

## ***Interactive comment on* “The large variation in organic carbon consumption in spring in the East China Sea” by C.-C. Chen et al.**

### **Anonymous Referee #1**

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#### General comments:

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  $f\text{CO}_2$  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  $f\text{CO}_2$  in that cruise.

Second, this paper teems with simple comparisons and (linear) regressions (P.S., I

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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.

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.

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).

Specific comments:

P. 16534, Line 17, should be 'led'.

p. 16535, line 4, 'when it comes to determine. . .'

p. 16536, line 1, 'released through. . .'

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

p. 16539, line 4, are duplicates sufficient

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

p. 16540, line 24, add 'maps' after 'contour'

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

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.

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

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

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

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.

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

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.

p. 16543, line 8, 'may not limite. . .'

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.

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

p. 16543, 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.

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

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

p. 16549, it is weird that CR was negatively correlated with  $f\text{CO}_2$ . Respiration is a process that releases  $\text{CO}_2$ . Although photosynthesis is often correlated with respiration, it should be the net community production (Primary production – CR) that directly affects  $f\text{CO}_2$ .

p. 16551, line 4, change 'double' to 'twice'.

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