

Response to Anonymous Referee #2

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 outlined.

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

It was a typo error that we have corrected. We added 400 ml in a volume of 1.6 L so that the dilution factor was 20%. In oligotrophic environments, adding only 10% usually leads to a very long lag phase.

The conclusions are appropriate and provide some interesting insights 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.

In the M&M section, we wanted to justify why we used in vivo fluorescence only to describe shape of vertical profiles, depths of dcm, high frequency time evolution, whereas discrete measures of chlorophyll by fluorometry were used to estimate and compare chlorophyll biomass stocks. This is why we prefer to keep this information in the M&M. However, it does not need to be developed to a great extent, and the last paragraph was reduced as follows: 'Due to the heterogeneity at the time of sampling and the nature of the populations present, i.e. essentially different fluorescence yields over depth and species (Neveux et al., 2010), the overall correlation of in vivo fluorescence (chl iv) with chl a was very patchy ($\text{chl a} = 1.582 * \text{chl iv} + 0.0241$, $n = 169$, $r = 0.61$). Thus in vivo fluorescence was used to track high frequency variability at the LD sites, the shape of vertical profile's distributions and the location of the dcm, as well as longitudinal trends. But fluorometric discrete data (chl a) was always used when calculating and comparing integrated stocks.'

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 their new additions are.

This is inevitably a problem during the process of special issues. Among the 6 publications not published in December 2017, 3 of them are now submitted in Biogeosciences Discussions and one in press has been published:

Bonnet, S., Caffin M., Berthelot H., Grosso, O., Benavides, M., Helias-Nuninge, H., Guieu, C., Stenegren, M. and Foster, R.: In depth characterization of diazotroph activity across the

Western Tropical South Pacific hot spot of N₂ fixation, Biogeosciences Discuss., doi.org/10.5194/bg-2017-567, 2018

Dupouy, C., Frouin, R., Tedetti, M., Maillard, M., Rodier, M., Lombard, F., Guidi, L., Picheral, M., Duhamel, S., Charrière, B., and Sempéré, R.: diazotrophic *Trichodesmium* influences ocean color and pigment composition in the South West tropical Pacific, Biogeosciences Discuss., doi.org/10.5194/bg-2017-570, in review, 2018

Moutin, T., Wagener, T., Caffin, M., Fumenia, A., Gimenez, A., Baklouti, M., Bouruet-Aubertot, P., Pujo-Pay, M., Leblanc, K., Lefevre, M., Helias Nunige, S., Leblond, N., Grosso, O. and de Verneil, A.: Nutrient availability and the ultimate control of the biological carbon pump in the Western Tropical South Pacific Ocean. Biogeosciences Discuss., /doi.org/10.5194/bg-2017-565, 2018.

Tenorio, M., Dupouy C., Rodier, M., and Neveux, J. Filamentous cyanobacteria and picoplankton in the South Western Tropical Pacific Ocean (Loyalty Channel, Melanesian Archipelago) during an El Nino episode, Appl. Microb. Ecol., doi.org/10.3354/ame01873, 2018

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 disconnected 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.

This sentence was moved upwards in the abstract

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?

It is because we had to paste an image in an A4 format whereas the initial table was in a Landscape format, not accepted in the edited version. We will work with the editor to make sure that our tables appear clearly in the final version of the manuscript. Dealing with the content of Table 1, we also organized referenced areas in a more logical order, roughly from west to east.

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).

It is done

Table 5 : can the authors add in if its the mean +/- the SD or the SE?

It is ± SE. It was added on the Table 5

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.

In the original ppt version, figures are not blurry. In addition, we made a new version of figure 1 where letters are better contrasted and the different groups of stations (WMA, EMA and WGY) are indicated. See the new Figure 1 at this end of the responses to the reviewer's comments

Figure 3 : check the format of the legend titles (add in uppercase letters when needed).
This has been corrected

Figure 4: why did the authors choose to put in the black dotted lines? It rather draws the eye at the cost of the other profiles.

We made a new Fig.4 (please see at the end of the responses to the reviewer's comments)

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).

Parenthesis for units have been added everywhere. In their original version in ppt format, the Figures are not blurry We will take care of that during editing process of the revised version

pg 9 line 341 : non significant for PP

This has been corrected

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

Yes, you are right. The term 'determined by fluorometry' was removed

pg 11, line 420 : 6-12% is not that low.

We removed the term 'low'

line 440 : check spelling of Lemee here (it's ok in the Refs).

This has been corrected

Paragraph starting 455: negative NCP values have also been observed in the oligotrophic water off-shore of New Caledonia (Pringault et al. Biogeosciences 2007).

Yes, this reference was added in the ms.

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?

Diel variability of BP was studied in the eastern south Pacific gyre at three sites: in the open sea away from Marquesas Islands, in the center of the South Pacific gyre, and in the eastern part of the south Pacific gyre (Van Wambeke et al., 2008). At these sites, a Lagrangian sampling strategy was used and we followed BP every 3 h up to 72 h long. From this study, the coefficients of variation (SD/Mean ratio) of euphotic zone-integrated hourly BP were 13, 16 and 19%. With the number of profiles varying between 9 and 16 per site, the standard error (SE represented on average 5 % of the mean. We used this value in the context of the OUTPACE cruise to estimate the SE introduced by the conversion from hourly to daily bacterial carbon demand, using propagation of errors (SE. related to triplicate variability of BP, and SE related to daily variability). The corresponding SE is now plotted in Fig. 3a. We also added this information in the text.

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?

Yes, co-author S. Duhamel did some cell sorting of *Crocospheera*-like cells which peaked at 60 m at the site LDC. She detected significant uptake of leucine by these cells in light and dark conditions. Although the activity was significantly detected per cell, due to their low abundance, their participation to the bulk uptake of leucine was very low. These results will be presented in a manuscript in preparation outside of this Special Issue. Moreover, assimilation of dissolved organic nitrogen labeled with ^{15}N and observed by Nanosims technique also suggested assimilation by *Trichodesmium*, although the hypothesis that this labeling could be obtained indirectly after a transfer via epibiotic heterotrophic bacteria could not be ruled out (Benavides et al., 2017).

530 : what is an artificial diazotroph culture?

We wanted to insist on the fact that the information did not concern natural environment. Of course a culture is not e natural environment. We removed the term ‘artificial’

570: not sure what the authors mean here in the sentence starting "They also showed..." - can the authors revised this?

The sentence was modified as:

‘They also showed a highly dynamic *Crocospheera* growth and decay during diel cycles survey, suggesting rapid switch between cell growth and mortality processes, such as grazing and viral infection.’

589: what do the authors mean by "highly diverse metabolic status" - maybe clarify the meaning here.

This last sentence was removed

New versions of some Figures

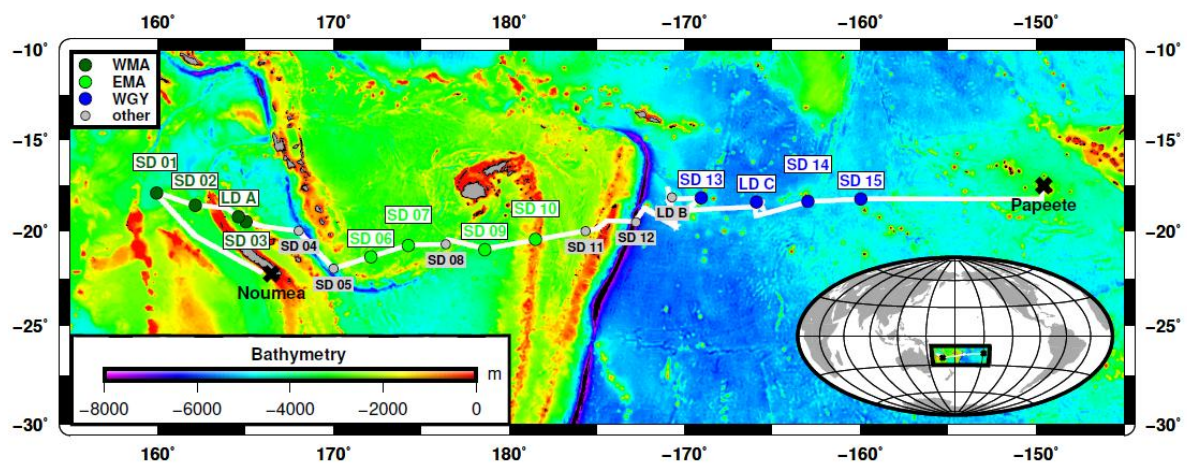


Figure 1 Stations locations during the OUTPACE cruise. The white line shows the ship track (data from the hull-mounted ADCP positioning system). In dark green WMA (western Melanesian Archipelago) included SD1, 2, 3 and LDA; in light green, EMA: eastern Melanesian Archipelago included SD 6, 7, 9 and 10 and in blue WGY (western gyre) included stations SD13, 14, 15 and LDC. Figure courtesy of T Wagener.

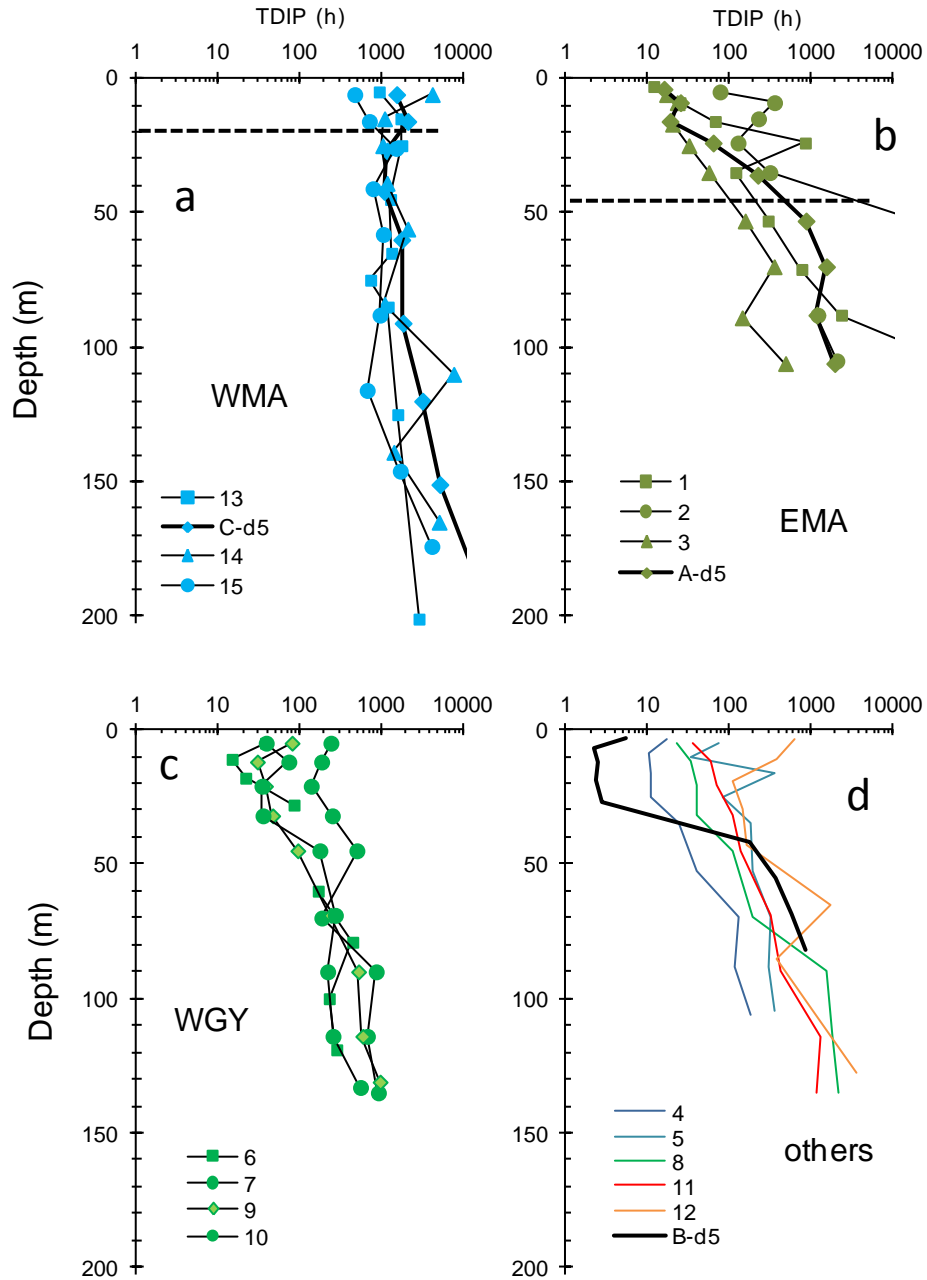


Figure 4 Vertical distributions of phosphate turnover times (T_{DIP}) in groups of stations WMA (a), EMA (b), WGY (c) and other stations (d). At the long-duration stations LDA, LDB and LDC, T_{DIP} profiles were determined at day 5 (bold lines). Horizontal bar in a (WMA) and b (EMA) delineates the mean phosphocline depth (mean \pm SD: 20 ± 7 m, and 44 ± 10 m, respectively) as determined by Moutin et al. (2018). At WGY (c), DIP concentrations were > 100 nM at all depths.