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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 #1

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In this article the authors present the results from an investigation into the impact of diazotrophy on bacterial production (BP) in a mesocosms experiment in the oligotrophic South Pacific. The authors also discuss the degree of coupling between BP and gross primary production (GPP) as well as the fate of organic carbon carbon in the mesocosms. This work forms part of a series of papers that concern the Vahine experiments that have been published in Biogeosciences or BGD. I found that this article has some very nice methodological aspects (3H uptake into specific groups – heterotrophic bacteria and different picocyanobacteria). It is also interesting to note that although the system considered has been proposed to be nitrogen limited for heterotrophic bacteria in other work, there did not appear to be a strong relationship between diazotrophy and

bacterial production.

I have a couple of general comments and then some specific ones that are listed below

1) I have been wondering about controls for the impact of the mesocosm incubation. Perhaps this is addressed in one of the companion papers but it would still be nice to have some comment on it here. Indeed, while the mesocosms used here are of very large volume, the fact that water is contained within these large, vertical "socks" still means that wind, wave and tidal action is reduced. Mari et al. 2007 showed that the residence time in this system played a strong role in determining carbon export and perhaps the authors could comment on this aspect, particularly as regards their partitioning estimates in the later part of the discussion.

2) In this work the authors looked at heterotrophic bacteria. Did the authors separate the free-living and attached fractions? Do the authors have any thoughts on how the attachment of bacteria to particles (I am thinking of those that are tightly associated (or attached) to the autotrophs in the mesocosms) might alter the fluxes? I am thinking specifically about the determinations of uptake of leucine into the Prochlorococcus and Synechococcus cell sorted fractions. Does the cell sorting separate off the associated bacteria? What about other small phytoplankton cells?

3) The bacterial production estimates were all conducted in the light and in the discussion the authors provide an interesting discussion of the implications of the work. However, for the export calculations and for the BCD and BGE calculations, the authors determine daily rates of production by multiplying by 24h the rates. Did the authors check for differences between light and dark uptake of leucine? If N2 uptake occurs in the light (as with primary production), and if we suppose that the highest rates of DDN release occur during N2 fixation, then one could suppose that BP would be higher during the light due to the supply of readily available N, which would in turn give higher BGE estimates. Given the link between PP and BP that was observed here, it appears that this is at least true for primary production, but is this the case for N2 fixation? Can

the authors comment on this aspect and on how it would alter their calculations? Indeed, it is rather intriguing that the heterotrophic bacterial production does not seem to be tightly coupled with DDN, despite the N limitation observed.

4) The authors have used the relation of GPP= PP * 1.72. If I understand correctly this means that over 40% of gross primary production is lost to cellular respiration and the release of dissolved production. I saw that the article has a French reference, can the authors perhaps add a little explanation of how this value is determined and where it was determined. Given the importance of this calculation for the rest that follows (Fig. 7 and the calculations of respiration, etc), it is critical that more information is provided one how this is determined to allow the reader to see how the calculations in Fig. 7 and the associated text were made. Indeed, even small differences in this value will have an effect on the determination of the other factors, if I follow correctly the text. Perhaps a sensitivity calculation would be interesting here to show how robust the calculation is ?

Specific comments:

19871, line 18 : No vertical structure was observed in the water column in the mesocosm. Was this also the case in the water outside of the mesocosms?

Why is M3 so different?

19878, section 4.3: I am wondering if the authors measured the dissolved fraction of primary production during the PP measurement? The complete method is cited as being in an associated article, but it would be nice to have it noted in the text.

19880, line 3: Coral mucus can also be an important source of organic matter in coral reef systems (Wild et al.)

19878, line 25 : change to “and” from “ad”

19879, line 9: maybe change the format of the () around log BP and log PP.

The authors do not really discuss the AP activity although it is present in 2 figures (3 and 4). Perhaps they can add a short interpretation of these results to the discussion. Do they have any thoughts on why it may have decreased in the lagoon on Day 5? Could there have been an input of P? From what source? Atmospheric deposition? This apparently can be an important factor in stimulating production and sinking flux in this system (Mari et al. 2014). Concerning Fig. 3, I am wondering if it is necessary that it is in the main body of the text. Perhaps it could be in the Supp. Mats section. At present there are quite a lot of Figs and Tables and as it shows almost the same information as is in some of panels in Fig. 4, perhaps it could be moved to the Supp mats.

Similarly, perhaps Table 2 can be removed as if I understand correctly the same information is presented in Fig. 5.

Can the authors add some lines to the graphs to show when P1 and P2 occurred in the Figs?

Which sample(s) are shown in Fig. 2?

Concerning Table 4, I appreciate the honesty of the authors to say that some outliers were removed and they provide the values. What do these outliers correspond to (manipulation error? Contamination? Resuspension?) How was it determined that they were outliers?

In the conclusions, the authors also refer to the need for techniques that are adapted to the measurement of bacterial respiration in the light. Have the authors looked at the work of Pringault et al. from the Southwest Lagoon of New Caledonia?

Refs

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