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# *Interactive comment on* "The role of hydrodynamic and biogeochemistry on $CO_2$ flux and $pCO_2$ at the Amazon River mouth" *by* Diani F. S. Less et al.

**Anonymous Referee #2**

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Less et al. presented their work (carried out from 2010 to 2016) on CO2 flux and pCO2 (water) as dependant variable against numerous biogeochemical, hydrodynamic and meteorological parameters from the Amazon River.

My foremost concern is regarding study design of the work. The work is like gathering of the data without any specific objectives, hypothesis or main story which could distinguish the work from other studies from the same region and / or contribute to knowledge gaps of the previous studies done till date. Also, there is no clear indication that how this knowledge can be upscaled, generalised or utilised from broader aspect in other parts of the world. Hence, the study is lacking novelty and in the present form it is having an impression of a mere descriptive case study.

I am also under the impression that the authors did not do justice with the theoretical meaning of data measured / estimated by themselves, and derived wrong conclusion based on apparent statistical relationship (e.g., high correlation coefficient) and sometimes based on spurious correlations. For example, the authors argued that the water temperature affected the k600 magnitudes based on the correlation coefficient. But k600 should be independent of water temperature because the parameter was normalized using Schmidt number.

Of course, the statistical analysis of pCO2(water) and air-water CO2 flux is important. Such analysis enables to draw simpler conclusion which is more useful. However, this work does not reach the minimum level due to the lack of theoretical understanding, hence mostly speculative.

I also have serious concern about the methodology of the study. Nothing has been stated about the uncertainty / precision / accuracy of any data of the respective parameters measured or estimated. Similarly, nothing has been stated about the calibration of any instrument.

The advantage of the floating chamber method of CO2 flux measurement is that unlike bulk formula method it provides a direct measurement of the flux. However, the disadvantage of the method is that under highly turbulent conditions the measurements are problematic because bubbles enter the chamber, or the chamber sometimes flips upside down. The data of current velocity presented in this study showed it was quite high during all the seasons. Did the authors take any special care for this problem? I think, the data taken during high current velocity or high turbulent condition should be filtered while using floating chamber method. Authors neither tried to figure out such uncertainty in fluxes when the current velocity was too high, nor they tried to analyze the relationship between the measured flux (by floating chamber) and the computed flux (by wind parameterization).

In the abstract, authors stated “FCO2 and pCO2 were used as dependent variables and analysed against 33 biogeochemical, hydrodynamic and meteorological parameters along the hydrological seasons" (P1 L16-18). This sentence is misleading as my understanding is authors measured / estimated total 33 parameters which includes pCO2 and FCO2 also.

Under the section "Methods" and subsection "Study area and sampling procedures", authors cited Table 1, 2 and S1 (P3 L8), while telling about sampling location. Again, authors cited only Table 1 (P3 L21) while telling about all biogeochemical, thermodynamic and kinetic variables! Both are not the proper place (under section 'Methods') for citing tables which display results.

I counted the number of all parameters displayed in Table 1, 2 and S1. Though it counts total 33 but the parameter, fine suspended sediment concentration (FSS) displayed twice in Table 2 and S1 with the same values! Does that imply that the authors measured / estimated actually 32 variables in total? This is quite ambiguous because of these disparities between what the authors wanted to do and what they actually did!

Authors measured / estimated many variables amongst which some are not at all directly or indirectly related to pCO2 or FCO2 (for example DON), but authors didn't try to measure (or even estimate) directly related dissolved inorganic carbon (DIC) and total alkalinity (TAlk) as we know the DIC / TAlk ratio is very much important to determine pCO2 and hence FCO2. The pattern of relationship between excess DIC and apparent oxygen utilisation (AOU) can provide useful insights about lack of correlation between pCO2 (and / or FCO2) and respiration along with anaerobic vs aerobic microbial activity within the system (P8 L1 to 2).

The statement regarding lack of correlation between U10 and FCO2 and its explanation (L11, P10) should not be a part of Discussion. Rather, this sentence is a clear indication of lacking proper study design because wind speed and U10 are one of the most important parameters regarding the discussion about air-water CO2 flux.

In the conclusion, authors stated that dissolved organic matter, water and air temperature as suitable predictors for estimating pCO2 and FCO2. The basis of this conclusion is that, these factors 'coincides' and 'correlates' with pCO2 and / or FCO2. However, this is not clear whether these variables are causative factors or not. This is an established fact that the water temperature will affect the solubility of gas which in turn would affect pCO2 and FCO2. This is also expected that air temperature correlates with water temperature. Hence, nothing is exciting in this conclusion. Also, stating DOC as one of the main predictors (based on correlation) for pCO2 and FCO2 without measuring or estimating DIC concentration is most unlikely, because there shouldn't be any direct relation of DOC with pCO2 and FCO2 until and unless the DOC is being converted to DIC. On the whole I come to infer that there is no take-home message for global audience.

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