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Interactive comment on "Dynamics of phytoplankton and heterotrophic bacterioplankton in the western tropical South Pacific Ocean along a gradient of diversity and activity of diazotrophs" *by* France Van Wambeke et al.

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General comments

In the manuscript "Dynamics of phytoplankton and heterotrophic bacterioplankton in the western tropical South Pacific Ocean along a gradient of diversity and activity of diazotrophs", Van Wambeke et al. present estimations of heterotrophic prokaryotic production in the Western Tropical South Pacific region, and they explore the causes of its variability, focusing on autotrophic activity and nutrient availability. The data pre-

sented in the paper are very valuable, as the carbon budget in the oligotrophic regions is still a topic of debate, in part because of lack of data to adequately characterize the bacterial contribution to it. The data presented are a great mix of observations and experiments, and the analysis made have great potential. However, the way the paper is written makes the information confusing and the conclusions vague. The title doesn't reflect the contents of the paper, the methods are not complete, the discussion is not well structured or clear, and the conclusions are not in line with the results highlighted in the discussion (See details in specific comments). The English and the writing needs profound review.

Specific comments

- Title: The authors mention in the title a gradient of diversity and activity of diazotrophs, but such gradient is not shown in the paper. They base part of their discussion in different groups of diazotrophs described during the same cruise by other authors (lines 530-541), but as they described it, it is not a diversity gradient, just differences in the dominant genera. The diazotrophs activity gradient is not clear either in the paper. I would suggest for the title to focus more on the analysis made to elucidate the factors controlling the bacterioplankton activity in different regions of an oligotrophic system.

- Abstract: The abstract is a good summary of the paper, but I think some parts can be removed: * Line 28: the i.e. can be removed, it makes the sentence too long, and it is not necessary * Line 30: The BGE estimation doesn't provide useful information here * Lines 33-36: I don't find this information about the bloom developed along the paper, I think this should be removed from here.

- Methods: Some variables have a lot of weight in the discussion but methods are not described. For example Nitrogen fixation rates, community respiration and GPP, or nutrients (nitrate, nitrite, and phosphate) concentrations. At least a brief description of the methods should be included, even if they have been explained somewhere else.

- Lines 109-112: The criteria used for stations selections is not stated. Even if it is

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described in other papers, a better explanation should be included here because it can affect the interpretation of the results.

- Line 113: It should be mention here that the sampling in LD stations was Lagrangian.

- Lines 151 and 161: The word "occasionally" is too vague, please specify at least how many times.

- Lines 165-179: The incubation time used should be specified.

- Lines 185 and 207: Are the cast numbers necessary? I think they can be removed.

- Line 230: The authors talk about a gradient, but they don't specify what kind of gradient. A gradient of productivity? A gradient of diazotrophs activity?

- Lines 240-245: Authors say that nutrients and organic matter distribution allowed them to distinguish two regions, but those data are not shown in the paper. At least, a description of the differences between the regions should be included. For example, you can include some extra data in Table 2 with nutrients and organic matter concentration, or whichever criteria used to identify those two regions.

- Lines 236-246 are confusing and need to be rewritten.

- Line 249: "Averaged per SD station, the dcm fluctuated..." I don't understand what that means and what the following ranges refer to.

- Line 269: I suppose what authors meant is that TDIP increased with depth, not the vertical profiles themselves.

- Line 281: I don't think the periodicity of the dcm fluctuation is evident in figure 5. That fluctuation and the increase of fluorescence in the afternoon are not well described. Those patterns are not apparent in the figures, and not statistical analysis is presented. However, I think those results can be avoided because authors don't mention them along the discussion at any point.

-Line 286: Values from In situ and on deck incubation cannot be directly compared, as temperatures for the incubations are different. Particularly for samples from depths below the mixer layer. So authors shouldn't highlight a higher value from on deck incubations without a proper analysis of correspondence between on deck and in situ estimations.

-Lines 324-326: Figure 8 shows a linear regression analysis, but in the text, authors present the correlation coefficient (r), but not in the figure or in the text they mention the significance of the fitting. With the correlation coefficient, authors shouldn't interpret the results as a dependency, because the correlation between two data sets doesn't indicate causality of one of the variables from the other.

- Line 331-334: Those N2fix temporal trends are not shown anywhere. If authors don't want to show them in a figure, use some statistics to state those patterns.

- Line 393-404: The definition of the regions is not well described. Some suggestions: * Use the same terms to name them along the entire manuscript. * Show them on the map (figure 1) * Refer here to Table 2, and complete the table with the criteria described here (nitracline depth). * Define what "distinct nutrient distributions" means or show it in a plot * In figure 4 put in different colors or patterns the profiles for each region

- Line 422: "after filtration and removal... producers" corresponds to methods section.

- Line 431-432: Why is the information in the last sentence of this paragraph relevant? Please, elaborate

- Lines 445-462: This paragraph is not well linked to the rest of the discussion. There is not any mention of the present paper results, and it is not clear the contribution of the present paper to the debate described in the paragraph. Please, elaborate.

- Lines 482-488: This paragraph is confusing and needs rewriting

- Lines 490-492: I don't understand the calculations described in here

- Lines 490-500: What is the overall contribution of all these calculations to the paper?

- Lines 506-509: I don't see the relation between the three first lines of the paragraph and the following ones.

- Lines 548-549: I think it is incorrect to deduce competition ability from this correlation

- Conclusions: The conclusions don't reflect the discussion or even the results presented

- References: There are six not published references in the list, and some of them have data with a lot of weight in the discussion.

- Table 1: Better put the PP units on the table. It would be really helpful to group the rows by region, so it is easier to follow the description in Lines 403-415.

- Figure 1: a more general map to locate the cruise area would be useful. In the legend, the fourth line will be easier to read using "respectively".

- Figure 2: I suggest using the same units for the two panels to make them directly comparable. Put stations number in both plots.

- Figure 3: * The units on the panel B are incorrect, it is mmol. * Why are data at station 13 missing? I don't find the explanation. * As I understand, the bars on figure 3 represents the values of each variable in every station, so these plots are not histograms, they are bar charts. * Bars should be represented by the corresponding error bars.

-Figure 4: this figure needs to be improved. Some suggestions: * Make the station names consistent with other figures. * Group the profiles by region, with color or pattern, or make 3 panels, one for each region. * Talking about this figure in the text (line 269) authors use the phosphacline. Represent the phosphacline here to help. * I would incorporate the profiles for the Long stations in figure 9, as you use the information in the discussion of the experiments.

- Figures 5, 6 and 7: * Vertical axis in the second panel should say fluorescence instead

of chl, as you explain in the text (e.g., line 280) * When describing these figures in the text, you use the density. Including the density level lines in the figures will be a good idea. * Explain in the legend what the vertical lines in the second panel mean, and make them the same color than the profiles in the third panel.

- Figure 10: What is it that you represent in here is not clear for me, not in the text or the figure legend. Please make the description of the calculation more clear and explain better the meaning and the interpretation. Use station names consistent with the rest of the paper.

Technical corrections - Line 23: space is missing between With and N2 loom - Lines 94 and 97, the period is missing - Lines 110 and, the first n in Lagrangian is missing - Line 119: I think authors meant experiments (in plural) - Line 234: a bracket is missing before the references - Lines 236-246 are confusing and need to be rewritten. - Line 249: "Averaged per SD station, the dcm fluctuated..." I don't understand what that means and what the following ranges refer to. - Line 269: Vertical profiles cannot increase or decrease with depth, I assume you are talking here about TDIP decreasing with depth. Please rewrite. - Line 278: Please, check this sentence. Space is missing between down and in. "Comumn" I suppose means columns with l. There is a comma instead of a period after the bracket. The sentence doesn't make sense in general. - Line 287: delete the bracket before IPPDECK - Line 327: PP instead of BP - Line 345: Those abundances, I guess are bacterial abundances - Line 346: a decimal point is missing in 014 - Line 347: the lowest instead of the lowerest instead of the lower

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