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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.

Anonymous Referee #2

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In this article the authors present the results from a study into bacterial and primary production in the tropical south Pacific ocean. The paper fits perfectly within the scope of Biogeosciences. I found the article interesting to read with some very interesting insights into the carbon balance of this part of the Tropical South Pacific, a region that has been rather less studied than some of the other oceanic provinces.

While the actual methods used can be considered as relatively classic in the domain, their application to this little studied area is novel. Indeed, although several authors have worked in the Tropical South Pacific, the vast majority of these studies have looked

at either N2 fixation alone or have been conducted in the coastal areas near to Islands. This data from the open ocean is particularly interesting and novel. The assumptions of the methods are appropriate and are clearly outlines.

I am wondering why was the ratio 400ml of bacterial 'inoculum' chosen for addition to 2.6L?

The conclusions are approriate and provide some interesting insigths into what is limiting bacterial production in this part of the ocean. Notably, it appears that available N is the limiting factor - which of course underlines the importance of N2 fixing organisms in this environment, as has been already shown by other work from this group.

I was a little perplexed as to why some results were shown in the methods section Pg 4, line 135.

The results section is sufficient to support the conclusions - I have one comment here though - it was a little awkward to have quite a few associated datasets were in other articles - it was a bit difficult to do a "stand-alone" review. But the authors do clearly give credit for other work and they clearly indicate what are their new additions.

The experiments and calculations are well described and will allow for replication by other scientists.

The Title clearly reflects the contents, particularly if we take into account the whole group of papers from the Outpace experiment.

The abstract is clear but I wondering if the last sentence should not appear earlier in the text, it does seem to be a little be deconnected from the rest of the text. Perhaps the authors can rephrase it if they wish to leave it as a last sentence or move it up.

Yes, the article is well structured, clear and I really enjoyed reading it. The language is fluent and clear and the appropriate formulae and correction factors used are presented clearly when needed.

concerning the tables : Table 1 and 2 : both of these tables are a little blurry - maybe check that in a revised version? Also, can the authors add the units into the table (I know they are in the legend, but I always find it easier to follow when they are in the table itself). Table 5 : can the authors add in if its the mean +/- the SD or the SE? Figure 1 : is a little hard to see - but maybe it's my printout - nevertheless, can the authors check that the figure is clear and not blurry. Figure 3 : check the format of the legend titles (add in uppercase letters when needed). Figure 4: why did the authors choses to put in the black dotted lines? It rather draws the eye at the cost of the other profiles.

Overall, can the authors unify the format of the axis titles on the figures - some have () some do not. Also can they check the clarity of the contour maps and the colour of the words/numbers on the graphics - sometimes they are hard to read (see figs. 5-7b).

pg 9 line 341 : non signficant for PP

line 250 : what do the authors mean here 'determined by fluorometry'? Don't both methods employ fluorometrey (Turner vs CTD)?

pg 11, line 420 : 6-12% is not that low. line 440 : check spelling of Lemee here (it's ok in the Refs).

Paragraph starting 455: negative NCP values have also been observed in the oligotrophic water off-shore of New Caledonia (Pringault et al. Biogeosciences 2007. I agree with the authors that calculating up hourly incubation values to daily ones is fraught with errors. Do the authors have an estimate of how much error may be introduced from these factors? It is interesting to note that Prochlorococcus could be responsible for up to 56% of leucine uptake - this could have some very strong implications for BCD calculations and hence, ecosystem metabolism calculations in areas where Prochlorococcus is abundant. What about the diazotrophs? Do they take up leucine? Is there any information on this?

530 : what is an artificial diazotroph culture? 570: not sure what the authors mean here in the sentence starting "They also showed..." - can the authors revised this? 589: what do the authors mean by "highly diverse metabolic status" - maybe clarify the meaning here.

The references are appropriate.

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