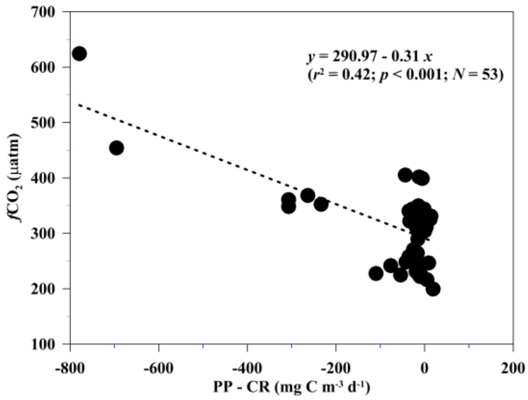
Responses to Reviewers' Comments on Biogeosciences Discuss., 9, 16533–16564, 2012 "The large variation inorganic carbon consumption in spring in the East China Sea" (Chen, Gong, Shiah, Chou, and Hung)

Anonymous Referee #1 (RC C6670)

General comments:

Comment 1. *This paper by Chen et al. addresses an important process of carbon flux, planktonic respiration, in the East China Sea. While this topic itself and the data are certainly important, this paper needs to be substantially fabricated in order to be formally published in the prestigious journal "Biogeosciences". First, in order to related fCO2 with CR, the authors need to consider net community production (NCP), which is the difference between gross primary production (GPP) and CR. While PP was only measured in one cruise, at least the authors can calculate the NCP in that cruise to see how it related with fCO2 in that cruise.*

Thank you for many valuable and constructive suggestions, and we also appreciate that you agree that this manuscript provides important data and interpretation. As you suggested, we have taken your and the other reviewers' comments very seriously in preparing this revision. Please refer to our detailed responses to your and reviewers' comments.



Yes, we agree with you that *f*CO_2 should be more directly related to net community production if any. To response your inquiry, we used the data of measured PP and CR from each incubated depth in 2010 for further analysis, and the result was shown as figure above. The *f*CO_2 values were indeed significantly decreased with NCP increasing as your expectation. However, this significant trend was mainly resulting from two smaller NCP data points observed at deep waters (20 and 30m) of St. 19A. This relationship was not statistical significance if those two data points excluded from this analysis. This result suggests that *f*CO_2 might be not regulated by NCP in this low primary production period. Similar conclusion could also be derived from the

relationship between $f\mathrm{CO_2}$ and CR in 2010 (please refer to our reply to your comment 26 for details). Even so, the result between $f\mathrm{CO_2}$ and NCP was indeed important and it was now included in the revision (Fig.8b).
In addition, we appreciate your valuable and construction comments, especially this and comment 26, that inspire us to deeply dig into our data set again and re-think through our way of presentation. Those comments were also very helpful for us to re-confirm the logistic of our conclusions. we also thank you for going into this ms thoroughly to correct our typos and provide valuable comments, and it has significantly improve the quality of this ms. After substantial revision following your and other reviewers' comments, we are confident that this manuscript is now suitable for publication in "*Biogeosciences*".

Comment 2. *Second, this paper teems with simple comparisons and (linear) regressions (P.S., I wonder why the authors always stick to the linear regression. There are no prior reasons for the use of a linear regression). At least the authors should give the specific statistical method in the 'Materials and methods' section. Then the authors need to convince the readers these statistical techniques are used appropriately. For example, for an ordinary linear regression, the normal distribution of the error terms needs to be satisfied. Also, to use a t-test, the assumptions of the normal distributions of the two variables and identical variance need to be satisfied.*

The statistical methods for analyses were given in the "Materials and Methods" section in the revision as suggested. Yes, you feel right that we do favor to use simple linear regression to explore the relationship between variables since it is much easier for the reader to follow if there was any correlation. We do understand that there were some relationships for variables in the shelf ecosystem might not appropriate expressed by a linear regression. For example, non-conservative decreases in the nutrient-salinity relationship observed along the river/ocean mixing gradient in the ECS were attributed to biological uptake (Tian et al., 1993). Similar pattern was also shown in our previous study, i.e., surface nitrate showed a significant negative exponential decrease in relationship to SSS, especially in the salinity range of 29 to 32 (Fig. 5 in Chen et al. 2009). Moreover, some data set has been log transform prior to perform linear regression or expressed as power function (e.g., CR vs. PP; Chen et al., 2009) since those data have huge variation due to measured from wide range of spatiotemporal scale (del Giorgio and William, 2005). Our data set were however limited to same cruise in the ECS with

relative smaller variation. Therefore, a simple linear regression was used in most of cases in this study, and multiple linear regression was applied in some cases if needed. As for group comparison (e.g., Table 1), we chose using Mann-Whitney Rank Sum test which does not make any assumptions about normality or equal variance, and is more sensitive when data may not have been taken from normal populations with equal variances.

Tian, R. C., F. X. Hu, and J. M. Martin (1993), Summer nutrient fronts in the Changjiang (Yangtze River) Estuary, *Estuar. Coast. Shelf Sci.*, *37*, 27-41.

del Giorgio, P. A. and Williams, P. J. le B. (eds.) (2005), Respiration in aquatic ecosystems. Oxford University Press, New York, 315pp.

Comment 3. *Third, there are a number of grammar errors (I list a few below) which are detrimental to the quality of the paper. The authors should ask a native English writer for help on this.*

We sincerely appreciate you for pointing out the typos and grammar errors of this ms, and they have been revised accordingly. In addition, we also have asked a native English speaker to do a thorough editing to improve our manuscript. In general, we are confident that this manuscript is now suitable for publication in "*Biogeosciences*".

Comment 4. *The last, I strongly suggest the authors to upload their data as a supplemental file for a better interpretation and usage of these data by the scientific community (This is NOT a criticism).*

Thank you for the valuable suggestion. We agree with you that it is important to provide data for further usage by the scientific community. Therefore, we have uploaded part of data as a supplemental file including, the sampling locations, SSS, SST, and averaged values of Chl *a*, nitrate, phosphate and CR over the euphotic zone. In addition, the range and mean±SD values of data were also shown in Table 1 for reference. We hope that this will have some contribution for understanding the ECS shelf ecosystem in the future studies.

Specific comments:

Comment 5. *P. 16534, Line 17, should be 'led'.*

It has been changed as suggested. We are also grateful that you went into this manuscript thoroughly, and providing valuable, constructive suggestions and corrections.

Comment 6. *p. 16535, line 4, 'when it comes to determine: : :'*

It has been changed as suggested.

Comment 7. *p. 16536, line 1, 'released through: : :'*

It has been changed as suggested.

Comment 8. *p. 16538, should provide how much volume were filtered for Chl and POC analysis and whether there were any replications.*

The filtrated volume for Chl *a* and POC was 2 liters each, and it has been indicated in this revision. There was no replicated sample for both variables due to large filtrated volume and tight cruise schedule.

Comment 9. *p. 16539, line 4, are duplicates sufficient*

To response your concern, coefficient of variation (CV) for CR in both 2009 and 2010 were calculated and they were 9% and 13%, respectively. The CV was small, and it suggests that duplicates should be fine. In addition, we did try to do incubation as many as possible, for example there were more than 300 incubations in each cruise. Therefore, we have to trade off between duplicates and sampling stations.

Comment 10. *p. 16540, line 19, what do you mean by 'dilute'?*

This might be not the accurate word to address it, and what we do mean is the mixture of seawater and freshwater. As matter of fact, it is really just the amount of freshwater discharge into the East China Sea. To clarify it, we therefore simply use 'fresh water' instead of 'dilute water' in the revision.

Comment 11. *p. 16540, line 24, add 'maps' after 'contour'*

It has been changed as suggested.

Comment 12. *p. 16540, line 28, change 'negatively linear regressed with' to 'negatively correlated with'.*

It has been changed as suggested.

Comment 13. *p. 16541, line 3-4, rephrase this sentence. This is basically mixing of riverine water with the oceanic water. The word 'dilute' is not appropriate here.*

This sentence has been rephrased, and it became to "These results indicate that nutrients were mixing between riverine water and oceanic water with fluvial runoff as a major nutrient source in the ECS surface plume ecosystem" in the revision.

Comment 14. *Line 8, give the detailed statistical method for comparison. Also for line 10.*

As your suggestion, the statistical method for comparison was given in the method section, and it was also indicated in the table's legend in the revision.

Comment 15. *Line 8-10, since it is not statistically significant, revise the sentence to avoid any comparison.*

Thank you for the suggestion. This sentence has been modified and it became to "The mean value of average nitrate concentration over the euphotic zone in 2009 and 2010 were 3.6 μM and 10.1 μM, respectively (*p* = 0.14; Table 1)" in this revision.

Comment 16. *Line 13-15, it is not clear to me what you mean here. Please be more specific.*

We meant that growth of phytoplankton, bacterioplankton, as well as other planktonic communities might response differently following the intrusion of riverine waters. To clarify it, this sentence has been slightly modified and it became as "This implies that sequential biological responses, e.g., growth of various planktonic communities, may behave differently following the intrusion of riverine waters enriched with nutrients and organic matter" in the revision.

Comment 17. *p. 16542, Line 18-20, have you ever measured phytoplankton growth rate? Positive correlation between Chl and nutrient concentration does not mean that phytoplankton growth rate is also positively correlated with nutrient concentration.*

We do agree with you that growth of phytoplankton might be regulated by various factors. In this study, we did not have direct evidence to prove how nutrient concentrations regulate phytoplankton growth rate since it has not been measured in this study. Based on our data and analysis, results do suggest that high phytoplankton biomass observed in 2009 was associated with nutrients discharged from the Changjiang River waters. To modify, this sentence has been changed to "These results all suggest that high phytoplankton biomass might be enhanced by dissolved inorganic nutrients, enriched from the intrusion of riverine water into the ECS in this period (Fig. 5)" in the revision.

Comment 18. *Line 26, since the difference is not significant, you cannot say it 'amazingly'.*

You are right. This word is not suitable in this case, and it has been removed in this revision.

Comment 19. *p. 16542, Line 29 - p. 16543, Line 1, the relationship between phytoplankton growth rate and biomass is very complicated, so please do not add too much speculation here. Anyway, it is not the main point of this study.*

Thank you for the valuable and constructive suggestion. We agree that this is not really the main issue of this study, and this has been deleted and combined with next paragraph. In our previous version, we did intend to use available data set to explore why phytoplankton was low in spring of 2010 even there was more available nutrients than that in 2009. In this revision, we have significantly trimmed the text and limited our discussion in this related issue as suggested. (Please also refer to our reply to your comment 21)

Comment 20. *p. 16543, line 8, 'may not limite: : :'*

It has been changed as suggested.

Comment 21. *line 15-23, again, correlations between Chl and environmental variables such as temperature do not equal to the correlations between growth rate (or bacterial production) and temperature. So limit the discussions here. The same applies to the last paragraph in page 16544.*

We agree with you that correlations between Chl *a* and environmental variables are not directly equal to how growth rate of phytoplankton is controlled by that environmental variable. In this case, we only intended to imply that growth of phytoplankton might be limited by cold temperature since we do not have direct evidence to prove it. Based on our data set and analyses, however, we do believe this should be true since the averaged temperature in CDW is much colder in 2010 (mean = 12.2℃) than that in 2009 (mean = 18.0℃). In general, the higher Chl *a* concentration is observed in the CDW region in the East China Sea, and this was also found in the spring of 2009 and in our previous studies during summer. Furthermore, the relationship between Chl *a* and temperature of surface water was significant, but with negative slope in 2009 (Chl *a* = 9.459 - 0.393 * Temp; p = 0.03; not shown in ms). We think this significant relationship was coincident due to high nutrient, low salinity, and low temperature of surface water appeared in the CDW region, and the Chl *a* concentration was more associated with nutrient concentration (but not with temperature) in 2009. In 2010, high nutrient, low salinity, and low temperature of surface water in the CDW was also observed

in the spring of 2010, but there was no high Chl *a* concentration observed in this region or in other area. Even the nutrient concentration was much higher in 2010 than 2009, but no significant relationship was evidence between Chl *a* and nutrients in 2010 (please refer to Fig. 5) which implies phytoplankton growth was limited by other factors instead of nutrients. Among all the available data, the Chl *a* concentration was significantly related to temperature, with positive slope for temperature < 15℃ in 2010 (as shown in Fig. 6). This result suggests that growth of phytoplankton might be limited by cold temperature in this region in the spring of 2010. As for region with SST ≧ 15℃, it might be due to combined effect of temperature and nutrient availability. However, we have omitted this discussion in the revision as your suggestion since this is not the main point of this ms. Overall, inter-annually, our data suggest that lower phytoplankton biomass observed in the spring of 2010 might be due to low temperature and low light intensity when compared to that in the spring of 2009. Spatially, phytoplankton biomass was more associated with nutrient concentration in 2009, but it was more related to temperature or combined effect of temperature x nutrient availability in 2010. In the revision, we have trimmed significantly in discussion on how temperature and light effect on phytoplankton biomass as suggested. However, we still leave Fig. 5 (now Fig. 6) in the revision for reference. Please also refer to our reply to comment 52 of Reviewer #2 in this relevant issue.

Comment 22. *Line 27, is it a real 'linear' relationship? Please show the figure or revise the sentence.*

Multiple linear regression between Chl *a* vs. SST and surface water nitrate in SST ≧ 15℃ was evidenced significantly in this period (n = 14; $p < 0.05$). This statement was however deleted to confine our discussion in this related issue as suggested.

Comment 23. *p. 16546, line 3-4, you cannot say that 'half of the CR was contributed by phytoplankton' just because the regression slope between CR and phytoplankton biomass is close to 0.5 because biomass is a 'standing stock' but CR is a 'flux', which is affected by some variables like temperature. If temperature increase leads to increases in CR but not phytoplankton biomass, the slope still changes but the contribution of phytoplankton respiration to CR does not change. Actually, it is hard to unambiguously measure phytoplankton respiration in the sea.*

Thank you for the valuable suggestion, and we agree with you that the regression slope of 0.5 between CR and phytoplankton biomass did not mean

"half of CR was contributed by phytoplankton". To further estimate, a biomass specific rate of phytoplankton respiration of 0.25 was used (Geider, 1992), but temperature effect is not included in this estimation. This estimation has also been applied to our previous studies, and good agreement between estimated (i.e., phytoplankton + bacterioplankton + protozoan) and measured CR was evidenced (Chen et al., 2006, 2009). In this study, result showed that phytoplankton was contributed about 23.3% of CR in 2009. This result has also been addressed in the revision.

Geider, R. J. (1992), Respiration: Taxation without representation? in *Primary productivity and biogeochemical cycles in the sea*, edited by P. G. Falkowski and A. D. Woodhead, pp. 333-360, Plenum Press, New York.

Comment 24. *Line 21-24, a positive correlation between POC and CR has nothing to do with the contribution of bacterial respiration to CR.*

POC was assumed as an indicator of total planktonic biomass in this study. The positive correlation between POC and CR suggested that high rate of CR in 2009 were associated with a higher planktonic biomass. Indeed, it was inappropriate to imply that CR was significantly contributed by bacterioplankton alone. To improve, this sentence has been modified and it became as "The high POC suggested that, in addition to phytoplankton, other planktonic communities (e.g., bacterioplankton, protozoan, and zooplankton) might serve as important components contributing to the CR in this period" in the revision.

Comment 25. *p. 16547, line 4, change to 'As stated above, one reason for the: : :'*

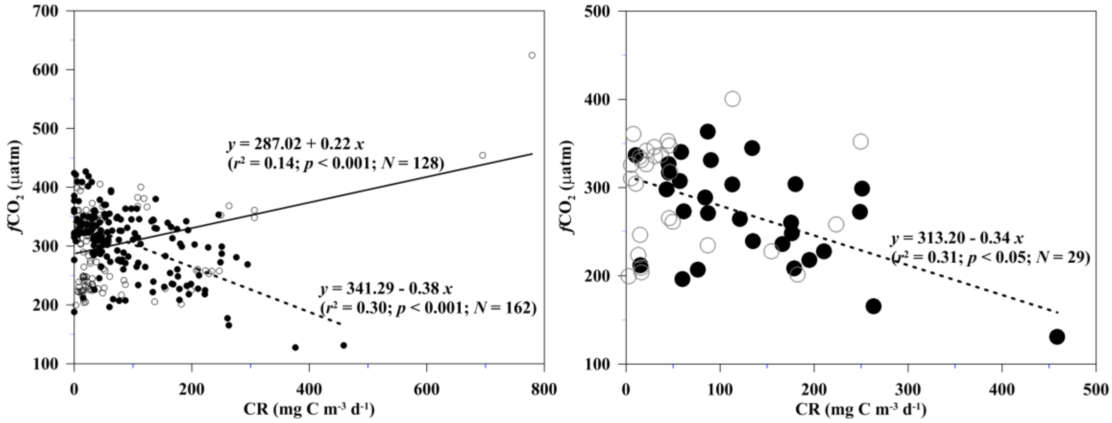
It has been changed as suggested.

Comment 26. *p. 16549, it is weird that CR was negatively correlated with fCO2. Respiration is a process that releases CO2. Although photosynthesis is often correlated with respiration, it should be the net community production (Primary production – CR) that directly affects fCO2.*

We agree with you that fCO_2 should be more directly affected by net community production during vigorous planktonic activities period, e.g., spring of 2009 in this study. However, effect of net primary production on fCO_2 might be less important or trivial in less productive period as spring 2010 in this study, and it was more likely related to or driving by physical forcings. That might explain why there was no significant relationship evidenced

between fCO_2 and net primary production in spring 2010 if two larger values data excluded in this analysis (please refer to our reply on your comment 1). Even though, primary production was not measured in vigorous planktonic activities period, the effect of planktonic communities on fCO_2 still could be examined by using variables including, Chl *a*, POC, and CR. Indeed, significant linear relationships were evidenced between fCO_2 and POC or CR in the surface water. Although, the relationship between fCO_2 and Chl *a* was at the margin of statistical significance (p = 0.007). All these results suggest that fCO_2 in the surface was strongly influenced by planktonic community. Although, it is reasonable to present the relationships between fCO_2 vs. POC or net community production (i.e., PP - CR; as suggested in comment 1) since they were integrated biomass or rate of total planktonic communities. However, POC was only measured in spring of 2009 and net community production was only available for 2010. In order to compare and evaluate how fCO_2 affected by planktonic community in both springs, therefore, we chose present the relationship of fCO_2 vs. CR as shown in Fig. 8a since CR was one of the major issues of this study and also measured in both springs. Yes, we agree with you that it looks unreasonable intuitively for CR negatively correlated with fCO_2 since CR is a process that releases CO_2. However, as our statement in the text, the higher CR indicates that planktonic activities were vigorous. The lower fCO_2 observed in 2009 implies that more CO_2 was absorbed via photosynthesis than that regenerated from CR in regions with higher planktonic activities. In 2010, there was no significant relationship evidenced between fCO_2 vs. CR, and it suggests that fCO_2 was not related to planktonic communities due to less production in this period. To further confirm this assumption, all data (instead of only surface water data in Fig. 8a) for CR and fCO_2 at each period was pooled and analyzed, and result was shown as Figure below for reference. Similar conclusion could be drawn as previous statement even though positively linear regression was found in 2010. Although this implies that fCO_2 will increase due to higher respiration rate in this low production period of 2010. However, this positive trend between fCO_2 and CR in 2010 was mainly caused by two data points with larger values, and there was no statistical significance if those two data points excluded from this analysis. It indicates that fCO_2 was not controlled by CR (or planktonic communities), and it was similar to conclusion derived from result of fCO_2 and CR in the surface water. Even though, there were more data points for all the pooled data (right panel of graph below) compared to that in the surface water (i.e., left panel of graph below). To concise and focus on conclusion, we chose to present the surface

water data (i.e., Fig. 8a or right panel) in the revision.



(Right panel) Relationship between fugacity of CO_2 (fCO_2) and planktonic community respiration (CR) in the spring of 2009 (●; dashed line) and 2010 (○; solid line) by using all data at each period. Fig. 8a of the revision was shown at left panel for your reference.

Comment 27. *p. 16551, line 4, change 'double' to 'twice'.*

It has been changed as suggested. Thanks!

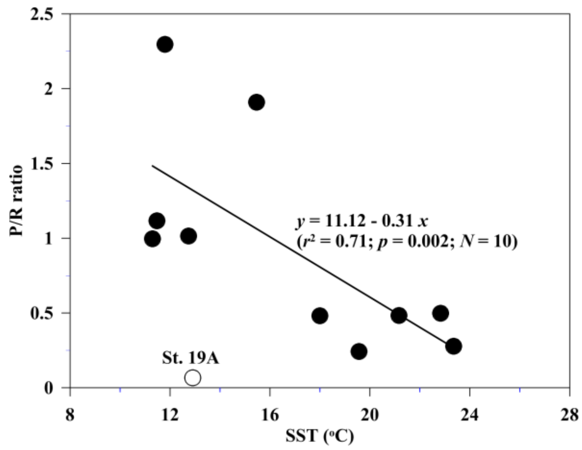
Anonymous Referee #2 (RC C6674)

General comments:

Comment 28.*This paper presented interannual variations in the community respiration in spring 2009 and 2010, as well as fCO2. However, authors failed to present an interesting store. Many concerns have been raised. First of all, the effect of temperature on community respiration that they presented is a known fact. It is interesting to determine if temperature affect ratio of primary production to respiration. However, they failed to do so since primary production data was not available. Secondly, the Changjiang river input would affect fCO2, but they never mentioned this possibility. They only attributed interannual variations in fCO2 to temperature. Some statements seemed incorrect. For example, they stated that Changjiang river discharge was similar in spring in 2009 and 2010. However, this statement is not supported by salinity data. In addition, I have a lot of comments (see below).*

Thank you for many valuable and constructive suggestions. In this ms, we presented very comprehensive data set in physical and chemical hydrograph and biological variables to explore the potential reason(s) for understanding why there were large inter-annual variation in planktonic community respiration (CR) in spring. As we know, this was probably the first data set in CR in spring in the East China Sea. This result is also extremely important for annual carbon budget since the spring, in general, has high riverine flow accompanied with high inorganic nutrients and organic carbon discharged into the shelf ecosystem. Our results demonstrated clearly, hopefully, that high organic carbon consumption (i.e., CR) in the spring of 2009 was attributed to high planktonic biomasses, and the lower CR rate during the cold spring of 2010 might be mostly limited by low temperature in the ECS. These results provide very important scientific information for us to understand organic carbon consumption, especially during seasonal transition period (from winter to spring), not only in the ECS but also for the other shelf ecosystems. We agree with you that organic carbon consumption is regulated by planktonic activities and temperature in any time. However, our results suggest that low CR in 2010 was mostly limited by low temperature which in turn constrained growth of phytoplankton, bacterioplankton, as well as other planktonic communities. Although growth of phytoplankton was not measured in this study, our results of Chl *a* vs. temperature or nutrients suggest that low Chl *a* observed in this period was mainly related to low temperature but not nutrient concentrations (please refer to our reply to reviewers' comments 21 and 52 for details). As for bacterioplankton, unfortunately, this was not

measured in this study, however, the low temperature effect on growth of bacterioplankton has been evidenced in the ECS by one of co-authors (Shiah et al., 1999) and this effect is also well known in other marine ecosystems (del Giorgio and Williams, 2005). Yes, we agree with you that it is worthy to see how temperature effect on P/R ratio. Even though both PP and CR was only measured simultaneously in spring 2010, the relationship between SST and P/R ratio was analyzed to explore how temperature effect on P/R ratio. Result is presented as below, and it shows that there was no significant trend between SST ad P/R ratios by using measured PP and CR to calculate R/R ratio. There was still no significant relationship between SST and P/R ratios if both measured and estimated PP in 2010 were applied to this analysis. However, a negative linear regression was significantly evidenced if P/R ratio of St. 19A was excluded (please see figure below). As your suggestion, this negative trend between SST and P/R ratios might be true since bacterioplankton is one of major contributors of CR and its growth and respiration will increase with temperature increasing. Unfortunately, as you know, we do not have enough data to prove this assumption, and therefore we reserve to address this observation and assumption in the revision. However, we glad to present this result if you believe that is worth it.



As for effect of the Changjiang River discharge on fCO_2, it is indeed important to evaluate its potential impact. As your suggestion, also in your comment 55, it has been evaluated and results show that its impact on fCO_2 might be minor (please refer to our reply to your comment 55 for details). The related results and discussions were also addressed in the revision.

As for different results between CDW area and salinity data on estimating the Changjiang River discharge, it seems contradictory, however, it was mostly due to one particular station (i.e., St. 19A in 2010) with the lowest SSS (18.35 psu). Overall, the Changjiang discharge in spring of 2009 and 2010 was

comparable or even larger than the averaged discharge observed in summer in the ECS from our previous studies (Chen et al., 2006, 2009). Please refer to our reply to your comments 29 and 43 for further details.

Overall, we have taken your and the other reviewers' comments very seriously in preparing this revision. In the revised version, we have modified our manuscript according to your and other reviewers' comments (please refer to our responses to your comments for details). We are confident that this manuscript is now suitable for publication in "*Biogeosciences*".

del Giorgio, P. A. and Williams, P. J. le B. (eds.) (2005), Respiration in aquatic ecosystems. Oxford University Press, New York, 315pp.

Shiah, F.-K., Gong, G.-C., and Liu, K.-K.: Temperature vs. substrate limitation of heterotrophic bacteriaoplankton production across trophic and temperature gradients in the East China Sea, Aquat. Microb. Ecol, 17, 247-254, 1999.

Detailed comments:

Comment 29. *Line 8 in page 16534, why did they say that the fluvial discharges are similar. Based on salinity data, the river discharge differed significantly between two spring seasons.*

We agree with you that the SSS seemed lower in 2010 than that in 2009, and it was mainly attributed to there was the lowest SSS observed at St. 19A (SSS = 18.35 psu) in 2010. The mean±SD values of SSS in CDW in 2009 and 2010 were however similar with values of 29.27±0.73 and 29.24±1.61 (St. 19A excluded), respectively. To clarify, this result has also been addressed in the revision. Based on calculated area of CDW (i.e., area of SSS≦31 psu), the area of CDW for 2009 (23,638 km^2) and 2010 (19,907km^2) in this study were larger than the mean area of CDW (15,604 km^2) in summer observed from our previous study (Chen et al., 2009). The contradictory results on riverine discharge (if any) between SSS and the area of CDW might be due to complexity of mixing processes influenced by different physical forcings (e.g., flow rate, coastal current, wind velocity and direction, Taiwan warm current..) in this plume region (please also refer to our response to your comment 43 for details). In both cases, we agree with you that it was inappropriate to described "the fluvial discharges were similar", and it has been revised to "These results showed that although the fluvial discharge rates were comparable to the high riverine flow in summer, the planktonic community respiration (CR) varied widely between the two springs" in the revision.

Comment 30. *Line 14 in page 16534, what are the planktonic activities that they mentioned here?*

We meant both planktonic biomass and metabolic rate based on higher POC and temperature in 2009. In addition, Phytoplankton biomass accounted for 42% of the mean POC if Chl *a* was expressed per carbon units. Therefore, we emphasized the importance of phytoplankton in this statement. However, we have re-estimated the contribution of phytoplankton to CR, and it was attributed to CR with a fraction of 23.3% in 2009 (please refer to our reply to comment 23 of Reviewer # 1). Even though other planktonic communities were not measured in this study, the high POC was strongly suggested that there were high planktonic biomass in this period. Therefore, this sentence has been revised and it became as "These results suggest that the high CR rate in 2009 can be attributed to high planktonic biomasses" in the revision.

Comment 31. *Lines 25-16 in page 16534, that sentence 'organic carbon consumption : : :: : :.physical factor (e.g. temperature)' is meaningless. In any time, organic carbon consumption is regulated by planktonic activities and temperature.*

Yes, we agree with you that organic carbon consumption is regulated by planktonic activities and temperature in any time. For example, in our previous study, we demonstrate that CR rates were proportionally related to the area of CDW, an index of river discharge rate. The reason was due to that growth of phytoplankton was associated with variable nutrient concentrations during different riverine flows (Chen et al. 2009). In this study, we try to explore the potential reason(s) for the large inter-annual variation of organic carbon consumption observed in spring even with comparable riverine discharge as that observed in summer of our previous studies. To emphasize the result, this statement has been rephrased and it became as "To conclude, these results indicate that high organic carbon consumption (i.e., CR) in the spring of 2009 could be attributed to high planktonic biomasses, and the lower CR rate during the cold spring of 2010 might be likely limited by low temperature in the ECS" in the revision. Hopefully, this change will satisfy your concern.

Comment 32. *Line 1 in P 16535, change 'intraseasonal' to 'interannual'. In this study, they mainly compared CR in spring between 2009 and 2010.*

Thanks! That is exactly what we try to express, and it has been changed throughout the text as suggested.

Comment 33. *Lines 13-14 in P 16536, delete that sentence. Don't present results or conclusions in the introduction.*

It is indeed inappropriate to present results or conclusions in the introduction section, and it has been omitted in this revision.

Comment 34. *Line 17 in P 16536, change 'intraseasonal' to 'interannual'.*

As our reply previously, it has been changed in this revision.

Comment 35. *Line 17 in P 16536, what variations?*

We meant "variations of planktonic community respiration", and it has been added into this revision. Thank you for the correction.

Comment 36. *Line 18 in P 16536, what is the role of planktonic acitivity? What is the carbon balance? They did not measure bacterial production that is equally important to respiration. How to evaluate the carbon balance without BP?*

We admire you for going through this ms thoroughly and understanding what we intend to deliver to the reader using existed data set. We agree with you that this description was not suitable based on our existed data and analyses, and it has been revised and became as "In addition, the relationship between CR and fCO$_2$ was examined to reveal the contribution of the planktonic community to the fCO$_2$ variation in spring" in the revision.

Comment 37. *CR is more related to DOC than POC. However, DOC data were not presented.*

In general, one of the most important contributors of CR is bacterioplankton in pelagic ecosystem. DOC is one of major factors regulated bacterial growth, therefore, CR should be closely related to DOC if this assumption hold true. Unfortunately, DOC was not measured in this study. To compensate, in this study, POC was measured and assumed as total planktonic biomass even it might be biased due to terrestrial source from fluvial runoff. Our result showed that significant relationships between POC and Chl *a* were found for the surface water (r^2 = 0.70; p < 0.001) and for the averaged value over euphotic zone (r^2 = 0.82; p < 0.001) (It should be note that these results were not presented in ms). Furthermore, phytoplankton biomass accounted for 42% of the mean POC if Chl *a* was expressed per carbon units. These all suggest that POC can be served as an indicator of total planktonic biomass. In such case, CR related to POC can be expected, and it was indeed significantly evidenced in this study. Similar results have also been found in our and other

studies (Chen et al., 2003, 2006; Robinson and Williams, 2005).

Robinson, C. and Williams, P. J. le B.: Respiration and its measurement in surface marine waters, in Respiration in aquatic ecosystems, edited by: del Giorgio, P. A. and Williams, P. J. le B., Oxford University Press, New York, 147-180, 2005.

Comment 38. *How to obtain daily primary production?*

In this study, primary productivity was measured by 14C assimilation method (Parson et al., 1984). For stations with PP measurement, the P^B-E curve at each sampling depth was constructed in a seawater-cooled incubator with artificial illumination (1000W submersible halogen quartz lamp). Samples were incubated for 2-4 hours. In total, nine different light levels (E) 2000, 1365, 950, 800, 480, 400, 260, 130, 65 µE m^{-2} s^{-1} of PAR as well as one dark bottle were set in the incubator. Samples were measured in duplicate at each light level. Equation (1) or (2a) was used to fit the experimental P^B-E curve results of each depth of the sampling station to estimate the parameters P_m^B and α.

$$P^B(z) = P_m^B(z)\left(1 - e^{-\frac{\alpha(z)E}{P_m^B(z)}}\right) \qquad \textbf{(1)}$$

$$P^B(z) = P_s^B(z)\left(1 - e^{-\frac{\alpha(z)E}{P_s^B(z)}}\right)\left(1 - e^{-\frac{\beta(z)E}{P_m^B(z)}}\right) \qquad \textbf{(2a)}$$

$$P_m^B(z) = P_s^B(z)\left(\frac{\alpha(z)}{\alpha(z)+\beta(z)}\right)\left(\frac{\beta(z)}{\alpha(z)+\beta(z)}\right)^{\beta(z)/\alpha(z)} \qquad \textbf{(2b)}$$

Using equation (3a) or (3b), we calculated primary productivity at various depths (PP(z)) at each station.

$$PP(z) = \int_0^t chl(z)P_m^B(z)(1 - e^{-\frac{\alpha(z)E(z,t)}{P_m^B(z)}})dt \qquad \textbf{(3a)}$$

$$PP(z) = \int_0^t chl(z)P_s^B(z)(1 - e^{-\frac{\alpha(z)E(z,t)}{P_s^B(z)}})e^{-\frac{\beta(z)E(z,t)}{P_s^B(z)}}\, dt \qquad \textbf{(3b)}$$

where $E(z,t) = E_0(t)e^{-K_d z}$, $E_0(t)$ is incident PAR above sea surface during the day, K_d mean attenuation coefficient of PAR within the euphotic zone, and t the time from sunrise to sunset.

The euphotic zone integrated primary production (IP) at each station was integrated from equation (3b) as follow:

$$IP = \int_0^{Z_e} PP(z)dz \qquad \textbf{(4)}$$

To brief the description, the following statement has been added into the method section, i.e., The results of photosynthesis-irradiance curves were

used to calculate primary production for stations with incubation performed (please refer to Gong et al. (1999) for details), for reference in the revision.

Comment 39. *Primary production was determined with CR at the same time?*

Measurements for PP and CR were incubated at the same time if measurement of PP were performed in this study. It should be noted that primary production was measured with samples taken at stations only occupied during daytime.

Comment 40. *What statistical analysis was performed?*

Thank you for your valuable suggestion. In this ms, the software SigmaStat (version 3.5, Systat Software, Inc.) was used for the analysis of simple and multiple linear regressions, analysis of variance (ANOVA), as well as Mann-Whitney Rank Sum test for group comparison. This was also indicated in the revision. Please also refer to our response to comment 2 of Reviewer #1 for this relevant issue.

Comment 41. *Line 1 in P 16540, what variation?*

We mean the largest spatial variation of temperature and salinity of surface waters, and it was indicated in the revision.

Comment 42. *Where was the data of Changjiang Diluted Water discharge from?*

In this ms, we define the area of the CDW as the water surface where SSS ≤ 31, and it was used as an index of the amount of freshwater water discharge into the ECS in this and our previous paper (Chen et al., 2009). The rational for using the area of the CDW as an index of the amount of freshwater discharge has been given in Chen et al. (2009). To estimate the area where SSS ≤ 31 in this study, Surfer 8 (Golden Software, Inc.) program has been applied. To clarify and concise the text, we have slight modified the cited reference as "refer to Chen et al., 2009 for details" for the readers' reference in the revision.

Comment 43. *CDW data presented here is contradictory with salinity data in this study. Why?*

We agree with you that it seemed there was more freshwater discharged into the ECS in 2010 than in 2009 if based on SSS data. However, the lowest SSS in 2010 was only restricted to St. 19A (SSS = 18.35 psu), and the mean±SD value of SSS for CDW was 29.24±1.61 if data for St. 19A excluded. This value was

almost the same as that in 2009 with mean±SD value of 29.27±0.73. The area of CDW was, however, larger in 2009 than in 2010, and it suggests there was more freshwater discharged into the ECS in 2009 than in 2010. The contradictory between these two might be due to complexity of mixing processes influenced by different physical forcings (e.g., flow rate, coastal current, wind velocity and direction, Taiwan warm current..) in this plume region. We do understand that responses of system, if any, should be more directly related to the volume of freshwater discharge. It is reasonable to regress the examined variables against the volume of freshwater. Unfortunately, the seaward-most river station (i.e., the Datong station) is located more than 500 Km from the river mouth. As we state in Chen et al. (2009), there are several reasons that might bias the estimation of amount of freshwater input into the ECS. For example, 1). accumulation of water near coastal areas around the mouth and adjacent areas of the Changjiang River, and 2). producing a considerable time lag of transport between flow observations (i.e., the Datong station) and river plume discharge. In any case, we found that *in situ* area of the CDW was a good indicator for river effects on plume ecosystem dynamics.

Comment 44. *Lines 1-2 P 16541, need to show data of SSS vs phosphate and silicate.*

Thank you for the valuable suggestion, and this make it more convincible to the reader. Relationships between SSS vs. phosphate and silicate have been added in the revision (i.e., Fig. 4c and 4d). In addition, range and mean±SD values of phosphate and silicate have also been added into Table 1 for comparison and reference. The related description has also been modified throughout the text.

Comment 45. *Lines 6-7 P 1641, replace '0' with 'undetectable' . 'undetectable' does not mean '0'.*

Thank you for the correction, and it has been revised in the revision.

Comment 46. *What is the detect limit of nutrients (NO3, PO4, SiO4)?*

The detect limit for nitrate, phosphate, and silicate are 0.3, 0.01, and 0.5 μM, respectively. They are also indicated in the method section in the revision.

Comment 47. *Lines 13-14, in P16542, Where is the data of chl a vs nitrate?*

It is indeed much easier to convince the reader by presenting the data, and the relationship between Chl *a* vs. nitrate has been added as Fig. 5a in this

revision. Thanks!

Comment 48. *Lines 15-16 in P16542, need to show data of chl a vs phosphate or silicate.*

Thanks again! The relationship between Chl *a* vs. phosphate has also been added as Fig. 5b in this version. To concise the text, the relationship between Chl a vs. silicate is however not presented since the pattern is similar to Chl *a* vs. nitrate or phosphate.

Comment 49. *Lines 18-20 in 16542, Fig 4 did not support this statement.*

To support this statement, as your suggestion, data of Chl *a* vs. nutrients (i.e., Fig. 5) have now been presented in the revision.

Comment 50. *Lines 21-22 in P 16542, I don't know that SSS show the same pattern between 2009 and 2010. SSS in 2010 was obviously lower than 2009.*

We apologize for the confusion, and we intended to address that the distribution pattern between SSS and nitrate was similar in the spring of 2009 and that in 2010 in this statement (instead of SSS show the same pattern between 2009 and 2010). To clarify, this statement has been slightly modified as "The distribution pattern between SSS and nutrients (e.g., nitrate, phosphate, and silicate) was similar in the spring of 2009 and that of 2010 (Fig. 2, 3, and 4a, c, d), and a similar trend was also found between SSS and Chl *a* in the surface water in 2009 (Fig. 4b)" in the revision.

Comment 51. *Line 1-2 in P16543, I don't understand how the previous statements explained the relationship between SSS and nitrate and Chl a. A linear regression of SSS vs nitrate is because nitrate was mainly from the river discharge, but not for Chl a.*

We appreciate your valuable suggestion, and we did jump one step ahead to come out the conclusion without presenting the direct evidence between nutrient and Chl *a*. As your suggestion on comments 47 and 48, the graph and description regarding relationship between nutrients (NO3 and PO4) and Chl *a* has been added in this revision. In the meantime, this statement has been removed to confine the discussion in this related issue of the revision.

Comment 52. *Line 17-19 in P 16543. There is a temperature and salinity gradient from inshore to offshore due to mixing of the river discharge with seawater. Maximum Chl a occurs generally in region with middle salinity (also middle temperature) due to* *long residence time. I don't agree that temperature control phytoplankton growth. Their statement is biased based on relationship of Chl a vs temperature alone.*

Yes, we do agree with you. In general, the maximum Chl *a* concentration appears in region with middle salinity (also middle temperature), and this is similar to our observation in the spring of 2009 and in our previous studies in summer which is typical example in the East China Sea from our observation. In the spring of 2010, cold temperature might be however the major factor controlling growth of phytoplankton as shown on Fig. 6. Indeed, this statement was biased if simply based on this significant relationship. To improve, as your suggestion, data of Chl *a* and nutrient concentration has been presented in this revision. Growth of phytoplankton might be less controlled by nutrient availability since there was no significant relationship observed between Chl *a* and nutrient concentration in the spring of 2010 as shown on Fig. 5 in the version. Hopefully, this can clarify your concern. Please also refer to our response to Comment 21 of Reviewer #1 for this related issue.

Comment 53. *Lines 21-22 in P 16546, It is not clear about that sentence. Phytoplankton contributed to CR? I think that bacteria and zooplankton mainly contributed to CR, not phytoplankton.*

Based on estimation, contribution of phytoplankton to CR was about 23.3% in the spring of 2009 (please refer to our response to comment 23 of Reviewer # 1 for details). The positive correlation between CR and POC in 2009 suggested that high CR was attributed to high planktonic biomass. Unfortunately, bacterioplankton, protozoan, as well as zooplankton were not measured in this study. However, the result strongly implies that, in addition to phytoplankton, other planktonic communities (e.g., bacterioplankton, protozoan, and zooplankton) might be served as important components contributing to the CR, in this period. To clarify, our previous statement on this has been revised in the revision as suggestion of yours and Reviewer #1.

Comment 54. *How to define inner, middle and outer shelf?*

It is indeed ambiguous without definition. Here, we define inner, middle, and outer shelves as regions with isobath: $\leqq$ 60m, 60 - 100m, and > 100m, respectively. Previous study has showed that the inner shelf in the ECS which generally has sea surface salinity less than 31 distributed within the 60-m isobath region between the latitudes of 27 and 32 °N along the coast [e.g., *Beardsley, et al.*, 1985]. To improve, this statement has been modified as "

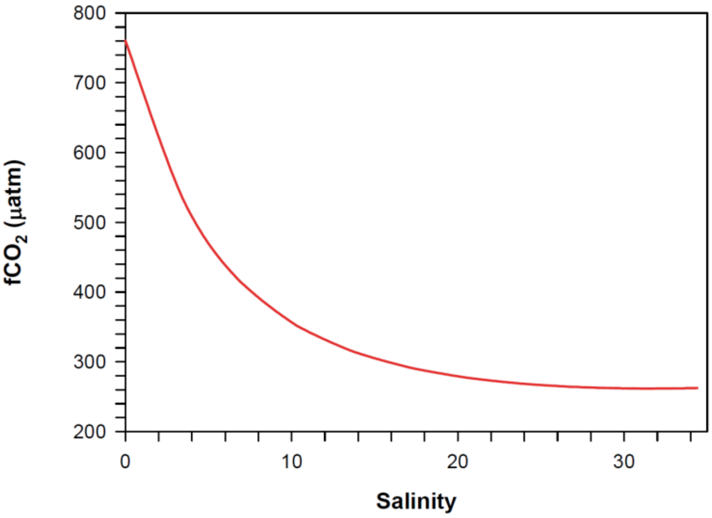
There were five stations with a P/R ratio ≧ 1 (mean value = 1.47), and these stations (Sts. 5, 21, 29, and 30) were mostly in the inner shelf (isobath ≦ 60m; Fig. 1; Beardsley et al., 1985), except for St. 28. Interestingly, the lower P/R ratios (< 1) were observed mostly in the middle (isobath within 60–100m) to outer shelves (isobath > 100m) in this period, except for St. 19A" in the revision. In addition, the isobath of 60 and 100m has also been added into Fig. 1, 2, 3, and 7 for reference.

Beardsley, R. C., Limeburner, R., Yu, H., and Cannon, G. A.: Discharge of the Changjiang (Yangtze River) into the East China Sea, Cont. Shelf Res., 4, 57-76, 1985.

***Comment** 55. The Changjiang River discharge would affect fCO2. Based on salinity data, the river discharge was different. It is necessary to evaluate how the river discharge affect fCO2 in spring.*

To response to your inquiry, we have simulated the influence of river discharge on fCO_2 variation using the conservative mixing data of TA and DIC between freshwater and seawater end-members. The TA and DIC data reported by Zhai et al. (2007) for the Changjiang River in spring were used as freshwater end-member (both TA and DIC=1575 μmol kg^{-1}), whereas the averaged surface data at stations 10, 12, 24 and 26 in spring 2009 were chosen to represent the seawater end-member (salinity= 34.4, TA=2279 μmol kg^{-1}, and DIC=1960 μmol kg^{-1}; W.C. Chou, unpublished data). The simulating fCO_2 mixing curve shows that fCO_2 varies from 262 to 277 μatm within a salinity range between 20 and 34.4. (Please see the figure below), which is relatively small compared to the observed inter-annual variation of fCO_2. Therefore, we think that the changes of river discharge didn’t play a significant role on the inter-annual variability of fCO_2. For the reader's reference, this result has also been included in the revision.

Zhai, W., Dai, M., Guo, X., 2007. Carbonate system and CO_2 degassing fluxes in the inner estuary of Changjiang (Yangtze) River, China. Mar. Chem. 107, 342–356.


800
700
600
500
400
300
200
0
10
20
30
fCO2 (µatm)
Salinity