that illustrates this finding.

Response: Thank you, we agree and have added such a figure.

L340-341. The Methods need to describe explicitly which areas were flooded with intact biomass, or after biomass harvesting.

Response: We agreed. This information was already in table 1, however it was not as explicit as it should. The text was updated with the information of suppression area for each reservoir.

L350. Could you actually observe increased water clarity in your data / samplings? If not, this discussion is not helpful to explain your data.

Response: No. Our turbidity data was not reliable due to poor calibration, as such we removed it from the paper. Updated as suggested.

L355. pCO2 were only lower during low water compared to high water in the downstream and dam categories. For flooded and river channel, they were similar (Fig.3). So it is not warranted to speak about a “drastic decrease”.

Response: We have deleted the word drastic from the text. The statistical test showed difference among seasons and to environment categories, which is corroborated by the lower pCO2 averages during low water both to flooded areas and river channel (as shown in table 2). This was corrected in the new version of the manuscript.

L375-383. Could the difference between Belo Monte and Petit Saut be explained by different water intake depths? Do you have water profile data?

Response: Very good question. As mentioned above we have DO depth profiles that show signs of anoxia near the bottom in the IR in the site closest to the dam. The hypolimnion is located under the input zone of Belo Monte turbines, and according to Kemenes et al. (2011 and 2016) and Abril et al. (2005), the hypolimnetic waters may increase the downstream emissions. Therefore, possibly the intake depth plays an important role in the different downstream emissions between Petit Saut and Belo Monte complex. This information was updated in the manuscript.

L391. It seems not warranted to assume that any site or time point should serve as a “reference” for river pCO2, since it varies in time and space.

Response: We have deleted this sentence.

L395. What is meant by “turbine activity”?

Response: With the term “Turbine activity” we meant periods when turbines are either working or stopped for maintenance. Since the sampling campaigns occurred during the installation, few of the operational turbines were actually working. This was perhaps reflected by downstream fluxes during the low water period, which resembled areas without turbine influence.

L398-406. I think you could further explore the patterns in k, e.g. between environments, and between reservoirs. Were the values in these reservoirs rather similar to other reservoirs or lakes, or rather to rivers?

Response: Thank you, very good point. The XR k600 values (22.99 $\pm$ 8.00 and 22.89 $\pm$ 21.40 cm h-1 on high and low water, respectively) were in the range of the Furnas reservoir (19.58 $\pm$ 2.5 cm h-1) located in Amazonia (Paranaíba et al., 2018). To IR (7.13 $\pm$ 1.59 and 60.80 $\pm$ 18.02 cm h-1 on high and low water, respectively) the high water k600 was similar to Javaes river (8.22 $\pm$ 3.80). Therefore, the average XR k600 was rather similar to reservoir, otherwise average IR k600 rather to river. Updated as suggested.

L403. There is no strong positive correlation between wind speed and FCO2 in your data. Fig 5 shows weak relationships, at best.

Response: Thank you for the highlight, we have updated the text as suggested.

L423-425. This sounds like the main result of this study. Make a figure to show and highlight this result, and discuss it in terms of reservoir properties and operation type.

Response: We have included new categories on figure 3 according reservoirs that show this difference and the suggested points were included in the discussion.

Figure 3. In panels c and d, I would suggest you order the environments in flow direction. That is, upstream first then XR environments, then IR environemnts, then downstream. If it gets too crowded, make two separate panels for high and low water. And the same for pCO2 and FCO2 and k600, i.e. you may end up in 6 panels instead of 2. Together with panels a and b, it would be 8 panels.

Response: We have reordered and changed the categories. We added the cate-

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gories in the following order: "unaffected river upstream", "XR", "IR", "downstream of the dams" and "unaffected river downstream". Panels were also separated by season.

Figure 4. When seeing this figure, I wonder how much of this spatial variability is driven by differences in pCO_2, and how much by differences in k.

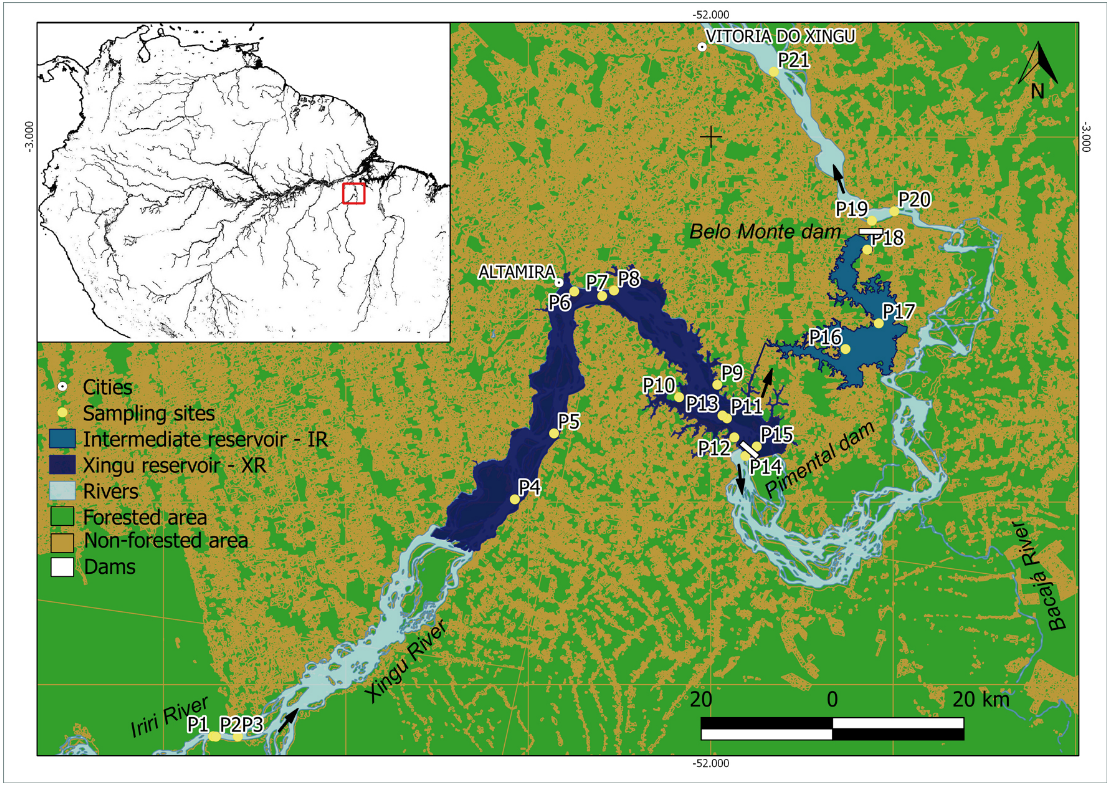
Response: To make the spatial variability more visible we have added one more panel related to k600 to figure 3.

Table 2. What are the values, mean $\pm$ standard deviation? How many measurements are behind each of these averages? Could you introduce a column with "n"? The k values are high and resemble rather riverine systems than lakes or reservoirs, I guess an effect of the fast water flow. The comparison with literature values would be better and more visible in a graph than in a table.

Response: Whole values are related to averages $\pm$ standard deviation, with the exception of Sawakuchi et al. 2017. During high water FCO_2 was measured three times (L157), and during low water two FCO_2 measurements were made simultaneously (L 160) and headspace was sampled on triplicates (L144). It is probable that the turbulence in both reservoirs is mostly related to water flow. We chose to keep literature values in table 2 and the new column was added as suggested.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-53/bg-2019-53-AC1-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-53, 2019.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VITORIA DO XINGU
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Belo Monte dam
P18
ALTAMIRA
P6
P7
P8
P17
P16
P9
P10
P13
P11
P15
P12
P14
Pimental dam
P5
P4
Cities
Sampling sites
Intermediate reservoir - IR
Xingu reservoir - XR
Rivers
Forested area
Non-forested area
Dams
Bacajá River
Xingu River
Iriri River
P1
P2P3
20
0
20 km

Fig. 1. Fig.2

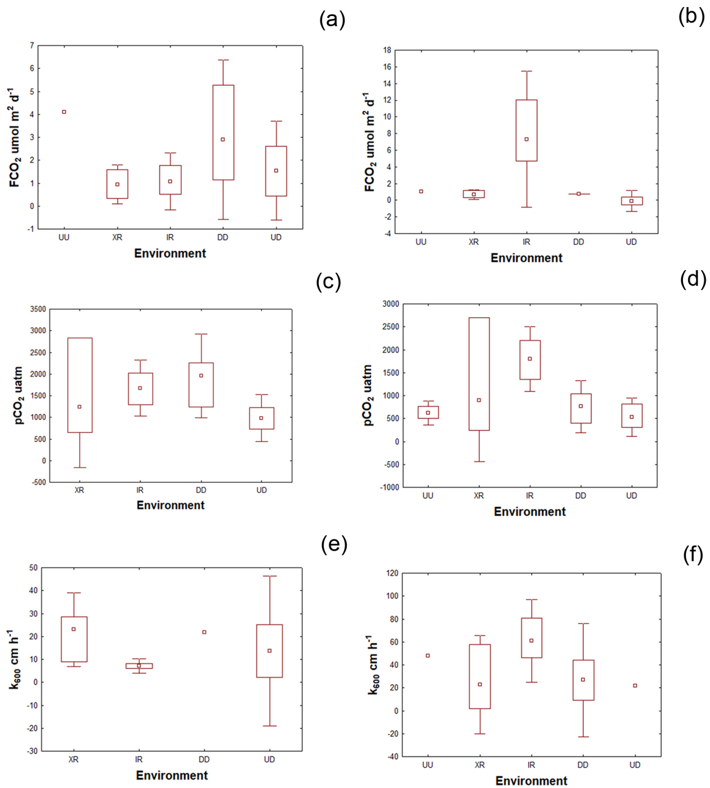
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Fig. 2. Fig.3

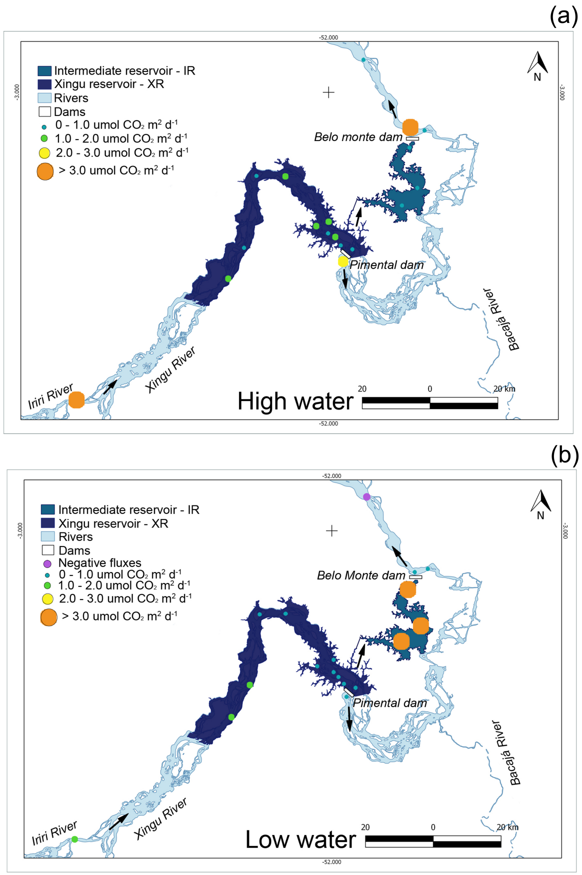

Fig. 3. Fig.4

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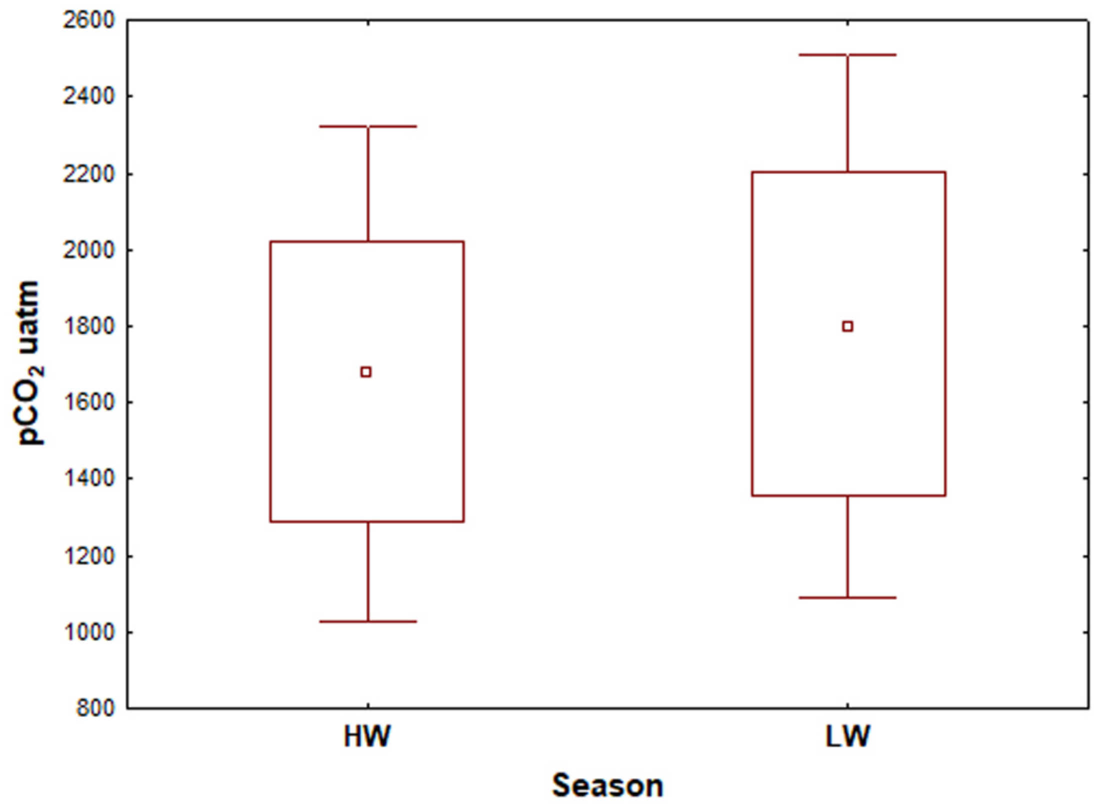

Fig. 4. Comment L303 suggestion: pCO2 of IR reservoir between seasons. HW is related to high water and LW to low water.

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