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Interactive comment on "CO_2 partial pressure and CO_2 emissions from the lower Red River (Vietnam)" *by* Thi Phuong Quynh Le et al.

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Anonymous Referee 4 Received and published: 14 February 2018 GENERAL COMMENTS Le et al. report a valuable and potentially interesting data-set of CO2 data measured with an equilibrator at several stations of the Red River. This is a valuable data-set as CO2 data directly measured are lacking worldwide, and in particular in sub-tropical and tropical environments. However, the paper suffers from a poor writing (English phrasing and syntax) that absolutely needs to be improved. Also, the presentation and discussion of the results are extremely convoluted. The authors make a list of numerous possible hypothesis but do not really provide a convincing interpretation of the data (a clear and solid "story-line"). Some of the comparisons in the discussion are out of scope and irrelevant such as comparisons with mangroves, beaver ponds, sea-ice and the Southern Ocean.

Thank you for the comments and suggestions. We revised the manuscript in taking into account all comments/suggestions from the reviewers.

MAJOR COMMENTS The discussion is extremely convoluted and goes in all sorts of directions but nothing really conclusive comes out of it. By looking at the pCO2 plot in Figure 2, we can conclude: - Differences between night and day are not statistically different whatever the site. The authors should try to explain this by comparing with other *river* sites where night-day pCO2 differences have been reported. The low phytoplankton biomass as indicated by chlorophyll-a content and the low differences in night-day temperature that induce low variations in CO2 solubility probably explain the low daily variations in pCO2. Anyway the low daily variations of pCO2 are an interesting aspect of the paper that deserves a longer discussion. Possibly make a table with studies that have shown daily variations of pCO2 with other variables such as average Chlorophyll-a content or POC, daily changes in temperature, . . . Thank you very much for the suggestion. We revised the text for discussing the temperature influence on pCO2 and fCO2 of the Red River in page 9, section "4.1 Temporal variations of pCO2 and CO2 fluxes of the lower Red River" as suggested. For example, we revise the text in page 9: "Concerning the lower Red River, water temperature did not show clear variation between the day and the night. In addition, low Chl-a concentrations were measured, from 0.5 to 3.1 μg L-1, probably as a result of the high turbidity limiting light penetration in the water column. Thus, phytoplankton activity had a low influence on C dynamic in the lower Red River system. Consequently, there are no clear variations of pCO2 and CO2 fluxes between the day and the night time at the different stations along the lower Red River" - Overall the spatial gradients (differences among sites) are very low, except for Hoa Binh. Probably that all sites are located very close and correspond to similar sized rivers with similar catchment characteristics (land cover + lithology) so that the spatial differences are low. An explanation for the higher values of Hoa Binh is needed. Is it because it's downstream of a dam? This would make sense based on existing literature (Sinnamary river, cf paper by Guérin et al.). Maybe it's mentioned somewhere in the text, but I missed the information being distracted by all of the other marginal bits and pieces of discussion.

Thank you for the suggestion. We revised the discussion about the influence of the dam in page 10 and 11, section "Influence of dams on pCO2 and CO2 emission"

"4.2.4 Influence of dams on pCO2 and CO2 emission Previously, reservoirs were suggested to decrease riverine pCO2 due to increased residence times and autotrophic production (Wang et al., 2007). However, Lauerward et al., (2015) found a low negative correlation between them. Abril et al., (2005) noted that intense mineralization of organic matter (OM) originating from the reservoir was possibly a significant source for pCO2 value in downstream river. In addition, the influence of the dam on the gas transfer velocity and then CO2 outgassing flux in the river downstream of the dam was also demonstrated in the study of the Sinnamary River (Guérin et al., 2007). In the present study, in the upstream part, pCO2 ranged from 964 ppm (at Yen Bai) to 3,830 ppm (at Hoa Binh), being highest at the Hoa Binh site where the lowest pH values were measured. Higher k600 values (from 63 to 68 cm h-1) were also observed at the Hoa Binh and Vu Quang sites. Noted that the Hoa Binh site is situated downstream a series of reservoirs, which have been constructed in both Chinese and Vietnamese parts including two large dams Hoa Binh (in 1989) and Son La (in 2010). The Vu Quang site is located in the downstream of a series of reservoirs, including two important Thac Ba (in 1970) and Tuyen Quang (in 2010). Previous studies emphasized that these dams have impacted water and sediment discharges downstream (Ha and Vu 2012; Ngo et al. 2014; Lu et al. 2015) with significant sediment deposition being observed in the reservoirs (Dang et al. 2010; Vinh et al. 2014; Lu et al. 2015). Thus, the higher pCO2 measured at these sites (average value of 3129 $\pm$ 32 ppm) may reflect the increased decomposition of OM and/or the water perturbation due to dam construction, especially for the Da River. The impact of dams on downstream pCO2 may be less for the Lo and the Thao Rivers (average values of 1395 $\pm$ 63 ppm and 993 $\pm$ 14 ppm, respectively), where less numbers and less size (only small and medium) of dams/reservoirs were built up in their upstream parts. Thus, the high pCO2 measured at these stations may reflect the increased decomposition of OM and/or the water perturbation due to the large dam construction."

- The seasonal variations are comparatively very small (except for Hoa Binh). This is an intriguing result that deserves being explored. The authors should compile in a table studies that report seasonal cycles of pCO2 (report the min-max of pCO2) and other relevant variables such as ratio of max/min of discharge, seasonal changes (minC2 max) of POC and DOC. It might be useful to show a plot with the full seasonal cycle of freshwater discharge and indicate the two sampling periods. This would allow readers to situate the samplings on the hydrograph. A table with the values of pCO2, POC, DOC, Chl a, SS and water discharge in dry and wet season was added in the in the Supplementary Material. A figure of daily river discharges at the outlets of the Thao, Da and Lo tributaries and the main axe of the Red River at Hanoi and SonTay was added in the Supplementary Material.

- The fact that the city of Hanoi does not seem to influence markedly the O2 and pCO2 levels is also intriguing and deserves some discussion. Was the station located within the city itself or slightly downstream ? Could this be due to the fact that freshwater discharge is relatively important (2000-3000 m3/s ?). A tabular comparison of studies that have shown the influence of cities on river CO2 might be useful. In rivers, the main driver of the gas transfer velocity is turbulence generated by flow. I suggest that the authors use flow velocity data (that are available at gauging stations) and use the equations of Raymond et al. (2012) to compute the gas transfer velocity and fluxes (equation NâŮę5 of Table 2 of Raymond et al. (2012) is recommended).

Thank you very much for the suggestion. We now calculate the k600 by using the equation of (Raymond et al 2012) as suggested. Hanoi station locates within the city itself and at this site, the river has not received yet the wastewater discharge of the whole city. This may explain that the city of Hanoi does not seem to influence markedly the O2 and pCO2 levels. We revised the section "Influence of population density on pCO2 and CO2 emission" in page 12 as followings:

"4.2.5 Influence of population density on pCO2 and CO2 emission Previous studies demonstrated very high value of pCO2 in river estuaries as a result of different human activities. For instance, pCO2 up to âĹij25,000 ppm was measured in the Rhine estuary (Kempe, 1982) or up to âĹij15,200 ppm in the Scheldt estuaries due to high discharge of pollutants (Borges and Frankignoulle, 2002). Concerning the Red River, from the upstream to the downstream part of the main axe, pCO2 together with CO2 outgassing flux slightly increased from Yen Bai (993 $\pm$ 14 ppm and 364.9 $\pm$ 10.3 mmol m-2 d-1 respectively) to Hanoi (1,275 $\pm$ 17 ppm and 304 $\pm$ 7.3 mmol m-2 d-1), whatever the season. However, it is worth to note that the Hanoi station was located within the city itself and at this station, the river has not yet received the wastewater discharge of the whole city. Consequently, the Hanoi station in this study may not reflect the influence of whole city, with probably lower O2 and higher pCO2 levels as observed for other urban rivers in the Red River Delta (Trinh et al., 2007; 2009; 2012)"

For all of the spatial and seasonal comparisons, some firm statistical testing is required (t-test, ANOVA, . . .). We now checked all of the spatial and seasonal comparisons by statistical testing from t-test and ANOVA, in page 6 for the method and throughout the ms. "Student t-test was used to test the difference of variables values between the two different times (the wet and the dry) and (the day and the night), whereas ANOVA was used to test the difference of variables within stations on the measured mean variables. Probabilities (p) were determined and a p value of < 0.05 was considered to be significant"

SPECIFIC COMMENTS L29-30: could also be due direct inputs of CO2 from soils or wetlands. Yes, it is a possibility, and it was added

L48-51: this is a very complicated way to state that C fluxes from rivers depend on lithology and land cover. This sentence was revised "Riverine carbon concentrations and CO_2 outgassing from rivers are impacted by both natural and human factors (Liu et al., 2016; Liu et al., 2017)."

L59: improve instead of "precise" It was changed

L75: Please justify the choice of the 5 stations. Also, it needs to be clearly explained that one of the stations is influenced by seawater intrusion. Are the other stations affected by tidal wave propagation ? The paragraph was revised as followings: "Five stations were studied in the lower Red River (Vietnam): Yen Bai station (at the outlet of the Thao river); Hoa Binh station (after Son La and Hoa Binh reservoirs, at the outlet of the Da River); Vu Quang (at the outlet of the Lo River); Hanoi and Ba Lat stations (in the main course of the Red River downstream). The three stations Yen Bai, Vu Quang and Hoa Binh are representative for three main tributaries (Thao, Da and Lo) of the upstream Red River whereas the Ha Noi station is representative for the main course Red River after confluence of three main tributaries. Within 5 stations observed, only the Ba Lat station which is located in the Red River mouth, about 13 km from the sea is influenced by seawater intrusion (Fig 1)."

L94: provide information on land cover, lithology, and other relevant catchment characteristics for the studied rivers. Some information concerning geomorphology, lithology or other river characteristics of the studied rivers were added in the text (page 3) as suggested. "The Delta is located in a very flat and low land, with an elevation ranging from 0.4 to 12 m above sea level (Nguyen Ngoc Sinh et al., 1995). Previous studies showed the difference of lithology in the three upstream tributaries: Paleozoic sedimentary rocks (55.5

L107: replace "sensor" by CTD It is not a CTD. It is a YSI probe with different sensors. So we revised the sentence "Physico-chemical parameters were automatically recorded every minute during 24h for each sampling campaign: pH, turbidity, salinity, chlorophyll a by a YSI6920 multiparameters probe (YSI, USA)"

L110: "all data must be entered on the documents" ? What does this mean ? This sentence was deleted

L113: total “alkalinity” It was corrected

L117-126: provide precision and accuracy of all the measured variables. Everywhere in the ms (text and tables) pCO2 values should not be given at the tenth of ppm, given that with an equilibrator precision of pCO2 measurements is typically of +/-1 ppm and accuracy of pCO2 measurements is typically of +/- 3-5 ppm (at best).

Precision and accuracy of the measured variables were added in the section 2.2 Sampling procedures and analysis

L125: what was the volume of sample water? What acid and concentration was used ? How was the end-point determined? What titrator was used? The authors discuss the differences of pCO2 measured directly and calculated from pH and total alkalinity (TA). However, for this discussion to be meaningful it is necessary to have an idea of the quality of the pH and TA measurements, and this is only possible if analytical techniques are described in detail. This is lacking for TA here, and elsewhere the authors need to provide information on the pH electrode calibration. Type of buffers, frequency of calibration, etc. The method for alkalinity determination was added in page 4 “Total alkalinity of the hourly samples was immediately determined on non-filtered water samples (30ml water sample) in situ by titration method with 0.01M HCl (APHA, 1995). For each sample, triplicates were titrated and the analytical error was below 3 pH electrode was calibrated using standard solutions (pH = 4.01 and pH = 6.88, Merck) and the pH precision and accuracy was $\pm$ 0.01.

TA data need to be expressed in μmol/L (as by convention). This is the unit of the TA as input variable for the CO2sys program. So why express the data in mg/L ? Further the data expressed per mass (mg/L) instead of per moles are extremely confusing. Is it mg of C ? Or mg of H+ ? (based on the conventional definition of TA as the quantity (number of moles) of protons to titrate bases in one kilogram (or L) of water). Should it be mg of C, then the TA for the first line of Table 2 (105 mg/L) would be 8750 μmol/L.

In this case the computation of pCO2 gives 3670 ppm for a pH of 8.2 and 26.4âŮęC (and not 270 ppm as stated). Should it be mg of CaCO3, then the TA for the first line of Table 2 (105 mg/L) would be 1050 μmol/L. In this case the computation of pCO2 gives 440 ppm for a pH of 8.2 and 26.4âŮęC (and not 270 ppm as stated). Should it be mg of CO3, then the TA for the first line of Table 2 (105 mg/L) would be 1750 μmol/L. In this case the computation of pCO2 gives 733 ppm for a pH of 8.2 and 26.4âŮęC (and not 270 ppm as stated). The result from TA analysis is in mg/l CaCO3 as units. During calculating pCO2 by CO2-SYS, when needed, it was converted into μmuol/l or mol/l. In the revised version we re-calculated pCO2 values

L140: Abbreviation IRGA not defined. It was added in the text "a portable InfraRed Gas Analyser (IRGA) (Licor 820, Licor®, USA)"

L144: replace "balanced" by equilibrated. It was replaced

L156: This equation was established in the 1970's well before the paper of Raymond and Cole. This sentence was deleted for the revised version

L160: replace "function" by parameterization. This sentence was deleted for the revised version.

L167: solubility changes with temperature, why did you state a constant value ? We calculated the fCO2 at 5 sited of the Red River with different α values : α = 3.941 10-2 mol.L-1.atm-1 at 24 oC for dry season and α = 3.138 10-2 mol.L-1.atm-1 at 27 oC for rainy season. The average flux of CO2 outgassing was 525.7 mmol.m-2.d-1. Then, this value was very closed with the one calculated from the constant α = 3.4 10-2 mol.L-1.atm-1 we used for both rainy and dry seasons of the Red River in this study, which resulted in the fCO2 average of the whole Red River 530 mmol.m-2.d-1. An explanation was added in page 6 "ïĄą is the solubility coefficient of CO2 for given temperature and salinity (Weiss, 1974) (mol L-1 atm-1). In this case, ïĄą = 0.034 mol L-1 atm-1. In this study, salinity variations were low, except for the Ba Lat station. Temperature did not change a lot. We checked the influence of different ïĄą values in the dry (α = 3.941 10-2 mol L-1 atm-1 at 24 oC) and the wet season (α = 3.138 10-2 mol L-1 atm-1 at 27 oC) at the 5 sites and compared with the constant ïĄą value of 0.034 mol L-1 atm-1"

L174-190: I suggest that this section is removed, as this topic has been discussed at length in several dedicated papers. This section was deleted as suggested

L195: The speciation of DIC was established decades before the papers of Cai et al. 2008 and Sun et al. 2010). The paper of Park (1969) formalised the equations to compute the speciation from all of the possible combinations. The reference Park (1969) was added

L203: The calculated pCO2 is about 5 times lower than the pCO2 measured directly, this can hardly be considered as 'similar results' as stated. We apologize for the error of values pCO2 calculated in the table 2. The text was also revised for discussion about the difference between pCO2 measured and calculated in page 7-8, as followings:

"3.3. Comparisons of the pCO2 results obtained by the two methods pCO2 along the lower Red River (Vietnam) in the dry and the wet seasons were determined by two methods: i) direct measurements using an equilibrator connected to an IRGA, ii) calculated from pH and alkalinity using the CO2-SYS® software. The direct pCO2 measurements gave slightly higher values than the calculated ones (Table 2), but the values of two methods were similar and presented the same trend of spatial and seasonal variations (R2 = 0.77, Fig. 2; Table 2). Lower values of the calculated pCO2 in this study may be caused by the analytical errors in pH or under-estimation of total alkalinity. Similarly, the CO2 outgassing rates which were calculated from measured pCO2 from equilibrator were higher than the ones derived from the calculated pCO2 from CO2-SYS, however they are in the same orders and have similar variation trends (Table 3, Fig 2). "

L230-235: Present and discuss either salinity or conductivity but not both as they provide the same information. We revised the sentence as followings: "Conductivity followed the same trend of the salinity variation which were around 0.2±0.0 mS cm-1 for 4 upstream sites and higher values were found for the Ba Lat site, especially in dry season (up to 6.6±3.4 mS cm-1)($p < 0.05$)" in page 7.

L233: Statistical test needed. Statistical test was added throughout the revised ms as suggested

L245-251: Statistical test needed. Statistical test was added throughout the revised ms as suggested

L260: The "good" correlation is of marginal interest what's relevant is that the pCO2 values from the two methods differ by a factor of 5 Again, the text was also revised for discussion about the difference between pCO2 measured and calculated, as followings in page 7 -8 "3.3. Comparisons of the pCO2 results obtained by the two methods pCO2 along the lower Red River (Vietnam) in the dry and the wet seasons were determined by two methods: i) direct measurements using an equilibrator connected to an IRGA, ii) calculated from pH and alkalinity using the CO2-SYS® software. The direct pCO2 measurements gave slightly higher values than the calculated ones (Table 2), but the values of two methods were similar and presented the same trend of spatial and seasonal variations (R2 = 0.77, Fig. 2; Table 2). Lower values of the calculated pCO2 in this study may be caused by the analytical errors in pH or under-estimation of total alkalinity. Similarly, the CO2 outgassing rates which were calculated from measured pCO2 from equilibrator were higher than the ones derived from the calculated pCO2 from CO2-SYS, however they are in the same orders and have similar variation trends (Table 3, Fig 2).

L261-269: This result is extremely intriguing because the sampled rivers are within the "acceptable" range of applicability of the computation of pCO2 from pH and TA, with low DOC and high pH values. The under-estimation of the computed pCO2 could be due to a bias in the measurements of pH and TA, which is not possible to evaluate given the lack of information in the material and methods. Anyway, the under-estimation of

TA and/or over-estimation of pH could explain why the computed pCO_2 is very low compared to directly measured pCO_2. The method for TA and pH determination was added in the revised manuscript in page 4. We may under-estimate TA resulting the lower computed pCO_2 compared to directly measured pCO_2. A paragraph was added to explain the difference of pCO_2 computed and pCO_2 measured in page 7 -8

"3.3. Comparisons of the pCO_2 results obtained by the two methods pCO_2 along the lower Red River (Vietnam) in the dry and the wet seasons were determined by two methods: i) direct measurements using an equilibrator connected to an IRGA, ii) calculated from pH and alkalinity using the CO2-SYS® software. The direct pCO_2 measurements gave slightly higher values than the calculated ones (Table 2), but the values of two methods were similar and presented the same trend of spatial and seasonal variations (R^2 = 0.77, Fig. 2; Table 2). Lower values of the calculated pCO_2 in this study may be caused by the analytical errors in pH or under-estimation of total alkalinity. Similarly, the CO_2 outgassing rates which were calculated from measured pCO_2 from equilibrator were higher than the ones derived from the calculated pCO_2 from CO2-SYS, however they are in the same orders and have similar variation trends (Table 3, Fig 2).

L271-298: This discussion is of marginal interest because it depends on the way the gas transfer velocity was computed from a parameterisation as function of wind speed. pCO_2 did not change markedly from night to day, but wind speed was higher during the day than the night. This is somewhat trivial, and if the authors re-compute the fluxes with a parameterization as function of flow, this day-night difference will be erased. Thank you for the comments. We remove this previous discussion since we recalculated the k600 and flux of CO_2 outgassing. K600 was calculated as presented in the section "2.5 CO_2 fluxes determination" , page 5-6: "In this study, k600 was calculated using the equation from Raymond et al. (2012) based on stream velocity (V, in $m\ s^{-1}$), slope (S, unitless), depth (D, in meters) and discharge (Q, in $m^3\ s^{-1}$), as follow: $k600 = 4725 \pm 445 \times (V \times S)^{0.86 \pm 0.016} \times Q^{-0.14 \pm 0.012} \times D^{0.66 \pm 0.029}$ Eq. (2) L298:

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Regarding the Ho et al. (2016) study, a note of caution is needed. This study used wind speeds from a sonic anemometer above the mangrove forest (obviously higher wind speeds that the level of the river below the canopy) and located 4 km from the coastline while the tracer injection point was located 12 km away from the coastline. For both these reasons, wind speed data used to build the Ho et al. (2016) relationship is over-estimated, meaning that the relationship itself is not reliable, and the role of currents in generating turbulence (and driving the gas transfer velocity) probably underestimated. Thank you for the comments. We remove this part of the discussion since we now recalculate the k600 and flux of CO2 outgassing of the Red River using a different equation. K600 was calculated as presented in the section "2.5 CO2 fluxes determination", page 5-6: "In this study, k600 was calculated using the equation from Raymond et al. (2012) based on stream velocity (V, in m s -1), slope (S, unitless), depth (D, in meters) and discharge (Q, in m3 s-1), as follow: k600 = 4725 $\pm$ 445 x (V x S) 0.86 $\pm$ 0.016 x Q-0.14 $\pm$ 0.012 x D 0.66 $\pm$ 0.029 Eq. (2)

L299-312: In this discussion there's a mix of studies in lakes and mangroves, which makes little sense when discussing CO2 dynamics in rivers. Thank you for the suggestion. We revised the paragraph and removed all reference studies un-necessary in lakes and mangroves throughout the revised ms.

L302: Roulet et al (1997) report on beaver ponds. This is a very specific environment that is not very relevant for comparison with Red river. The reference Roulet et al (1997) was removed as suggested.

L321: references are needed to back this statement. The reference (Richey et al 2002) was added in the revised ms.

L323: or (in addition) inputs of CO2 from wetlands. It was added as suggested

L335: Chanda study deals with mangroves and Takahashi with oceans. This is irrelevant (and out of scope) to discuss CO2 dynamics in rivers. The references were removed.

L338: Comparison with Southern Ocean is irrelevant (and out of scope). The sentence was removed

L346: Why did you not include freshwater discharge in the PCA and the correlation matrix analysis ? The PCA and the correlation matrix took into account river discharge.

L359: Statistical test needed. Statistical test was added throughout the revised ms.

L397: The cited references deal with Artic Ocean and sea-ice, not with Pearl River. This was removed from the revised ms.

L401: Statistical test needed. Statistical test was added throughout the revised ms.

L407-408: The city of Ha Noi does seem to influence DOC and POC but there seems to be little effect on O2 and pCO2. This is intriguing. We revised the discussion in page 12, in the section "4.2.5 Influence of population density on pCO2 and CO2 emission Previous studies demonstrated very high value of pCO2 in river estuaries as a result of different human activities. For instance, pCO2 up to âĹij25,000 ppm was measured in the Rhine estuary (Kempe, 1982) or up to âĹij15,200 ppm in the Scheldt estuaries due to high discharge of pollutants (Borges and Frankignoulle, 2002). Concerning the Red River, from the upstream to the downstream part of the main axe, pCO2 together with CO2 outgassing flux slightly increased from Yen Bai (993 $\pm$ 14 ppm and 364.9 $\pm$ 10.3 mmol m-2 d-1 respectively) to Hanoi (1,275 $\pm$ 17 ppm and 304 $\pm$ 7.3 mmol m-2 d-1), whatever the season. However, it is worth to note that the Hanoi station was located within the city itself and at this station, the river has not yet received the wastewater discharge of the whole city. Consequently, the Hanoi station in this study may not reflect the influence of whole city, with probably lower O2 and higher pCO2 levels as observed for other urban rivers in the Red River Delta (Trinh et al., 2007; 2009; 2012)."

L410: some of the cited "rivers" are in fact mangrove brackish systems (irrelevant and out of scope). We checked the cited "rivers" in this paragraph

L426-429: the authors provide no evidence to back up this hypothesis that is mainly speculation. The sentence was removed from the revised manuscript.

L427: Elsewhere in the discussion the authors say that the Hoa Binh site has high pCO2 because downstream of a dam, and here the authors say the low pCO2 values in the Red River is also due dams. This cannot work both ways. Should the authors want to explore why pCO2 values are lower in the Red River than other rivers worldwide comparing levels of DOC and POC with other rivers might be useful. Also, some studies have provided hypothesis to explain large-scale variations of pCO2 across river catchments such as productivity, population density, temperature, . . . e.g. the Lauerwald study that is cited by the authors. The sentence was removed from the revised manuscript. The discussion about different factors that impacted on pCO2 and fCO2 of the Red River was re-written and re-organized through the revised manuscript.

L434-451: This section provides a summary of the paper (and duplicates the abstract) and does not provide a real conclusion. The conclusion was revised

L565: typo in "Costs" The reference was removed from the revised manuscript

L598: Nathalie is the first name not the family name. The first name and family name of the authors in this reference were corrected

L616: Patricia is the first name not the family name. The reference was removed from the revised manuscript.

L618 and L650: This reference appears twice. The reference was removed from the revised manuscript.

Table 3: There's no need to provide the air-water CO2 fluxes derived from the pCO2 calculated from pH and TA. Since calculated pCO2 is under-estimated compared to directly measured pCO2, this is also the case for the fluxes, in a very predictable and trivial way. The flux of CO2 outgassing derived from the pCO2 calculated from pH and TA was removed from the table 3.

In tables and figures is given in the turbidity of the probe in NTU, when in fact authors measured TSM. Why not show also (or instead) the TSM data ? Also, it might be useful to show the POC follows closely TSM, but The data on TSM and

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-505/bg-2017-505-AC5-supplement.pdf

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Supplementary Material

CO_2 partial pressure and CO_2 emission along the lower Red River (Vietnam)

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Figure SM1: Daily variation of river discharge at the outlet of the Thao (Yen Bai), Da (Hoa Binh), Lo