

## ***Interactive comment on* “Factors limiting heterotrophic bacterial production in the southern Pacific Ocean” by F. Van Wambeke et al.**

### **Anonymous Referee #2**

Received and published: 21 November 2007

#### General Comments

The authors addressed a very important question, that is, what nutrient(s) limits the production of heterotrophic bacteria along vertical and longitudinal gradients across the South Eastern Pacific Gyre. Enrichment incubations with various combinations of inorganic Fe, N, P and glucose were conducted under trace metal clean and non clean conditions. Growth parameters of phytoplankton, heterotrophic bacteria and heterotrophic nanoflagellates (bacteriovores) in enrichments were measured and compared with those in no-addition controls. The authors found that iron was never served as a single limiting nutrient factor to heterotrophic bacterial growth in the sampling area. The author also concluded that phytoplankton and bacteria were limited by different nutrients at the edge of the gyre but were both limited by N within the gyre. The paper

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contained much valuable data and was well written in general. However, some of their conclusions, I am afraid, cannot be fully supported by the data. Better illustration and analysis of the data and more careful discussion and conclusion are suggested.

The samples used for incubations were whole water without filtration. That means bacteria, phytoplankton, heterotrophic nanoflagellates (HNF) and other bigger zooplanktons were included in the experimental systems. Since the main purpose of the authors was to test the bottom-up effect, would it be easier for data collection and interpretation by removing the top-down control factors prior to the enrichment experiments? The fact that HNF and other bigger predators were in the system made the bottom-up effect being masked by the top-down controls. That might partly explain why bacterial abundance didn't change as dramatically as leucine incorporation rate. The discussion of the better indicator should consider this fact.

The main data for TMC studies were provided in Table 4. I have a hard time to read the table. Is it possible to transform it into a figure (like Fig.2)? The leucine incorporation experiments seemed to have a big variation between triplicates. In most cases, the change has to be more than 5 folds to make an increase significant. For 48h HNL samples, a 9- fold change was not statistically significant enough! Any conclusion that drew from the leucine incorporation data is not convincing enough to me. I would like to see the table include standard deviations.

I also have problem to believe the conclusions that were drawn from the non-TMC condition, because there is no replicate data in most cases. The authors used STB18 to set up threshold for other samples. But the leucine incorporation data for this station that used to set up the threshold (G in figure 3) had fairly big variations among the triplicates (as big as 2 folds).

Specific Comments 1. Is there a specific reason why  $\text{NH}_4$  was not included in some N-included enrichments (MAR, HNL in table 2)? Will this affect the results? Ammonia in general supports more heterotrophic bacteria production than nitrate. 2. It would be

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easy for the reader if the author can include a little more details of the methods than just refer to other papers. 3. Results, Page 3804. Table 1 should be Table 3, I assume. 4. page 3805. Line 20. The stock didn't always increase less than the flux in table 4 5. Page 3822. Two headings of Table 4. 6. Page 3825, Fig. 2. Several typos in the figure and legend. HN-HNL, clan-clean, EGYR-EGY 7. Page 3826. T should be C.

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Interactive comment on Biogeosciences Discuss., 4, 3799, 2007.

**BGD**

4, S1962–S1964, 2007

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