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Interactive Comment

## *Interactive comment on* "Heterotrophic bacterial production in the South East Pacific: longitudinal trends and coupling with primary production" *by* F. Van Wambeke et al.

## Anonymous Referee #3

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The authors here report, for the first time, primary production, net and gross production (from oxygen) and bacterial production and abundance in the South East Pacific. As pointed out by the authors, ocean color data indicated that this oceanic regime is one of the most oligotrophic on the planet, yet more direct measurements are lacking. This paper is important because it provides basic data on both phytoplankton and bacteria, both standing stocks and production (the authors rightly assume that the prokaryotes are dominated by bacteria, but this should be mentioned explicitly in the paper). The authors also did several control experiments on measuring primary and bacterial production (e.g. they compared deck with in situ incubations for primary production). Perhaps most noteworthy is the net production data, which the authors use to comment on



the on-going controversy of heterotrophic oceans. But the authors reached the wrong conclusion with these data.

The last part of the abstract says that net community production was negative and there must be external inputs of carbon or some uncoupling between respiration and production. But the net rates were -13 + 20 to -37 + 40 nmol O2/m<sup>2</sup>/d. Clearly with these errors, the authors cannot conclude that -13 and -37 are statistically different from zero. In Table 2, the authors report for station UPX1 a net community production of -38 + 23. This rate may be significantly different zero, although I'd like to see a statistical test (I'm not 100% sure that a simple t-test is the right test.) But overall, the data on net changes in oxygen indicate that the South East Pacific is in metabolic balance.

The other aspect of the paper that I take exception to is the reporting of bacterial growth efficiencies (BGE). The only way I'm aware of for estimating this important factor is to measure respiration and biomass production in the bacterial size fraction. But the authors measured respiration in size fractionation experiments only at one station (they did so much on other parameters!) and found that bacteria accounted for all of the measured respiration. They then used applied this observation to the other stations, although it is true that Table 3 gives BGE estimates assuming that bacteria account for both 50% and 100% of total respiration. Regardless, I think they have too much about BGE given that they did only two size fractionation experiments at one station.

Finally, the authors should consider adding more discussion and a figure and table or two about phytoplankton (primary production and chlorophyll). The authors have lots about the bacteria, which is great, but the phytoplankton are the base of the food chain. If forced to pick one microbial group to focus on, I'd have to go with phytoplankton.

Specific comments.

1. page 2772, lines 3-4: The authors say that with different BGE assumptions, there was a "decrease in the BCD/IPP ratios from 2.1 to 8.6". Do they mean from 8.6 to 2.1

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or is there some other typo? It seems that Figure 5 should be cited for this-it seems to be the source of the data for the BCD/IPP ratio.

2. Table 3. As mentioned above, I have problems with reporting BGE values for all these stations. Also, since the variation in respiratory quotient and photosynthetic quotients is small compared to other possible errors and variation, I suggest just using one value for these parameters and not report the range. The range makes it harder to compare the numbers.

Perhaps more importantly, I don't understand how "GCP"; was calculated. The GCP of Table 3 cannot be the same GCP in Table 2. This needs to be explained better, and a different term and abbreviation have to be used to distinguish these values from the net oxygen data in Table 2.

Clearing this up is essential because it bears on a very important observation. The authors observe, I believe, that bacterial carbon demand (BCD=BP/BGE) exceeded 14C-primary production, but it was usually much less than the GCP reported in Table 3 (but not in Table 2).

3. Analogous to Table 4, I think the authors should have a table giving a few examples of chlorophyll and 14C-primary production rates from other oligotrophic oceans. Is the South Pacific the most oligotrophic ocean?

4. Analogous to Figure 2, I suggest that the authors give contour plots of chlorophyll and 14C-primary production, unless these data are given in another paper.

5. Figure 5. At a minimum, the same scale should be used for both bacterial and primary production. The scales differ by only 2-fold, not enough to use different ones, and it would greatly facilitate the two rates.

But I wonder if a scatter diagram would be more interesting. Other figures, especially if the authors add contour plots of chlorophyll and primary production, would show how production varied along the transect. A scatter diagram would show directly the

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relationship between bacterial and primary production. The authors could add lines for BCD equaling primary production (perhaps two for different BGE assumptions).

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