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Biogeosciences Discussions

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Interactive comment

Interactive comment on "Physical-controlled CO_2 effluxes from reservoir surface in the upper Mekong River Basin: a case study in the Gongguoqiao Reservoir" *by* Lin Lin et al.

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Thank you very much for the comments. We appreciate your questions and opinions on the arguments and we are now looking for a third-party proofreading of language. Comment 1: Firstly, I don't think the argument that few CO2 efflux measurements have been made in China is substantiated (see global map in Figure2 of Deemer et al. 2016). The authors even cite a number of other studies of carbon dioxide dynamics in Chinese reservoirs. I think the authors could emphasize the importance of understanding these dynamics in the Mekong basin given all the reservoir development that is slated for the region (maybe cite Zarfl et al. 2015 Aquatic Sciences). The authors could also

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do a better job of describing the unique hydrology/climate in the Mekong Basin since the diverse readership base may not be familiar with the characteristics of dry vs. wet seasons in this region. Response: Thanks for the reasonable comments on the citation. It was true that many studies on pCO2 in reservoirs accumulated in China. But many current CO2 efflux from reservoirs in China were estimated with pCO2 and wind speed but not direct measurements. Even though under most of the circumstances the pCO2 could predict the efflux effectively, we thought the CO2 efflux estimated in this way could be underestimated as some physical controls on CO2 emissions could be neglected. Hence, this research was based on quantification of the CO2 emissions from a reservoir in the upper Mekong River, because Most of the studies focus on the variation of pCO2 in surface water rather than the emissions. Yet we agreed that the Introduction should be better to emphasize the importance of understanding these dynamics in the Mekong Basin. Thus, we will add some information about the South Asia monsoon climate and potential effect of artificial operation on the CO2 production and emissions from the reservoir and describe the potential monsoonal/hydrological effect on the CO2 emissions for those readers who do not familiar with the catchment. We hope that this could distinguish our study from other existing research on the dynamics of CO2 production and emphasize its necessity. Comment 2: Secondly, I think the authors should be careful in their discussion of global carbon budgets vs. reservoirs as greenhouse gas emitters-specifically, there is no mention in the paper about the potential role of methane as a GHG source and it is somewhat implied that CO2 might be the dominant emission pathway even though it is generally accepted that methane is often the dominant GHG source on an CO2 equivalent basis. Response: Yes, we agreed with the referee that the methane is the dominant GHG source on the CO2 equivalent basis at global scale. We were trying to say that quantitively the amount of carbon dioxide released from the reservoirs was higher than that of methane if the global warming potential was not considered. But the expression could be awkward and lead to misleading implication. As suggested, we should be more careful in evaluating the effect of carbon dioxide vs. methane. Since the article focuses on the CO2 emissions instead of methane, we decided to delete the description of greenhouse gases and focused on the damming effect on CO_2 emissions. Comment 3: Thirdly, I think the authors need to better integrate the diel sampling component of their study into the way that the other results are analyzed. The authors don't mention the temporal sampling scheme employed during their 16 sampling campaigns-were sites always sampled in the same order? Over what range of times? Are we confident that variation in fluxes measured is more a function of spatial variation than temporal variation? Response: Thank you for the kind reminding. We will add the information about the sampling timing. All the sixteen sampling campaigns were implemented in the daytime and basically followed the same order. Each campaign usually last for two days. In the morning of the first day (usually 9am), the sampling will be started from Point R1 to P2. Sampling in each point costed around 40 minutes so the sampling for the four points (R1, P1, L and P2) could be completed before 4pm. In the second day, the sampling starts at R2 (the time varied from 10am to 11am), following by Point D (1pm). In the third day, the sampling at Point P3 and P4 requires the boat so normally the campaigns were conducted in the afternoon around 3-5pm. We do not think the diel variation would overshadow the effect of seasonal variation as the sampling timing was similar in all campaigns. We believed that the variation in fluxes is a function of both spatial and seasonal variation. There was significant variation in fluxes between riverine sites and the pelagic sites, but the variation is only significant in the dry season. In the wet season there is no significant spatial variation in fluxes between riverine sites and reservoirs sites (maybe the littoral area need to be isolated). It means that the extremely high emission rates only occurred in the riverine sites in the dry season. Since the sampling campaign spanned two to three days, different sampling dates might also lead to variation in fluxes. However, it should be noticed that average water retention time of the studied reservoir is 1.4 days. This type of daily-operated reservoir usually experienced repetitive fluctuation of water level in daily cycle, according to the operation guide of Chinese reservoirs. In the dry season, in particular, everyday the water level will be drained down to the lowest level (this is consistent with what we observe in the studied reservoir though we do not have enough data of water level to support it). In the same timing each day, the hydrological condition did not vary too much. Actually, the diel variation in water level might cause much more variation in fluxes than that sampling in different days. Yet we cannot deny the effect of diel variation of hydrological condition on the variation of flux and that is why the diel sampling was conducted. However, our diel sampling did not capture the variation of CO2 emissions for a whole daily cycle. As shown in Fig. 5, the diel sampling did not cover the CO2 effluxes from 9am to 12noon, during which the sampling was conducted in the riverine sites. Due to the incomplete diel sampling, we had to average the daily flux and nocturnal flux respectively and integrated them into other results. We cannot calibrate the flux for all the sampling points according to the timing since sampling in riverine area were actually conducted in the period that we did not capture in the diel sampling. For the same reason we could not conclude that the flux was not independent of the sampling timing. Given the significant spatial variation and temporal variation in fluxes, we examined and separate their effects with correlation analysis between flux and some controlling factors like light and heat (which was represented by water temperature). Firstly, we are confident that the light availability did not affect the spatial variation of fluxes since all the sampling were conducted when sunlight was sufficient. Secondly, we did not find the significant relation between water temperature and flux ($p>0.10$) as the water temperature varied very little in the diel sampling. Instead, the relation between pCO2 and flux was significant (correlation coefficient = 0.665, $p<0.001$). Thus, the diel variation in flux was attributed to the variation of pCO2. Assuming the measured fluxes in different points were caused by temporal variation, higher efflux must be resulted from higher pCO2. However, no significant relation between pCO2 and flux was found in the whole dataset and in all the grouping zones ($p>0.10$). Yet the relation is significant in Point P4, though the correlation coefficient is negative. Therefore, we believe that some physical factors other than pCO2 caused the variation in different sampling points. The timing and order of sampling campaigns will be added into the Methodology part and the correlation coefficient will be added into the Supporting Information for further clarification. Comment 4: Fourth, while I think that hydrology may be a dominant control on reservoir CO2 emissions in this reservoir (e.g. it seems a completely valid and plausible hypothesis), I don't think the authors present enough evidence in support of this mechanism to present it as a result (e.g. in the abstract of the paper). Reservoir hydrology co-varies with other seasonal variation in temperature and the authors present no systematic approach for differentiating other possible controls. Response: As our reply in Comment #3, we believed that there was supposed to be some factors other than temperature controlling the seasonal variation in flux at river inlet owing to the insignificant relation between water temperature and flux. However, the gradient in water temperature between inflow and receiving waterbody was significantly related to the flux in all the river inlet($p<0.001$). Therefore, we speculated that the flux might be rely on the various mixing mode. According to Summerfield (1991), the mode of mixing between sediment-laden river water and receiving waterbody was dependent on the relative water density. The hyperpycnal flow occurs when the incoming water was colder, denser and contained more suspended sediment loads than the receiving waterbody. On the contrary, hypopycnal flow occurs while the inflow was more warmer and clearer than the stagnant water in the receiving body. Here in Fig. 1&2 the flux was negatively related to the SPS concentration gradient at the riverine inlets. They showed that the high fluxes occurred when the inflow was warmer, less turbid and lighter than the receiving waterbody in the reservoir. As in dry season the inflow was warmer, less turbid and less dense than the reservoir water, we consider that the inflow became an overflow on surface and the higher pCO2 can enhance the emission rate. The situation was opposite in the wet season when the heavy turbid flow plunged into the reservoir bottom and short water retention time allowed little time for mineralization of organic carbon. The hypothesis was also supported by the negative relation between water discharge and CO2 flux (Fig. 3&4). But considering the mixing mode and hydrological condition could be covaried with the water temperature, we will add scatter plots showing that the flux in river inlets were significantly ($p<0.001$) related to the gradient in suspended sediment concentration between the incoming water (rep-

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resented by R1 & R2) and receiving waterbody (represented by P1 & P4) as well as the relationship between flux and water discharge ($p<0.001$) as evidence. See the graphs here and Fig. 7 in the manuscript. Comment 5: Finally, it is difficult to interpret the zonation grouping-the authors should consider incorporating a statistical assessment of significant differences between sites. For example, were the riverine samples from both sites more similar to each other than to other sites? Or was one riverine site emitting CO_2 at much higher rates than the other? Reservoir inlets are often hot spots for biogeochemical activity-are we sure that these riverine sites are fully riverine and that their hydrology isn't influenced by the dam? In addition to these scientific concerns, the manuscript needs to be edited for proper English. There are grammatical issues and vaguely written statements that could benefit from a third-party editor. Response: We grouped the sampling points according to their surface velocity. The flow velocity in the surface water was 0.2m/s and 0.7m/s at the Point R1 and R2 respectively, according to the data measured in the preliminary fieldwork in this research in 2015). The Point D at the downstream of the dam also maintained a flow velocity but the flow was largely regulated by the dam. All the other points (including P1$\sim$P4 and L) was located within the backwater area and no flow velocity can be detected at the water surface at these locations. The fluxes from riverine sites were significantly different from the other sites (see Page 6, Line 17) but fluxes from R1 and R2 did not show significant difference ($p>0.10$). Therefore, we are confident that the fluxes at the riverine sites were significantly higher than the sites in reservoirs and at the downstream of the dam. Generally, we considered that the water in the backwater area is stagnant in a reservoir and no flow velocity can be detected on its surface, even though subsurface flow could be maintained as the water was discharged to the downstream (as we put it in Page 3, Line 24). The boundary of backwater area is the boundary of a reservoir and the upper bound of the influenced area where the backwater pushed by the dam can influence the hydrological condition. But this boundary could be varied due to various water level. In this case, we selected the points at the upstream of the boundary as the river inlet and minimize the effect from the Gongguoqiao Dam and guarantee the water was still flowing on surface. But unfortunately, we cannot really exclude the effect of the dam under construction at the upstream of the sampling points. We believed that the water discharge was not affected by the Miaowei Dam but the dam under construction might change the deposition processes of sediments. We've tried to highlight the significant differences in the article. We will add the surface flow velocity to the introduction of riverine sampling points to validate the zonation grouping. Page 1 Line 12: change "cycle" to "cycling" Response: We changed as suggested. Page 1 Line 14: did the authors use a statistical approach to see if reservoir emissions were significantly different by season? Response: Yes. We used the Non-Parametric Tests (Independent Samples) to test the difference. The emission rates showed significant difference between the dry season and the wet season ($p<0.001$). Page 1 Line 17: I don't think the analysis presented here conclusively linked CO2 emissions to physical mixing. Response: Even though the positive relation between water temperature gradient and CO2 emission rate could hardly suggested the influence of different mixing mode, the relation between CO2 emission rate and sediment gradient between river inlet and receiving waterbody and different seasonal variation trend of CO2 emission rates and water discharge can link the emission to the mixing mode. Neither pCO2 or water temperature cannot explain the seasonal variation of flux for riverine points. Thus, there was supposed to be physical processes affecting the emissions. Page 2 Lines 3-5: Carbon dioxide is generally thought of as the largest contributor to total carbon emissions, but methane is generally the largest contributor to total greenhouse gas emissions on a CO2 equivalent basis. I think the authors should be careful to make this distinction clear. Response: The sentence was revised as "Since carbon dioxide takes up largest portion in total carbon emission from inland waters". Because we are not going to present the data of methane flux, we will only emphasize the amount of the carbon dioxide in carbon emission while not consider the effect of methane. Page 2 Line 18: By "biogeochemical processes of phytoplankton" do you just mean photosynthetic uptake? Response: Yes. As the word here is too vague, we changed the word into "photosynthetic uptake" as suggested. Page 2 Line 24: The way you have phrased this sentence makes it sound

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like all the studies you are citing were conducted in the Three Gorges Reservoir, but Pacheco et al. 2014 was in Brazil. Also, I don't see Tao 2017 listed in your references section. Response: The sentence was reorganized and the citation of Tao 2017 was deleted. Page 2 Line 25: Do you mean watershed? Not waterbody? Response: The article here refers to the eutrophication in the waterbody (see Page 2 Line 22). The stagnant tributaries which was impacted by the backwater can suffer severe eutrophication as the nutrient input cannot diffuse and thus cause algae bloom. Page 3 Line 5: Why is information about Xiaowan Reservoir relevant here? Also, perhaps this is a good place to mention the construction of Miaowei Dam (which is noted in your Figure 1). Was the dam completed after your sampling ended in Dec 2016? Was the system hydrology affected at all by the fact that a dam was being constructed upstream during your study? Response: Because the outflow of Gongguoqiao Reservoir feeds directly into the Xiaowan Reservoir, we need to exclude the effect of backwater of Xiaowan Reservoir on the hydrological condition at Point D. We will add some introduction to the three reservoirs (Miaowei, Gongguoqiao and Xiaowan) here and supplement the detailed sampling timing and how we define the riverine sites. It will be highlighted here the sampling ended before completion of the Miaowei Dam. The construction of the dam, possibly impact the deposition of sediments in the riverine site but did not regulate the flow. Page 3 Line 9: Is this a hydropeaking (load following) reservoir? It might be nice to see water level data from the reservoir given the current discussion of water level fluctuation you have incorporated into your discussion. Page 3 Line 16 (and throughout): You use "mainstream" when I think you mean "mainstem". Response: According to the meaning of hydropeaking reservoir we searched online, we confirmed that the reservoir is a hydropeaking reservoir. We will supplement the water discharge data to represent the variation of water level. We will replace the "mainstream" with "mainstem" throughout the article. Page 4 Line 2: Consider reformulating the equation to take out unit conversion factors (which seem a little distracting and un-necessary). Response: The equation was quoted from the reference (Page 3 Line 30) but we can take out the conversion factor. Page 4 lines 26-28: The authors discuss dam hydrol-

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ogy as if they don't know what type of spill practices are employed in the reservoir. Isn't this information available? Also, the height of reservoir spill (epilimnion versus hypolimnion) could be mentioned in the study area section. Response: Since the water level frequently fluctuated in the reservoir, the height of reservoir spill might be variable. But we did know that the water passing the turbine was drawn from epilimnion as the hypolimnion water was too turbid that it will harm the turbines. The staff from the reservoir told us that in the rainy season the water passing the turbine was drawn from a layer around 4m deep under the water surface. This is consistent with our observation that the flow velocity of subsurface flow was highest at around 5m deep at a high water level. The minor difference in water temperature between Point P3 and D was consistent with this spilling practice. This information of spilling practice will be added into the introduction part of study area and the sentences at Page 4 Line 26-28 will be changed accordingly. Page 5 line 3: Why do the authors feel that the dataset is limited? Is there reason to think that sometimes the running waters from inflow are not more aerobic than the reservoir water? Response: Theoretically, the running waters from inflow are more aerobic than the reservoir water. But since a dam was under construction at the upstream and the DO data was unavailable since October owing to malfunction of the instrument, we are uncertain about that in the dry season. We will add this information and explanation in the sampling part. Page 5 line 11: Change this sentence to something like "With the exception of one sample, the reservoir was consistently supersaturated with CO2, indicating its role as a CO2 source to the atmosphere" Response: We changed it into "Most of the water samples had pCO2 higher than the atmospheric values.". Page 5 Lines 20-24: A plot that shows water level and point CO2 measurements over time might be helpful hereâĂŤI got a little lost in this description of the results. Response: We will add the plot showing the relation between CO2 flux and water discharge here. See Fig. 3&4 in the reply letter. Page 7 Line 1-2: Where do the authors show this analysis? Right now there is no mention of a statistical analysis of drivers and no corresponding table or figure. Response: We will delete the part as now we are doubting this relation as the nutrient and chlorophyll can co-vary with the SPS concentration. Page 7 Lines 4-21: So, given these results, are you confident that the CO2 efflux measurements you made are still predominantly representing spatial (rather than temporal) variation? Also, it sounds like physical differences (rather than biology) may be driving the differential emissions you see during the day versus at night? Would you agree? Response: The results showed that the higher emission rates were only found at the riverine inlet in the dry season (spring and winter). The effluxes showed large diel variation, and this might affect the spatial variation. As the reply in Comment #3, we followed the same order of sampling at each site and kept the similar sampling timing in each campaign. Except the incontinuous sampling and the diel sampling, no sampling was conducted at night. Thus we think the diel variation will not have effect on the analysis of seasonal variation. Page 7 Line 24: How do you define a pristine river channel? Was R1 at all influenced by the construction of Miaowei Dam? How do you differentiate free-flowing river from reservoir inlet? Response: We here define the pristine river channel as no dams at the upstream impounded the water and regulate the flow. We cannot deny that the flows at R1 could be influenced by the Miaowei Dam. Flow velocity and deposition processes might be affected since the river channel had changed. But since the dam did not regulate the discharge, we believe that the seasonal variation of water discharge would remained the same but possibly with less sediments. We consider that the river reach with surface velocity over zero (v>0m/s) as free running river while the reservoir inlet was supposed to be a profile close to the boundary where the surface flow velocity decreased to zero. As the boundary of backwater area varied frequently due to the fluctuation of water level, we have been trying to select a sampling close to the boundary where the river was free-flowing as the reservoir inlet. Page 8 Lines 11-12: I don't think Figure 7 really shows this. Response: The evidence is not sufficient here. We will add the graphs and some discussions explaining how different mixing modes lead to the variation in fluxes. Page 9 Line 8: Not sure "constraint" is the right word. Response: It refers to that the emissions at the downstream was kept at a low level. Possibly we can change the word to "restricted". Page 10 Line 17: Why "potential"? Response: The word could be redundant and we will delete it. Page 10, Conclusion: No discussion about why emissions were so high from the river in the dry season. Was this pattern consistent in both river sites? Response: We have tried to explain different mixing modes leading to the different seasonal variation in the CO2 fluxes in Page 10 Line 22-25. Possibly the explanation was not clear enough. With the correlation analysis between CO2 effluxes and water discharge as well as SPS concentration, we can explain the modes and how they influence the CO2 fluxes much clearer. Page 10, Line 31: What pattern are you referring to? Response: This pattern refers to the higher emission rates in the dry season. We will explain that in the sentence. Figure 6: Continuous versus incontinuous diel sampling was not explained in the methods. Response: A continuous diel sampling on CO2 efflux was conducted before the last sampling campaign. Besides, incontinuous sampling for the diurnal variation in fluxes were also conducted in the riverine sites during the first sampling campaign in January. The information will be added into the Sampling section.

GUO, J., JIANG, T., LI, Z., CHEN, Y. & SUN, Z. 2011. Analysis on partial pressure of CO2 and influencing factors during spring phytoplankton bloom in the backwater area of Xiaojiang River in Three Gorges Reservoir. Advances in Water Science, 22, 829-838. PACHECO, F. S., ROLAND, F. & DOWNING, J. A. 2014. Eutrophication reverses whole-lake carbon budgets. INLAND WATERS, 4, 41-48. RAN, J., LIN, C., GUO, J., CHEN, Y. & JIANG, T. 2011. Spatial and temporal variation of carbon dioxide partial pressure over the Xiaojiang River backwater area of the Three Gorges Reservoir. Resource and Environment in the Yangtze Basin, 20, 976-982. SUMMERFIELD, M. A. 1991. Global geomorphology: an introduction to the study of landforms, Harlow, Essex, England;New York;, Longman Scientific & Technical. TAO, F. 2017. Air–water CO2 flux in an algae bloom year for Lake Hongfeng, Southwest China: implications for the carbon cycle of global inland waters. Acta Geochimica, 36, 658-666. ZHAO, Y., ZENG, Y., WU, B. F., WANG, Q., YUAN, C. & XU, Z. 2011. Observation on greenhouse gas emissions from Xiangxi River in Three Gorges Region. Advances in Water Science, 22, 546-553.

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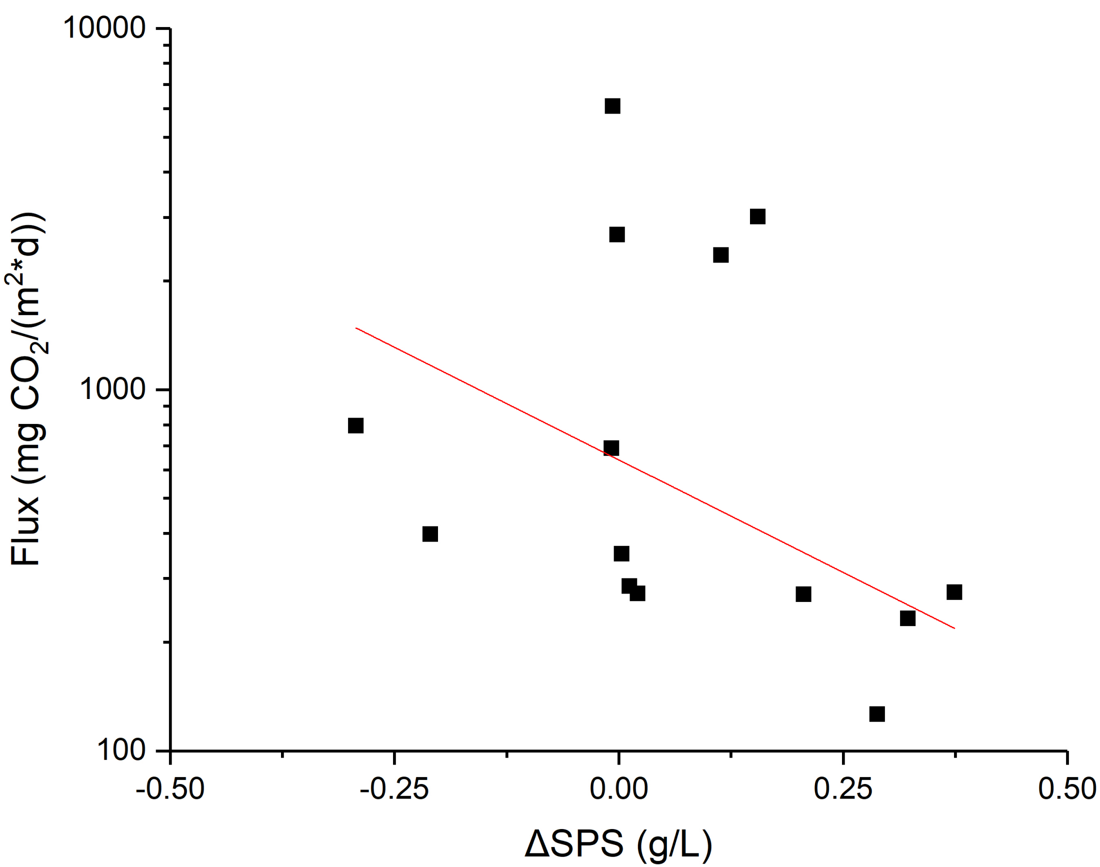

Fig. 1. The scatter plots showing the relation between CO2 flux and the difference in SPS concentration between riverine sites and reservoir surface at Point R1 (mainstem)

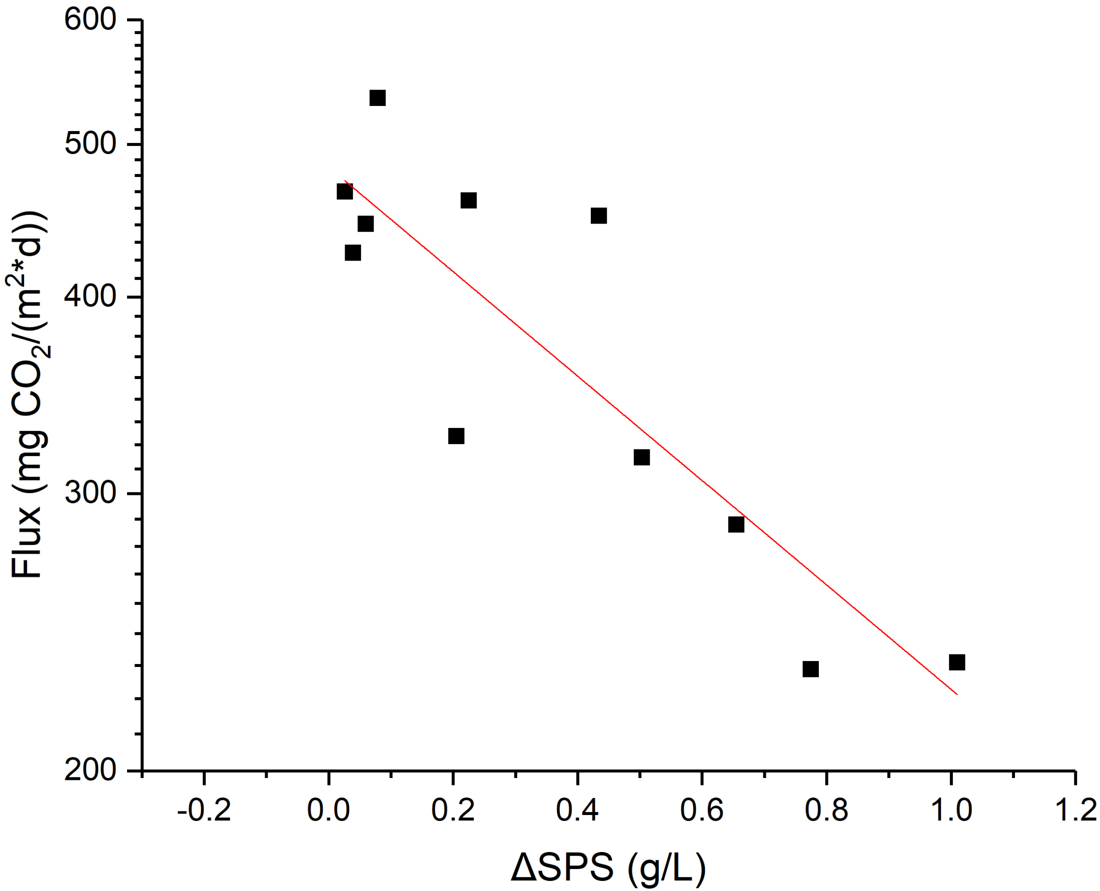

Fig. 2. The scatter plots showing the relation between CO2 flux and the difference in SPS concentration between riverine sites and reservoir surface at Point R2 (tributary)

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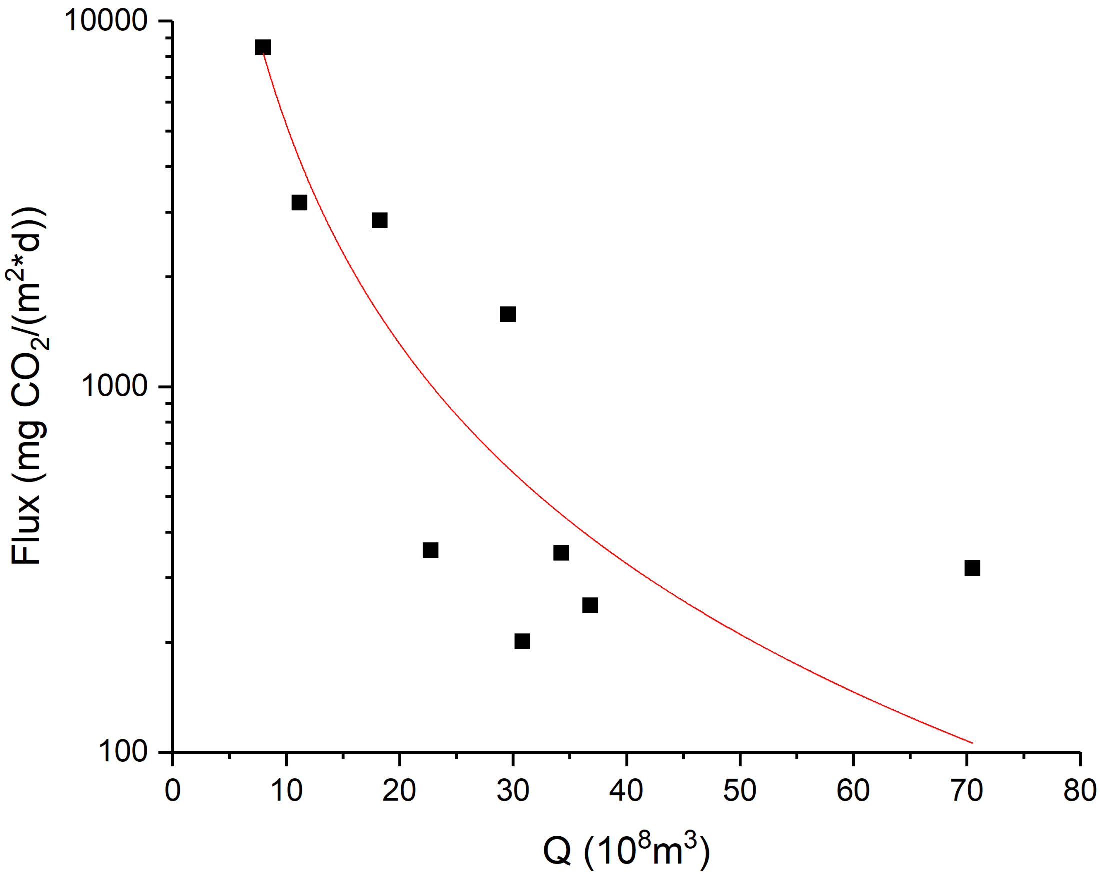

Fig. 3. The scatter plots showing the relation between water discharge and CO2 flux at the Point R1(mainstem)

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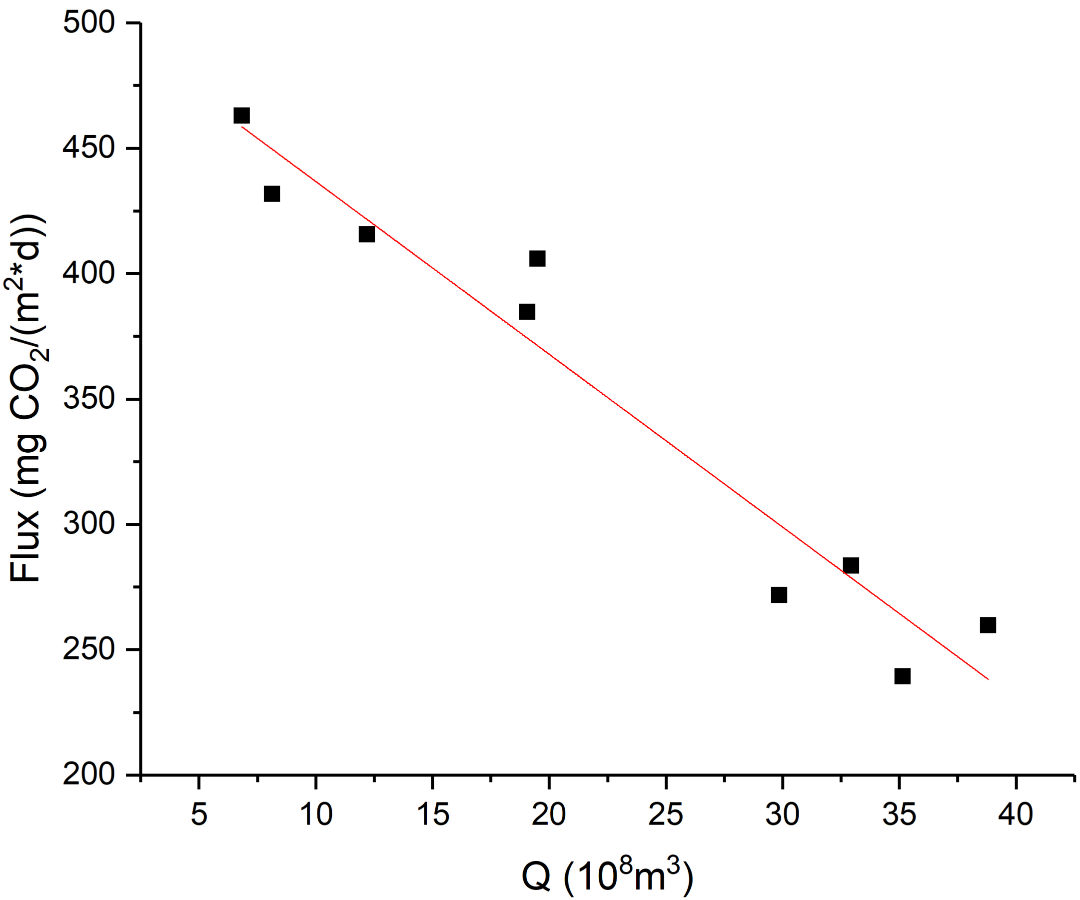

Fig. 4. The scatter plots showing the relation between water discharge and CO2 flux at the Point D (downstream)

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