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Interactive comment on "Effects of flooding on organic carbon consumption in the East China Sea" *by* C.-C. Chen et al.

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Responses to reviewers' comments on ms no: bg-2015-106 "Effects of flooding on organic carbon consumption in the East China Sea" (Chen, Gong, Chou, Chung, Shiah, and Chiang)

Referee #1 (Prof. T.C.M. Malone) General comments Comment 1. It is important to distinguish between biomass-limitation (as indicated by [Chl]) and growth rate limitation (as indicated by PP/Chl). Please clarify. Response: Dear Tom, Thank you so much for thoroughly reviewing our manuscript and providing valuable and constructive comments. Further more, we also thank you for the editing suggestion in the supplement, and it has been revised accordingly and/or response in our reply. The comments, yours and that of other reviewers', do inspire us to re-dig into and analyze our data set and

re-construct our ways of presentation. In this revision, a substantial amount of figure (e.g., Fig. 1), table, and text have been modified and a new table (e.g., Table 2) and figure (e.g., Fig. 6) have been added. Please refer to the supplement for the revised version where most of the revised text or materials were marked in red. Overall, the revised version has been significantly improved in results and discussion, as well as conclusion according to yours and that of other reviewers' comments. Thank you for the reminder. In this study, most of cases were more related to biomass-limitation, and it has been clarified as much as possible in this revision. In addition, as your suggestion, the growth rate limitation has also been provided and compared to observed values in summer of the ECS from our previous study (please refer to our response to your Comment 9).

Given the known importance of fluvial (allochthonous) inputs of organic matter (dissolved and particulate) as substrates for CR, the focus on the fluvial input of dissolved inorganic nutrients is unfortunate. To address the "Effects of flooding on organic carbon consumption" measurements of suspended sediments, dissolved organic matter and particulate organic matter should have been made. Response: We do agree with you that it is important to measure suspended sediment, dissolved and particulate organic matters if the issue is focused on "Effects of flooding on organic carbon consumption". Unfortunately, these variables were not measured and they could not be repeated since this was a field study. However, this comment do inspire us to re-dig into our data set and reconstruct ways of our presentation in this revision. To compensate this flaw, hopefully, transparency data recorded from CTD was used and it could represent as an integrated index of suspended sediments, particulate and dissolved organic matters. Surprisingly, transparency data showed that its value (80.5%) in the 2010 flood was similar to or only slightly lower than averaged value (81.9%) observed over the past six years' measurement (2003-2008) in summer of the ECS. Even though, this result was not as our expectation. This could be partially explained by that most large particulates from terrestrial source might be confined to and precipitated in the coastal region, not in the expanded CDW zone in the 2010 flood. Furthermore, it should also

be noted that the sampling period of 2010, even at the peak of the flood, was almost one month later since the beginning of this flood (Please refer to ours response to your Comment 4 for details). Therefore, it is reasonable to speculate that plankton communities was in the late phase of succession of this flood event. The transparency during the sampling period of 2010 might increase due to organic matters (particulate and dissolved) was however uptake and transferred to higher trophic levels. This assumption could be indirectly evidenced from high zooplankton biomass (105.7 mg C m-3) measured in this flood period. Hopefully, these additional data, transparency and zooplankton biomass, can strength and prove our argument in this revision. For other details, please also refer to the revised text.

Adjectives like “huge” and “tremendous” are used too frequently and should be dropped in favor of more quantitative terms. Surely they can estimate the actual riverine inputs? Response: Thank you for pointing out the repeated usage of those adjectives. Be honestly, this was probably the most difficult comment for us to response. To estimate the actual riverine input, more data are needed which are not available at hand. Therefore, we have dropped most of them in this revision. Hopefully, this is acceptable.

Overall conclusions: The most likely data-based scenario is that phytoplankton production during the period of observation was light limited due to fluvial inputs of suspended matter and CDOM and that the increase in CR was primarily caused by an increase in allochthonous inputs of organic matter (dissolved and particulate). Response: As your statement, the typical scenario of flooding effect on organic carbon consumption might be briefly summary as follows, a). During the flood, large amount of allochthonous organic matter (dissolved and particulate) and dissolved inorganic nutrients accompanied with freshwater discharged into shelf ecosystems, which in turn fuels the growth of phytoplankton, bacterioplankton and other plankton communities; b). In the meantime, the CR rate increases due to increasing of plankton communities; and c). The system becomes more heterotrophic since fluvial input of organic matter was consumed and stimulated the growth of heterotrophic bacterioplankton in this system. This sce-

nario might be still hold true at certain period of this flood event. However, in the study period of 2010, even though the CR rate was high, but with low biomass of bacterioplankton. The high CR rate observed in the 2010 flood might be mostly contributed by phytoplankton and zooplankton. This assumption could be indirectly supported by that biomass (per carbon unit) of phytoplankton and zooplankton accounted for 45.6% and 43.1% total plankton biomass (i.e., summed biomass of phytoplankton, bacterioplankton, and zooplankton) in 2010. This result also suggests that, at the sampling period of 2010, the succession of plankton communities might be at the late phase of this flood event. Please refer to our response to your Comment 14 on this related issue for details.

Specific comments: Comment 2. Primary production, p. 6, lines 18-20: What time of day were the samples collected and incubated? Diurnal periodicity is an important factor here, especially when using short-term incubations (2 hours) to estimate daily rates. Response: Yes, you are right that the PP value might vary with time of day of incubation. This fact has been proved by previous publication (e.g., MacCaull and Platt 1977). The diel variation of PP incubation has also been awareness since the beginning of this project (2000). In this study, all of PP samples were collected and incubated roughly between 0800 to 1300 (sunrise 0500; sunset 1830). To clarify, the sentence has been slightly modified and became as “The samples were collected and incubated from three depths within ZE at stations occupied during daylight” in this revision. MacCaull, W. A. and T. Platt. (1977) Diel variations in photosynthetic parameters of coastal marine phytoplankton. L&O, 22(4):723-731.

Comment 3. p. 7, line 17: How can a 2001 publication provide data “over the last decade” (2000-2010)? Response: Thank you for the reminder. In this revision, the website address of data source has been used as a reference.

Comment 4. p. 8, lines 1-2: When were the stations occupied (samples collected) relative to the peak in river flow? Response: Thank you for pointing out the importance of sampling time period relative to this flood event. It allows us to re-evaluate the

potential of succession phase of plankton communities at the sampling period. Based on the river flow data, the sampling period was at the peak of flood event in 2010, and this flood started about one month prior to sampling. To remind the reader, the starting time of this event has been added into this revision. For your reference, the daily averaged flow rate at Datong hydrostation for both 2009 and 2010 were shown in Fig. 9. The shade areas indicated the sampling periods, and the horizontal dashed lines were the suggested flood criteria (4x104 m3 s-1) for the Changjiang River. Please also note the Datong station was the seaward-most station of the Changjiang River, the location of which might produce a time lag of water transport between flow observation and river plume discharge. To save the text, we prefer not to present this figure in revision, unless you do feel that it is necessary.

Comment 5. p. 9, lines 17-19: In regard to phosphate limitation, please include the distribution of dissolved reactive phosphate in Figure 1 to support this possibility. What about light limitation as suggested above (p. 8, lines 18-19)? Response: Thank you for the valuable suggestion. The distribution of phosphate has been added into Fig. 1 in this revision as suggested. The phosphate distribution has also been described and discussed in the related area of this revision. Overall, the phosphate concentrations in the surface water in the 2010 flood were low with mean$\pm$SD value of 0.17$\pm$0.30 μM, except one station has extremely high concentration of phosphate (1.7 μM; Fig. 1f). The values of nitrate and Chl at this station was 2.1 μM and 1.1 mg Chl m-3, respectively. At this station, phytoplankton biomass seemed not limited by nitrate or phosphate since their concentration were still high. One of potential causes for the low phytoplankton biomass in this station might be due to grazing of zooplankton which has biomass of 123.5 mg C m-3 averaged over the euphotic zone. Except for this station, phytoplankton biomass in the 2010 flood was more likely limited by phosphate, in term of both availability and N:P molar ratio (mean value = 40.4). Please also refer to our response to your Comment 7 for other details. As for potential light limitation, we have added transparency data into Table 1, and detailed discussion and comparison have also been given in this revision. Overall, the transparency for the ECS (80.5%)

and the CDW zone (78.4%) in the 2010 flood were similar to value (81.9%) observed from summer in the ECS over the past six year (2003 – 2008). Based on data, overall, phytoplankton growth seemed more limited by phosphate than light availability in the 2010 flood. Please also refer to revised text and our response to Comment 20 of Referee #2 in this related issue for details.

Comment 6. p. 10, lines 7-8: This is confusing. It is stated above (p. 9, lines 26-27) that concentrations "reached bloom criteria" (> 20 mg/m3). Response: We apology for the confusing. To clarify, previous sentence has been modified and became as “The Chl a concentration in the CDW even reached bloom criteria > 20 mg Chl m-3, historically, in the ECS” as suggested by Referee #2. Hopefully, this can clarify the confusing.

Comment 7. p. 10, lines 8-10: This correlation suggests that biomass was not limited by phosphate concentration. Response: Good point. In response to your comment, we have dug into and re-analyzed our data set. Interestingly, the insignificant linear relationship (p = 0.09) between Chl a and phosphate in the surface water of 2010 was likely caused by one station with low Chl a value (1.1 mg Chl m-3) but high phosphate concentration (1.7 μM; Fig. 1f). The linear regression became statistically significant if this data point excluded from the analysis ($p < 0.001$). Overall, the phosphate concentration in the surface water of the 2010 flood was low with mean$\pm$SD value of 0.17$\pm$0.30 μM, and it was similar to value observed in 2009 (0.13$\pm$0.17 μM). These statements have also been added in this revision. In addition, high N:P molar ratio in the CDW of the 2010 flood was also observed with mean value of 40.4. It all suggests that phosphate might be one of the limited factors for the phytoplankton biomass in the 2010 flood. Hopefully, this can satisfy your inquiry.

Comment 8. p. 10, lines 15-16 (“: : :the phytoplankton biomass in the surface water (Table 1), or average over ZE (data not shown), did not did not differ significantly between 2009 and 2010.”): This is based on concentration (mg/m3). If one defines the CDW zone by the same isohaline (e.g., 31 psu) for each year, what was the total Chl content of the CDW and was there a difference? Response: Thank you for the

constructive suggestion and we like it. To response to your comment, a new Table, i.e., Table 2, has been created which including total biomass of different biological variables over ZE integrated for the ECS and the CDW zone in both 2009 and 2010. This table allows us to further compare the effect of flood on the stock of different variables. As expected, even though the phytoplankton biomass in the surface water or averaged over the ZE was not significant different between 2009 and 2010. The total content of Chl a was much higher either for the ECS or the CDW in 2010 than that of 2009 (please refer to Table 2 for details). The result and discussion of this table has also been added into the related section in this revision.

Comment 9. p. 10, 18-19: How does PP/Chl compare between 2010 and Chen et al. (2009) estimates of the ECS in summer? Response: Thank you for the valuable suggestion. The PP/Chl value has been estimated for the 2010 in this study and Chen et al. (2009) as suggested. The PP/Chl value was higher in the 2010 than that of Chen et al. (2009), with meanïĆśSD values of 27.1ïĆś17.2 and 19.7ïĆś5.5 mg C mg Chl-1 d-1, respectively. This result suggests that phytoplankton in the 2010 might be less limited by its growth rate. The result of this estimation has been included in the revision.

Comment 10. p. 10, line 22: Should this be “abundance” or biomass? Abundance would be measured in terms the number of cells while biomass is measured in terms of mass. Response: We apologize for not making our statement clearly. We do refer to abundance and this was cited from Gong et al. (2011). To clarify, this sentence has been modified and became as “Gong et al. (2011) also showed that the abundance of phytoplankton was twice as high in the CDW than in other regions” in this revision.

Comment 11. p. 13, lines 1-4: As for chlorophyll, how does bacterial biomass integrated over the area of the plume (defined by the 31 psu isohaline) compare? Response: Thank you for the valuable comment. As our response to your Comment 8, a new Table, i.e., Table 2, has been created which included total biomass of different biological variables over ZE integrated for the ECS and the CDW zone in both 2009

and 2010. This table allows us to further compare the effect of flood on the stock of different variables. As expected, even though the bacterial biomass in the surface water or averaged over the ZE was significant lower in 2010 that of 2009. The total bacterial biomass in the CDW zone was two times higher in 2010 than in 2009, with values of 47.7 and 21.0 x 106 kg C, respectively (please refer to Table 2 for details). The result of this table has also been added into the related section in this revision.

Comment 12. p. 14, line 17: It might be "reasonable" to speculate, but not to assume. Response: Yes, you are correct that it is more appropriate using “speculate” in this case. Therefore, this sentence has been slightly modified and became as “Therefore, it is reasonable to speculate that the differences in CR rate in both periods might have been partially caused by variants in the composition of the phytoplankton” in this revision.

Comment 13. p. 14, lines 23-24: A reference for how PP/CR may be interpreted as an index of whether or not the system is autotrophic or heterotrophic should be given here. Also, as for phytoplankton and bacterial biomass, this should be calculated for the CDW as a whole (integrated over the area of the plume as defined by the 31 psu isohaline). Response: Thanks! References for PP/CR have been cited as suggested. A new table, i.e., Table 2, has been created to present the total biomass of biological variables (e.g., phytoplankton, bacterioplankton, and zooplankton) which has been calculated for the ECS and the CDW zone. This table provides another viewpoint to examine how effect of flooding on response of total biomass in variant biological variables in the CDW zone.

Comment 14. p. 15, lines 3-10: This is most likely and is contrary to the conclusion above that CR is controlled by phytoplankton production. The conclusion that the CDW may be a heterotrophic system is also consistent with large amounts of allochthonous (fluvial) organic matter inputs. If CR were "controlled" by primary production, the system would probably be autotrophic. Response: Thank you for pointing out inconsistent of the conclusion that if CR were “controlled” by primary production, the system would probably be autotrophic instead heterotrophic which was observed in this study. To

strength our conclusion, data of zooplankton was added and discussed in this revision. In previous version, those data were not analyzed and presented since zooplankton was only sampled for the whole water column. Therefore, it was not fit into our way of presentation. In this revision, to estimate zooplankton biomass over ZE, the measured value of zooplankton was multiple by a fraction of "ZE to depth of water column" at stations. This estimation should be reasonable even though it have to assume that zooplankton was evenly distributed in the water column. Overall, zooplankton biomass over ZE was significantly higher in the 2010 flood than in the 2009 non-flood, with mean ($\pm$ SD) values of 105.7 ($\pm$ 144.4) and 22.6 ($\pm$ 25.7) mg C m-3, respectively ($p < 0.01$). Further analysis shows that CR was positively significant related to total plankton biomass (per carbon unit) in both periods (please refer to new Fig. 6). It suggests that CR rate was dependent on biomass of total plankton communities. Results also show that autotrophic plankton biomass (i.e., phytoplankton) accounted for 41.3% and 45.6% of total plankton biomass (i.e., summed biomass of phytoplankton, bacterioplankton, and zooplankton) in 2009 and 2010, respectively. As for heterotrophic plankton biomass, bacterioplankton attributed to 38.7% and 11.3% and zooplankton contributed for 20.0% and 43.1% of total plankton biomass in 2009 and 2010, respectively. It suggests that phytoplankton and bacterioplankton might be the most important components attributed to CR in the 2009 non-flood. During the 2010 flood, the CR rate might be mostly contributed by phytoplankton and zooplankton. This might also support and explain why the system in the 2010 flood was heterotrophic and with high CR rate. Please also refer to our response to your Comment 1 for other details in this related issue.

Comment 15. p. 16, lines 12-13: High relative to what? Response: We intend to mean that primary production in 2010 was at the high end of observed values in summer of the ECS. To clarify, this sentence has been modified and became as "Compared to PP observed in summer of the ECS in previous study, primary production was indeed high in the 2010 flood (Table 1; Chen et al., 2009)" in this revision.

Referee #2 General comments: Comment 16. An interesting set of data contributing to a globally relevant issue of increased flooding events and their impact on coastal plankton communities and carbon balance. Considering the topic of flooding, however, not enough is made of potentially major factors such as the impact of increased particulate matter loads and CDOM concentrations on light availability in the coastal water column or of the impact of allochthonous organic carbon inputs on community respiration and metabolic balance. The conclusions of the paper could be strengthened if these factors are considered more. Response: Thank you for agree with this is an interesting set of data contributing to a globally relevant issue of increased flooding events and their impact on coastal plankton communities and carbon balance. We also appreciate for so thoroughly reviewing our manuscript and providing many valuable and constructive suggestions. It do inspire us to re-dig into our data set and reconstruct ways of our presentation. Further more, we also thank you for the editing suggestion in the supplement, and it has been revised accordingly and/or response in our reply. The revised version of this manuscript has been significantly improved in results and discussion, as well as conclusion according to yours and that of other reviewers' comments. To response to your comment, we do agree with you that it is important to understand the impact of suspended sediment, particulate and dissolved organic matter inputs through fluvial discharge on community respiration and metabolic balance in the coastal water column. As you can see, unfortunately, those parameters were measured in this study. To compensate this flaw, transparency data recorded from CTD was used instead and it could be treated as an integrated index of suspended sediment, particulate and dissolved organic matter of the water column. These data set indeed strength our argument and conclusion on how availability of light limited on growth of phytoplankton and serve as a hint for the amount of suspended sediment, particulate and dissolved organic matter in the water column during study periods. Please also refer to our response to your Comment 20 and Comment 1 of Referee #1 for other details.

Specific comments: Comment 17. Primary production method, p.6 lines 20-23: Were

samples always taken at same time of day? Short incubations so important to consider diurnal variability. Response: You are correct that the time of sampling and incubation for primary production might result diurnal variability. To reduce the potential diurnal effect, all the sample for PP was taken and incubated roughly between 8:00 to 13:00 (sunrise 0500; sunset 1830) during the day time. This ambiguous statement has been clarify in this revised version. Please also refer to our repose to Comment 2 of Referee #1 for other details.

Comment 18. p.7 line 17: Reference from 2001 can't refer to the last decade of data if data set reported is from 2009 and 2010. Response: Thank you for the correction. It has been replaced by the website address of data source for reference in this revision.

Comment 19. Wording, p.8 line 12: Maybe clarify at times when referring to values from previous studies as can get confusing to reader at times. Response: Thanks! To avoid confusing, hopefully, data of observed values from previous study was provided in the revision.

Comment 20. p.8 lines 18-19: "suggests that the growth of phytoplankton might be limited by the availability of light". Data on suspended particulate matter, CDOM or turbidity may help reinforce this statement and whole argument could be expanded upon more. Response: Thank you for the constructive comment. This suggestion do help us a lot to re-dig into our data set and inspire our ways of thinking about data presentation in this revision. Even though we do not have POC and DOC data in this study, we do have transparency data recorded from CTD. This data has been analyzed and included into Table 1 in this revision. It also has been compared to results in summer of the ECS over the past six years (2003-2008; unpublished data) of our study. Interestingly, the averaged transparency was the lowest in the CDW zone of 2009 compared to all the other data observed in the ECS. It suggested that the growth of phytoplankton in the CDW zone in 2009 might be limited by the availability of light. During the 2010 flood, the transparency was, however, similar to the value observed over the past six years in the ECS. This could be partially explained by that most large particulates from ter-

restrial source might be confined to and precipitated in the coastal region, not in the expanded CDW zone in the 2010 flood. Furthermore, it should also be noted that the sampling period of 2010, even at the peak of the flood, was almost one month late since the beginning of this flood (Please refer to our response to Comment 4 of Referee #1 for details). Therefore, it is reasonable to speculate that plankton communities was in the late phase of succession of this flood event. The transparency during the sampling period of 2010 might increase due to organic matters (particulate and dissolved) was however uptake and transferred to higher trophic levels. This assumption could be indirectly evidenced from higher zooplankton biomass measured in this flood period. Please also refer to our response to Comment 1 of Referee #1 for similar issue. For other details, please refer to the revised text of this revision.

Comment 21. p.9 line 18: Adding a contour plot of phosphate to Fig. 1 would help reader interpret the nutrient dynamics in the two years of study. Response: As suggested, contour plots of phosphate have been added into Fig. 1 in this revision. It indeed help a lot to interpret the nutrient dynamics in the 2009 non-flood and the 2010 flood. Please also refer to ours response to Comment 5 of Referee #1 for other details.

Comment 22. Wording, p.9 line 27: Initially unclear that this is referring to a previous study. Response: Thank you. We like you suggestion and it help to clarify this ambiguous statement. This has been changed in this revision as suggested.

Comment 23. p.10 line 9: These correlations suggest that biomass is not limited by phosphate at this time. Response: Yes, you are correct that the insignificant relationship between Chl a value and phosphate concentration suggest that biomass was not limited by phosphate at this period. After re-analyzed our data, the significant relationship was observed between Chl a and phosphate in the surface water in 2010 if excluded one data point with exceptional high phosphate concentration from this analysis (please refer to Fig. 1f). In addition, the averaged phosphate concentration in the surface water of the 2010 flood was low with mean$\pm$SD value of 0.17$\pm$0.30 μM, and it was similar to value observed in 2009 (0.13$\pm$0.17 μM). The N:P molar ratio in the CDW

of the 2010 flood was also high with mean value of 40.4. It all suggests that phosphate might be one of the limited factors for the phytoplankton biomass in the 2010 flood. Please also refer to ours response to Comment 7 of Referee #1 for other details.

Comment 24. p.12 lines 7-9: Wording confusing. Was cyanobacteria present in the CDW in 2010 or not? Response: Thank you for so thoroughly reviewing our manuscript and pointing out the typos. We intend to mean that the pattern of predominant (e.g., >70%) by cyanobacteria as that of in 2009 was observed only at stations located in region other than the CDW zone in the 2010 flood. During the 2010 flood, in addition to cyanobacteria, the dominant taxa of bacterioplankton in the CDW zone also included favobacteria, gammabacteria, alphabacteria, and actinobacteria. The statement has been modified in this revision. Hopefully, it can clarify this confusing.

Comment 25. p.13 lines 18-24: Could the CR:PP regression and relatively steep slope suggest that allochthonous organic carbon inputs are fuelling a higher CR rate for the same PP rate than in other regions? Response: This is an interesting question. The steep slope of CR:PP regression suggests that the CR rate might have been more dependent on in suit organic carbon production (e.g., PP) when compared to results observed from the same area but different time periods. For the same PP (if standardized by Chl a), the relatively steep slope of CR:PP regression might suggest that a higher CR is more fueling by allochthonous organic carbon inputs. Based on our result that PP to Chl a was relatively higher compared to results observed in summer in the ECS (please refer to our response to Comment 9 of Referee #1 for details), and it suggests that the CR rate might have been more dependent on in suit organic carbon production in the 2010 flood. Hopefully, our response is reasonable and can satisfy your inquiry.

Comment 26. p.14 lines 16-17: “phytoplankton assemblage varied between both periods”. Earlier (p.10 lines 25-26) it is stated that phytoplankton was not identified or enumerated in 2009 and an assumption of potential community composition is made based on other findings. The next sentence goes on to “assume” based on these dif-

ferences in community composition. Suggest some rewording as these conclusions are a little tenuous. Response: Yes, we agree with you that it might be “reasonable” to speculate, but not to assume. Therefore, this sentence has been slightly modified and became as “Therefore, it is reasonable to speculate that the differences in CR rate in both periods might have been partially caused by variants in the composition of the phytoplankton” in this revision.

Comment 27. p14 line 24: a reference could help here. Response: Thanks! The references has been cited as suggested.

Comment 28. p15 lines 3-7: Expand upon this argument as this is a more likely scenario than the previously proposed control of CR by in situ PP considering the volume of riverine discharge and the potential for DOM within this discharge to be more bioavailable as flooding will have minimised the amount of time it has spent being reworked by microbes in the soil. Response: Your guess might be correct on that the potential for DOM within this discharge to be more bioavailable as flooding will have minimized the amount of time it has spent being reworked by microbes in the soil. In this study, this flood started about one month prior to our study (Please refer to our response to Comment 4 of Referee #1 for details). Based on our results, it shows that transparency of the 2010 flood was similar to or only slightly lower than values observed in summer in the ECS over the last six years’ measurement (Please refer to our response to Comment 4 of Referee #1 and your Comment 20 for details). In addition, biomass of bacterioplankton and zooplankton were low and high in the study period of the 2010 flood, respectively (Please refer to our response to Comment 14 of Referee #1 for details). It all suggest that plankton communities might be in the late phase of succession at the study period of this flood. Furthermore, the CR rate of the 2010 flood might be more attributed to phytoplankton and zooplankton based on proportion of phytoplankton or zooplankton biomass to total plankton biomass (i.e., summed biomass of phytoplankton, bacterioplankton, and zooplankton). Please also refer to our response to Comment 1 and 14 of Referee #1 for further details. We appreciate that your com-

ments indeed improved our results and strength our discussion and conclusion in this revision.

Referee #3 General comments: Comment 29. This paper studies the influence of fluvial discharge on organic carbon consumption, which is an interesting and important objective, especially in the light of the predicted increase in flooding episodes with climate change. However, I have some important concerns about whether the data and approach in this paper allows meeting this objective. Methods report that CR was measured with duplicated samples taken from several depths. From both a practical (loosing one sample means having no replicated measurements) and a statistical point of view, two replicates are far too little to measure plankton CR rates. This is a critical variable for this paper, and precision should be at least clearly indicated. (e.g., the slope of the PP:CR relationship derives from three low CR data whose precision is unknown). Methods should state the volume of samples and sampling depths, as well as time of sampling and temperature gradients during incubations. Response: Thank you for the valuable and constructive suggestions. We also appreciate that you agree with this is an important manuscript on studying the effects of flooding on organic carbon consumption. In this revision, we have taken your and other reviewers' comments very seriously in preparing this revised manuscript. Overall, we feel that the comments were very helpful and they contributed to a greatly improved manuscript. It is a tedious and labor intensity work to perform community respiration measurement in the field. There were about 500 samples (initial + duplicates of incubated samples) have to hand in each period of this study. Based on our previous measurement, our duplicates have high precision (e.g., Chen et al. 2003). Therefore, duplicates, instead of triplicates, samples were incubated in each sampling depth of our incubation. Hopefully, this is understandable. In previous, to save space of the text, we did intend to simplify the method for community respiration since it has been described in our previous studies (e.g., Chen et al. 2003, 2006). As your suggestion, more detailed methods for community respiration are given in this revision (please refer to Material and Methods section for details). To perform respiration incubation, 4 - 6 water samples were collected within

the euphotic zone, at depth intervals of 3, 5, 10, 15, 20, or 25 m. All the samples were incubated in the dark chambers with running surface water. In average, the differences (taken as positive values) in water temperature between incubated chamber and in situ environments where samples collected from were 1.33 $\pm$ 0.81 and 2.70 $\pm$ 1.43 âĎČ (mean $\pm$ SD) during each incubation in 2009 and 2010, respectively. Temperature effect on respiration due to incubation was therefore excluded from our estimation. The precision of this method may be indicated by the root-mean square of the difference between the duplicate samples over the course of 2009 and 2010, which are 0.02 and 0.03 mg L-1, respectively. For your reference, the detected limitation for respiration rate, based on our measured method, is about 12 mg C m-3 d-1, when using a respiration quotient (RQ) of 1. Our results showed that there were only 55 out of 371 cases below the detection limit. Those cases, however, were not excluded from our analysis since they may imply that respiration rates were indeed relatively low compared to the other factors measured.

Comment 30. The data set includes inorganic nutrients and chlorophyll a concentrations, heterotrophic bacterial abundance, 14C primary production and O2 community respiration, which might be adequate for a purely descriptive accoount of differences in metabolic balances between flooding and non-flooding situations in the ECS. However, this dataset is insufficient to support the discussion in the paper, based on deriving explanatory hypotheses from regressions. The fact that all these variables change after the flood does not imply causative relations, especially when key controlling factors like inputs of organic matter are excluded from the analyses (see also comment about regional differences below). This leads to some unsustained and contradictory conclusions. E.g., the relationship between Chla and heterotrophic bacterial abundance leads to suggesting that bacterial growth is mainly supported by organic carbon produced locally by the phytoplankton (p. 5619), however neither bacterial growth (only heterotrophic bacteria abundance) nor allochthonous organic matter are measured. This interpretation disagrees with the observation of higher bacterial biomass in the non-flood 2009 when Chla was lower. Such conflict is then resolved by the presumed higher

protozoan grazing in 2010, however neither protozoan grazing nor biomass were measured. This is too speculative, and the hypothesised importance of microzooplankton would contradict the forthcoming hypothesis (p. 5621) that CR rate was dominated by phytoplankton and/or bacterioplankton, which only derives from the slope of the PP:CR relationship. Altogether, the authors defend that CR is explained from the respiratory activity of phytoplankton and/or bacterioplankton, with the bacterioplankton supported by organic carbon locally produced by the phytoplankton. This thoroughly contradicts the observed heterotrophic situation, with an average P/R ratio of 0.42 in 2010, which "implies that a large amount of (allochthonous) organic carbon was respired by the plankton community into the water column during the flooding period." (p.5623). Which in turns thoroughly contradicts the conclusion that "vigorous photosynthetic processes might be a potential cause of the drawdown of huge amounts of fCO2 in the surface water during periods of flooding." (p.5624) (which is only supported by the relationship of fCO2 with Chla, but not with PP). Response: Thank you for so thoroughly reviewing our manuscript and providing such valuable and constructive comments. Based on our previous presentation, we do agree with you that there were some missing links among results, discussion, and conclusion. During the flood, generally, large amount of allochthonous organic matter (dissolved and particulate) and dissolved inorganic nutrients accompanied with freshwater discharge into shelf ecosystems, which in turn fuels the growth of phytoplankton, bacterioplankton and other plankton communities. Therefore, it is reasonable to assume that the system becomes more heterotrophic since fluvial input of organic matter was consumed and stimulated the growth heterotrophic bacterioplankton. High CR rate was indeed observed in the 2010 flood, but with low bacterial biomass and only slightly higher Chl a concentration when compared to that in the 2009 non-flood. Based on biomass of bacterioplankton and Chl a, it seemed not enough to support high CR rate observed during this flooding period. In this revision, data of zooplankton biomass was added and analyzed (please refer to our response to Comment 14 of Referee #1 for details). Overall, zooplankton biomass over ZE was significantly higher in the 2010 flood than in the 2009 non-flood, with mean ($\pm$ SD)

values of 105.7 ($\pm$ 144.4) and 22.6 ($\pm$ 25.7) mg C m-3, respectively ($p < 0.01$). Further analysis shows that CR was positively significant related to total plankton biomass (per carbon unit) in both periods (please refer to new Fig. 6). It suggests that CR rate was dependent on biomass of total plankton communities. Results also show that autotrophic plankton biomass (i.e., phytoplankton) accounted for 41.3% and 45.6% of total plankton biomass (i.e., summed biomass of phytoplankton, bacterioplankton, and zooplankton) in 2009 and 2010, respectively. As for heterotrophic plankton biomass, bacterioplankton attributed to 38.7% and 11.3% and zooplankton contributed for 20.0% and 43.1% of total plankton biomass in 2009 and 2010, respectively. It suggests that phytoplankton and bacterioplankton might be the most important components contributed to CR in the 2009 non-flood. During the 2010 flood, the CR rate might be mostly contributed by phytoplankton and zooplankton. These results might also support and explain why the system in the 2010 flood was heterotrophic and with high CR rate. As for inputs of organic matters, unfortunately, these variables were not measured and they could not be repeated since this was a field study. However, this comment do inspire us to re-dig into our data set and reconstruct ways of our presentation in this revision. To compensate this flaw, hopefully, transparency data recorded from CTD was used and it could treat as an integrated index of suspended sediments, particulate and dissolved organic matters of water column. Surprisingly, transparency data showed that its value (80.5%) in the 2010 flood was similar to or only slightly lower than averaged value (81.9%) observed over the past six years' measurement (2003-2008) in summer of the ECS. Even though, this result was not as our expectation. This could be partially explained by that most large particulates from terrestrial source might be confined to and precipitated in the coastal region, not in the expanded CDW zone in the 2010 flood. Furthermore, it should also be noted that the sampling period of 2010, even at the peak of the flood, was almost one month late since the beginning of this flood (Please refer to our response to Comment 4 of Referee #1 for details). Therefore, it is reasonable to speculate that plankton communities was in the late phase of succession of this flood event. The transparency during the sampling period of 2010 might

increase due to organic matters (particulate and dissolved) was however uptake and transferred to higher trophic levels. This assumption could be indirectly evidenced from high zooplankton biomass (105.7 mg C m-3) measured in this flood period. Hopefully, these additional data, transparency and zooplankton biomass, can strength and prove our argument in this revision. For other details, please also refer to the revised text of this revision.

Comment 31. The comparison of variables between the averages of 2009 and 2010 (Table 1) is difficult because important spatial differences exist each year. These imply large variances in the annual averages and that differences may be non significant (e.g., the discussion about which nutrient controls PP each year is based on mean N/P molar ratios with SD of aprox. 20). As the region influenced by the river is much larger in 2010, it is difficult to know if the differences between mean annual rates result from differences in composition and functioning within this region or from the differences in the total area affected. I would suggest a regionalised analysis based on comparison of data in comparable oceanographic conditions, e.g., areas influenced by the river discharge under flood and non-flood conditions, and then to scale the conclusions to the respective areas affected. Response: Thank you for the constructive suggestion. In this revision, Table 1 has been modified and estimated values for the CDW zone was added. It allows us to examine whether the difference between 2009 and 2010 was caused by areas influenced by the flooding effect. In addition, contour plot of phosphate in the surface water has also been added into Fig. 1 in this revision. This provides us to understand the spatial variation between the flood and the non-flood periods. Overall, the phosphate concentration in the surface water of the 2010 flood was low with mean$\pm$SD value of 0.17$\pm$0.30 μM, and it was similar to value observed in 2009 (0.13$\pm$0.17 μM). In addition, high N:P molar ratio in the CDW of the 2010 flood was also observed with mean value of 40.4. It all suggests that phosphate might be one of the limited factors for the phytoplankton biomass in the 2010 flood. Please also refer to our response to Comment 5 and 7 of Referee #1 and Comment 23 of Referee #2 for other details. As your suggestion, a new table (Table 2), has also

been created to present the total biomass of biological variables (e.g., phytoplankton, bacterioplankton, and zooplankton) which has been calculated for the ECS and the CDW zone. This table provides another viewpoint to examine how effect of flooding on response of total biomass in variant biological variables in the CDW zone. Hopefully, you do find our response and revising of the text has reasonably answer all your concerns.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C3328/2015/bgd-12-C3328-2015-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 12, 5609, 2015.

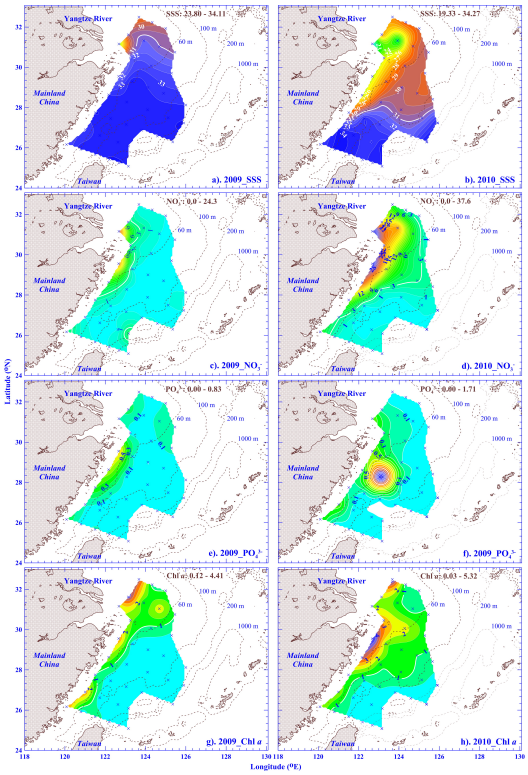
Yangtze River
Mainland China
Taiwan
a). 2009_SSS
b). 2010_SSS
c). 2009_NO3-
d). 2010_NO3-
e). 2009_PO43-
f). 2009_PO43-
g). 2009_Chl a
h). 2010_Chl a
Latitude (0N)
Longitude (0E)

Fig. 1.

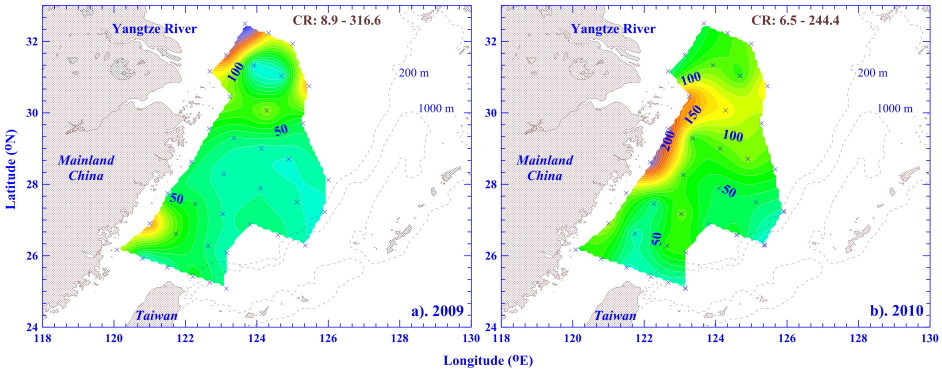
Yangtze River
CR: 8.9 - 316.6
Mainland China
Taiwan
200 m
1000 m
a). 2009
CR: 6.5 - 244.4
b). 2010
Latitude (0N)
Longitude (0E)

Fig. 2.

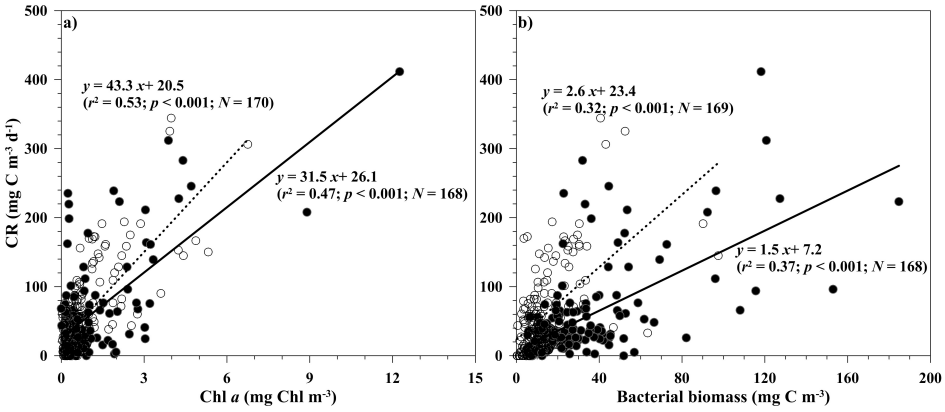
a)
y = 43.3 x+ 20.5
(r² = 0.53; p < 0.001; N = 170)
y = 31.5 x+ 26.1
(r² = 0.47; p < 0.001; N = 168)
CR (mg C m-3 d-1)
Chl a (mg Chl m-3)
b)
y = 2.6 x+ 23.4
(r² = 0.32; p < 0.001; N = 169)
y = 1.5 x+ 7.2
(r² = 0.37; p < 0.001; N = 168)
Bacterial biomass (mg C m-3)

Fig. 3.

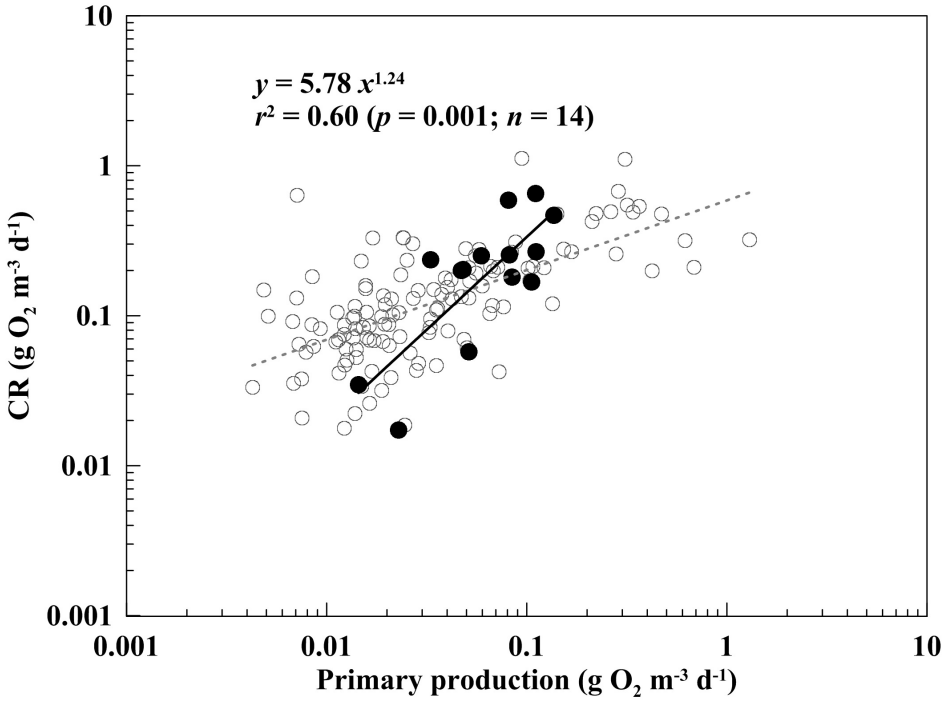
y = 5.78 x1.24
r² = 0.60 (p = 0.001; n = 14)
CR (g O2 m-3 d-1)
Primary production (g O2 m-3 d-1)

Fig. 4.

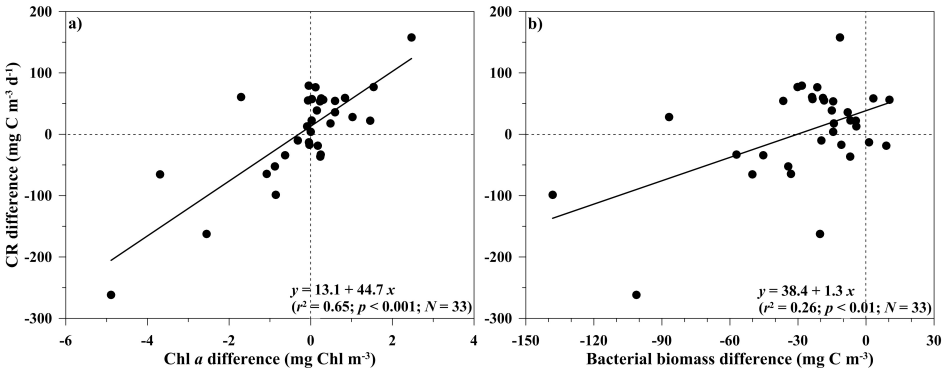
a)
b)
CR difference (mg C m-3 d-1)
y = 13.1 + 44.7 x
(r2 = 0.65; p < 0.001; N = 33)
y = 38.4 + 1.3 x
(r2 = 0.26; p < 0.01; N = 33)
Chl a difference (mg Chl m-3)
Bacterial biomass difference (mg C m-3)

Fig. 5.

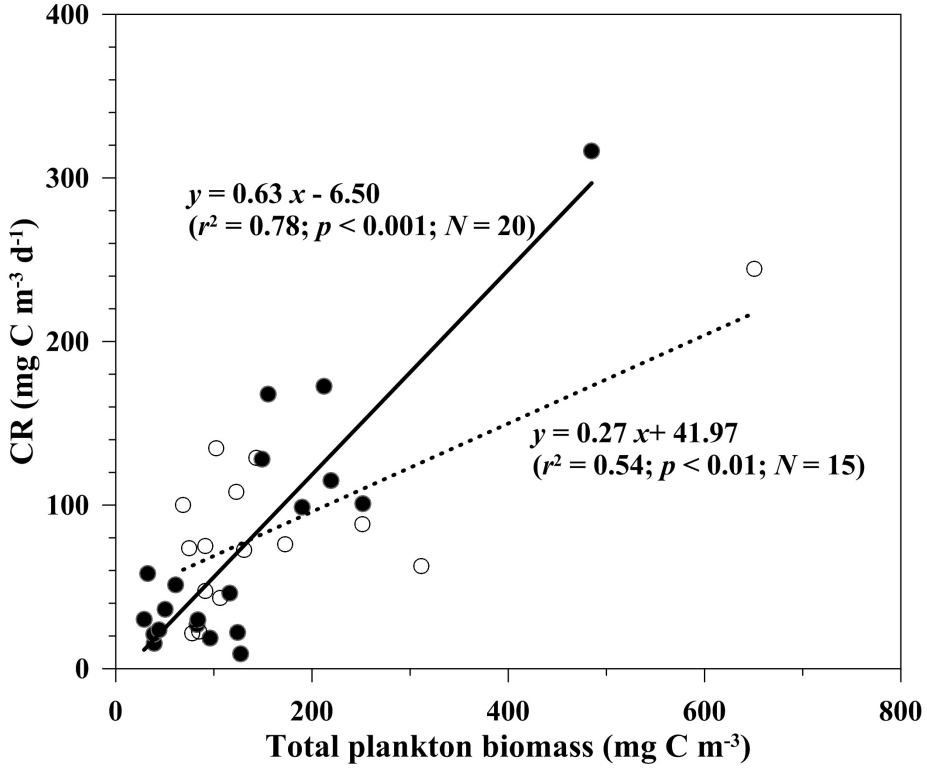
y = 0.63 x - 6.50
(r2 = 0.78; p < 0.001; N = 20)
y = 0.27 x+ 41.97
(r2 = 0.54; p < 0.01; N = 15)
CR (mg C m-3 d-1)
Total plankton biomass (mg C m-3)

Fig. 6.

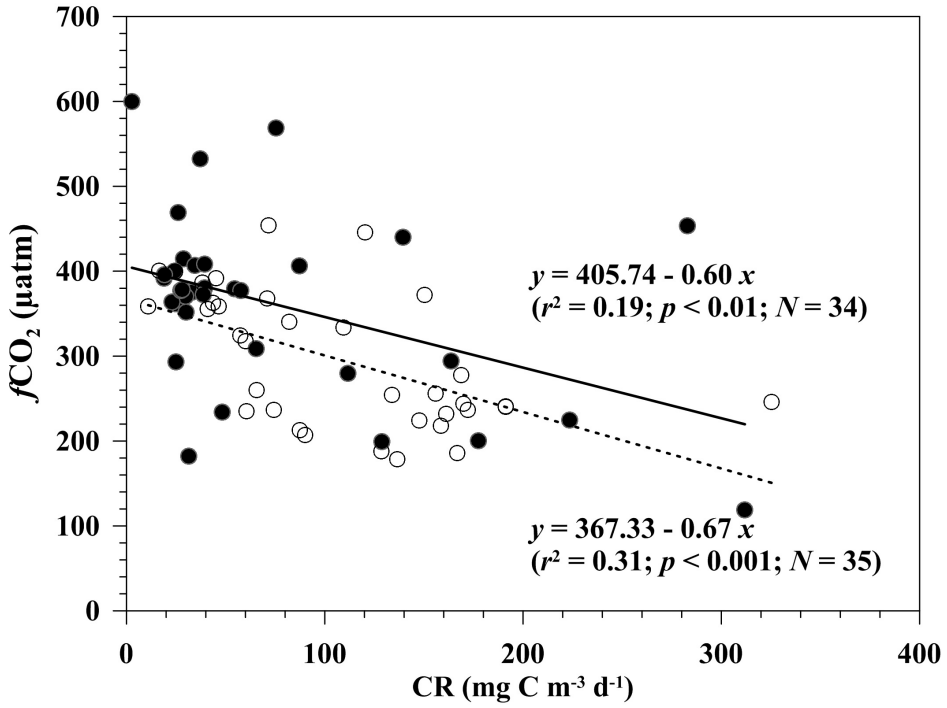
700
600
500
400
300
200
100
0
0
100
200
300
400
fCO2 (µatm)
CR (mg C m-3 d-1)
y = 405.74 - 0.60 x
(r2 = 0.19; p < 0.01; N = 34)
y = 367.33 - 0.67 x
(r2 = 0.31; p < 0.001; N = 35)

Fig. 7.

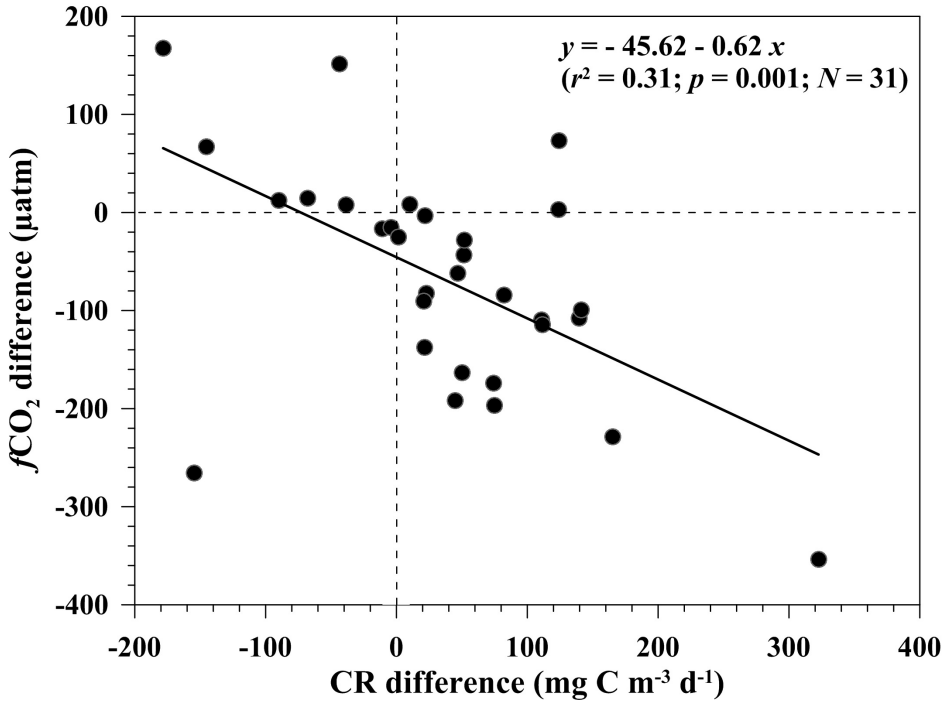
200
100
0
-100
-200
-300
-400
-200
-100
0
100
200
300
400
fCO2 difference (µatm)
CR difference (mg C m-3 d-1)
y = - 45.62 - 0.62 x
(r2 = 0.31; p = 0.001; N = 31)

Fig. 8.

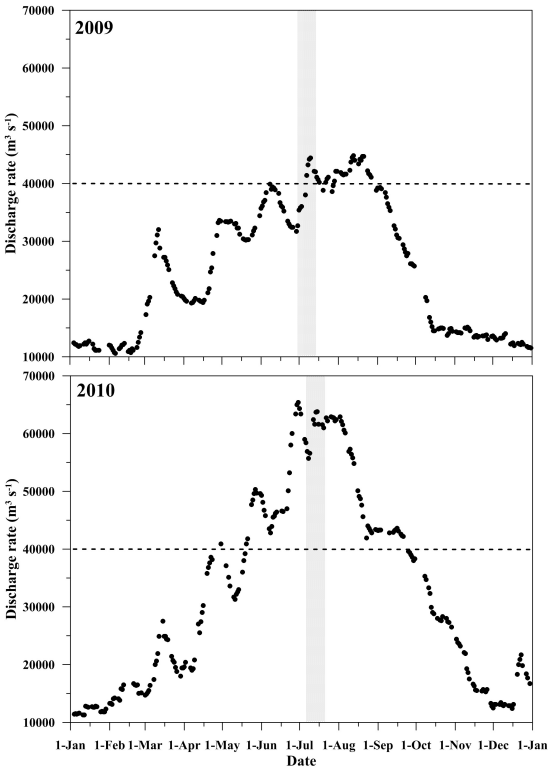
2009
2010
Discharge rate (m³ s⁻¹)
70000
60000
50000
40000
30000
20000
10000
1-Jan
1-Feb
1-Mar
1-Apr
1-May
1-Jun
1-Jul
1-Aug
1-Sep
1-Oct
1-Nov
1-Dec
1-Jan
Date

Fig. 9.