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Biogeosciences Discussions

Interactive comment on "Heterotrophic bacterial production and metabolic balance during the VAHINE mesocosm experiment in the New Caledonia lagoon" *by* F. Van Wambeke et al.

Anonymous Referee #2

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The paper by Van Wambeke et al. is one of a series of papers describing the complex mesocosm experiment VAHINE organized by S.Bonnet in New Caledonia. This paper deals with the coupling between N2 fixation and bacterial production in the mesocosms. I have to admit I am not an expert on quantitative dynamics of heterotrophic bacterial communities and I do not follow this field in detail. I always felt that the quantitative estimates of turnovers and efficiencies of bacterial stocks involve too many assumptions that question the validity of the results. Still, I found that this paper uses some interesting and for me new approaches that I am happy I had a chance to read the paper.

To give some constructive criticism, I feel that there are some unnecessary overlaps with other papers in the VAHINE series. For example, the abstract states that in this paper the authors examined relationships between N2 fixation rates and primary production. . .". However, this was done in detail in the accompanying paper by Berthelot et al. Also, some data from Berthelot et al. paper are repeated here.

Text p.19874, l 20- I found confusing the description of data for cell-specific rates of leucine incorporation. Where are the data for heterotrophic bacterial phytoplankton LNA, HNA and hi-HNA shown? Table 3 provides data for autotrophic phytoplankton cells. Since this constitutes important part of the paper, I strongly recommend to clear this issue in the text and in the Table 3.

As far as I can see, the authors mix bacteriochlorophyll and rhodopsin. The paper by Hauruseu and Koblizek (2012) studied photoheterotrophic AAP bacterium Erythrobacter that contains bacteriochlorophyll, not rhodopsin. The contribution of photoheterotrophy to overall energy budget of AAP bacteria is different from flavobacteria containing proteorhodopsin like SAR11. So I suggest the authors correct this in the discussion text on p. 19881 and in the abstract as well. It is a pity the experiment did not look for the response of bchl cotainig AAPs to the nutrient enrichment. Also, at the time of the review I could not locate the paper by Pfreundt , Sungin, Bonnet, Berman-Frank and Hess that should be already on the BGS web.

I am missing some discussion / explanation for the data that are out of the trend, for example, what happened on day 11 with HBA abundances? Why was the mesocosm M3 so different?

Regarding the model calculations for net autotrophy shown in Fig.7 – I found it highly speculative. The overall balance depends on the assumption that GPP= PP x 1.72. But is this valid for the siutation in the NC lagoon? The relation between GPP and PP (better NPP) given as NPP/GPP can range anywhere from 0.2-0.6...

Minor comments: p.19866, l.27: "located 28km off the coast"

p.19872,l.7: “occurred during P2”

In the Legend to Fig.7, the Bacterial Carbon Demand is wrongly abbreviated as DCB

Interactive comment on Biogeosciences Discuss., 12, 19861, 2015.