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Biogeosciences
Discussions

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 #2

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This ms. presents bacterial production and respiration data for the most oligotrophic oceanic region, the southern Pacific. The data are compared to primary production and inferences made about the metabolic balance of this oceanic region. Since the region hadn?t yet been explored for this type of data, the values are welcome, and the ms. certainly deserves publication.

The data are correct, even if needing many conversions. Most of the authors' choices are well explained, so there is not much to say. If anything, I believe the sentence in the abstract ?such imbalances being impossible? is not warranted and should disappear from the Abstract. Two things I found a bit weak were i) the lack of determinations of leucine-to-carbon conversion factors, since so much effort was done to determine carefully all variables. However, a discussion is done about how lower CF would affect determinations of BP, and I thus find the issue relatively well solved. And ii) the fact that only on 2 occasions (of a single station) whole community respiration was compared to BR. I can certainly anticipate that in waters as distinct as those studied, the assumption that most or all DCR is BR is probably a source of error.

The corrections of BP to be applied in the determinations of BGE (page 2771, l. >25) are unclear. It's not told what is corrected and which is the real estimate used to compute BGE.

Perhaps the section I found less convincing was that in page 2776. I do not think it is proper to compare O2 derived gross community production with 14C-derived PP as a way to estimate dPP. There is, further, a bunch of papers (several by Moran et al, and several by Teira et al) that have good empirical relationships between PPP and dPP. Both predict that with very low CHL dPP is a very large fraction of TPP. Particularly useful for your computations are the Moran papers because he used a method that accounted for bacterial reassimilation of the excreted dPP. And yes, I would presume that dPP in the south pacific is a very large fraction of total PP. Then, I can't buy your argument that the fact that BP is maximum at around midnight indicates that bacteria are using the large fraction of dPP. An equally likely explanation is that BP is low during the day because of the detrimental effects of light on heterotrophic bacteria.

By the way, it is quite difficult to review a paper that continuously refers to unpublished papers. The reviewer should have access to everything that is used in the argumentation, but can't judge on the authors' unpublished work.

Finally, I missed a graph with the relationship between PP and BP as compared to the classical Cole et al. 1998 relationship.

Minor points.

- I had a lot of trouble at matching Fig. 1 with Table 1. The numeric codes are repeated in Table 1, so I suggest you just choose one nomenclature and use only this one all over.

- page 2765, l. 6. Are samples ?counterstained?? No, they are just stained !

- page 2776. Details of precision of BP measurements should have been explained in the M&M section, not here.

Interactive comment on Biogeosciences Discuss., 4, 2761, 2007.