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***Interactive comment on “Carbon and nitrogen uptake in the South Pacific Ocean: evidence for efficient dinitrogen fixation and regenerated production leading to large accumulation of dissolved organic matter in nitrogen-depleted waters” by P. Raimbault and N. Garcia***

**Anonymous Referee #1**

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General Comments:

The paper reports on uptake, regeneration, and loss rates of various forms of N in the euphotic zone of the South Pacific, on a zonal section that spans a wide range of trophic states (from nutrient enriched to severely nutrient impoverished). The overall data inclusion is good (though I am not an expert on the very difficult rate measurements reported here); the section plots and tables of the various rates and derived variables

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(such as the f-ratio) are valuable. The authors should be applauded for their great effort to create such a fantastic data set. I hope the data will be made publicly available.

The authors interpreted their data in the context of the mature science of N uptake dynamics. So while they report rates in previously poorly sampled areas, they did not develop particularly new ideas. They largely tested and refined older ideas. I ask that the authors look for the new science in their data. For example, I believe there is an opportunity for them to pursue their N<sub>2</sub> fixation data more aggressively. There is considerable debate now on the importance of N<sub>2</sub> fixation in the global ocean. The global rates keep going up and up and up. The authors have unique data on the process from wide swaths of the South Pacific. What is the likely basin-wide rate in the South Pacific (given that the measured rates had a small range, a rough estimate might be possible). What is significant about the finding that the rates in the gyre were so similar to rates where NO<sub>3</sub> was abundant? And that the rates were measurable even where Tricho was absent? And what do the results tell us about the recent suggestion by Deutsch et al 2007 that water upwelled from OMZs is preconditioned to favor/drive global marine N<sub>2</sub> fixation? The authors referred to the Deutsch paper briefly, but only to say that their measured rates are 6 fold lower than that predicted by the Deutsch model. That is an important finding to develop further. There are proposals being written today to test the Deutsch idea, yet the authors may already have the answers. Can the authors tell us whether or not the path to higher global rates is supported by their data, given that they have sampled a system that represents a massive part of the global ocean experiencing N<sub>2</sub> fixation?

The linkage the authors sought to establish with DOM accumulation in the oligotrophic waters is very weak and should be eliminated. Fig. 11, which shows the inverse relationship between PP and integrated DOC stocks, does not tell us how the system works. DOC accumulates seasonally as a function of net community production, not of PP. That there is a relationship as shown in Fig 11 is due to the physics of the system. PP could be replaced with vertical turbulent mixing on the X-axis and the relationship

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would still hold; but this too is old news. The references to the DOM literature are well out of date and sometimes used inappropriately. Some of the findings from those old references are incorrect. Most importantly, high release rates of DOM as measured by the authors have little to do with net DOM accumulation in the ocean. The manuscript should stand on the merit of what it can say based on the data presented, and those were all about the N cycle. The paper can say little about DOM accumulation in the ocean, yet that is presented as central to the paper, given its prominent place in the title. The authors do not have the data required to connect their data to the issues of DOM distributions and accumulation. They have data on DOM release, which is a different topic. DOM release feeds microbes; it does not necessarily result in DOM accumulation.

Finally, the grammar is so poor that it is very difficult to read, enjoy, or judge the science held within. For example, there are 11 grammar/sentence structure/logic issues in the Abstract alone. There are another 6 in the first paragraph of the Introduction. I sympathize that English is a second language for the authors. The manuscript needs to be proof read and corrected by someone who is fluent in the language of the journal.

#### Specific Comments:

- 1) There are hundreds of grammatical and sentence structure errors to be corrected. There are too many to list here.
- 2) Figures 4 & 6 and their labels are tiny and hard to read.
- 3) The occupied section looks zonal, not longitudinal.
- 4) Correct usage of the words production, productivity and productive is required.
- 5) The references need to be updated.
- 6) The authors need to be more precise in their use of references. As example:  
...recent work pointed out the importance of nanoplanktonic N<sub>2</sub> fixers (Zehr et al. 2001;

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Karl et al 2002).....;

Zehr reported the presence of the genes for N<sub>2</sub> fixation in nanos; but the presence of the genes does not point out the importance of nanos. As I understand it, the role of nanos still needs to be established. And Karl et al is a review mentioning Zehrs work on the genes. Their mention of it does not constitute pointing out the importance of nanos. If we do not know if nanos are important, maybe the authors data will tell us! That would be a good contribution.

7) References are missing, duplicated, out of date, sometimes mis-spelle

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**BGD**

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