

Interactive comment on “Distinct bacterial production–DOC–primary production relationships and implications for biogenic C-cycling in the South China Sea shelf” by C.-C. Lai et al.

Anonymous Referee #1

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General comments:

This is a short but interesting manuscript reporting the relationships among bacterial production (BP), DOC, and primary production (PP) in the South China Sea shelf waters. The data set gives a comprehensive picture of the horizontal, vertical and time series distributions of sigma-t, nutrient (phosphate) concentration, DOC, BP and PP. Through analysis of the relationship between BP, PP and DOC in different environments, the authors examined the so called “Extended Malfunctioning Microbial-loop (EMM)” hypothesis they proposed, that is, the effects of inorganic nutrient supply on

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the coupling/decoupling of BP with PP, and thus DOC dynamics. This work is original, and the data are of quality and thus of value for publication.

Major concerns for the authors to consider include - 1. The most important conclusion of this study is that the positive relationships between BP and DOC and PP are lost in the outer-shelf, which is just to justify the EMM hypothesis, but I did not see fundamental differences between the EMM and the known MM. 2. One of the points of this work is the influence of phosphate availability on those relationships. As the authors pointed out in the introduction (P9072, L12-14), the nutrient control of BP-DOC pattern is already in the MM. 3. Another concern is their overlook of DOC input from Peal River (PR) and their bioavailability which actually definitely affect BP-DOC relationship, especially in PR mouth area. 4. lack of data analysis of bacterial abundance, viral abundance, flagellate abundance, etc. which also affect BP-DOC relationship in various ways in microbial loop. 5. The author also claimed that internal wave contributes to the observed BP-DOC pattern, but no data back up the point. The influence of internal wave on microbial community is not really understood so far but I think it is not just nutrient supply. 6. One interesting argument is that river nutrient input elevates primary production and DOC which is quickly depleted by bacteria creating a more heterotrophic system in the shelf and a stronger CO₂ source. This point has been previously proposed (Nianzhi Jiao, Kai Tang, Haiyuan Cai, Yujiao Mao, 2011, Increasing the microbial carbon sink in the sea by reducing chemical fertilization on the land. Nature Reviews Microbiology 9, 75 (January 2011) Doi:10.1038/nrmicro2386-c2) and should be cited here. 7. The authors should describe clearly the depth ranges of the depth-averaged values, since they were different in different figures and relationships analysis and the trends were even contrary in different depth ranges. For example, Figure 6 showed that concentrations of depth-averaged PO₄ (within euphotic zone) were low in the inner-shelf and then increased seaward. However, Figure 8 showed higher depth-averaged PO₄ concentrations (within mixed layer depth) in the inner- than outer-shelf, and the legend mentioned “c.f. Fig. 6”. It really makes confusing to understand the context. The authors should state clearly in the methods section where only “within

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the deepest sampling depth (i.e. euphotic zone)” was currently mentioned. Also, I suggest the authors to add a line indicating mixed layer depths in Figure 5B. 8. In-depth discussion should be included, for example, in term of the possible competition mechanisms for limiting nutrients between heterotrophic bacteria and phytoplankton. Finally, missing of N measurements made the conclusions weak.

Specific comments: 1) Page 9072, line 19-26: The two sentences are repeated.

2) Page 9073, line 9: The location of St. 2 is not consistent between Fig. 1 and 5. St. 2 should be in north-east (not north-west) off the Dong-Sha atoll?

3) Page 9074, line 6; Page 9075, line 6 and 10: Abbreviation was repeated several times through the manuscript.

4) Page 9074, line 17: Not all depth-averaged values were obtained by dividing depth-integrated value by the deepest sampling depth of the station. Please refer to the above suggestion “1”.

5) Page 9075, line 5: Should be “Fig. 3A” (capital, not lowercase). Check through the MS.

6) Page 9076, line 6: Should be “PO4 concentrations were. . .”.

7) Page 9076, line 27: It is the S-E (not N-E) and S-W corners of the sampling area according to Fig. 6A.

8) Page 9077, line 9: Should be “Fig. 7A and D” (not b).

9) Page 9078, line 8: Liu et al., 1998 is not included in References section.

10) Page 9078, line 14: “The shallow area was probably not nutrient-limited” should be supported by citations, since many studies have been done in the South China Sea shelf. Or the data, such as N: P, obtained from this study should be mentioned to support “not nutrient-limited”.

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11) Page 9078, line 16: Should be “relationships” (not responses).

12) Page 9079, line 22 and 23: Should be “Fig. 7B and E” and “Fig. 7A and D” (not Fig. 6).

13) Page 9079, line 25: Delete “rate”.

14) Page 9084, Fig. 2: Should be “June 2010” according to the text.

15) Page 9088, Fig. 6: Inner-shelf are indicted by symbols of red dots (not squares).

16) Page 9090, Fig. 8: Add “depth-averaged” before “bacterial production..”; Delete “c.f. Fig. 6a”, probably “c.f. Fig. 5B”? Please point out the sources of these data in the legend and the text, i.e. stations (all data, the PRD data or the shelf-mapping data?) and cruises (both cruises, 2009 or 2010?)

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