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Interactive comment on “Primary Productivity and heterotrophic activity in an enclosed marine area of central Patagonia (Puyuhuapi channel; 44° S, 73° W)” by G. Daneri et al.

Anonymous Referee #1

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The manuscript by Daneri et al describes a comprehensive study of primary production, community respiration and bacterial production over a seasonal cycle in an enclosed marine area of central Patagonia. The authors present an interesting set of data, however the manuscript lacks of clear hypotheses/objectives, and a fully adequate discussion of their results. There are also some weaknesses in the estimation of bacterial production.

As I understand, the authors use the term bacterial secondary production (BSP) to refer to bacterial carbon demand; I suggest using the standard terminology to avoid confusions (bacterial production, BP, bacterial respiration, BR, and bacterial carbon

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demand, BCD). Moreover, the authors do not measure BR and they use a BGE derived from the equations of del Giorgio and Cole (1998) and Kritzberg et al (2005). It is well known that different models may provide contrasting BGE estimates (e.g. Rivkin and Legendre 2001, López-Urrutia and Morán 2007, Robinson 2008), and it is not clear why the authors choose these two models, which provide very similar and rather low BGE estimates. For this reason I do not find appropriate the discussion about BGE and the %BCD/GPP. On the other hand, they also use two different leucine to carbon conversion factors (CFs). As the authors may know, a high variability in empirically determined CFs has been reported in many ecosystems; therefore, it is always preferred to estimate the conversion factors rather than using literature values. For the purposes of the paper, I suggest just providing raw BP rates (in leucine units). If the authors wish to have an estimation of the BP/GPP ratio, as a measure of the importance of heterotrophic bacteria in consuming primary production, they can use a range of published CFs, and provide the corresponding range of BP/GPP ratios. In any case, they must clearly address the limitations of using literature CFs in the discussion.

Specific comments. Abstract. The abstract should be more concise, clearly indicating the aim of the study and their main results and conclusions. The authors even do not mention the mean GPP/CR ratio, which appears to be balanced during the productive season and heterotrophic during the non-productive season. A balanced or heterotrophic GPP/CR contrasts with what the authors state in the conclusions (page 5950, lines 16-20). They also do not mention the correlation between BP and river discharge and/or DOC concentration.

Introduction. The authors should more clearly indicate the purpose of their study, indicating clear hypotheses and/or objectives, not just listing what they did.

Mehods. Page 5938, line 8. Why did the authors incubate only during the light period?. Page 5938, line 28. Why did the authors use 50 nM as saturating concentration? Did they check that for the sampling area?. Page 5939, line 21. Why did the authors use non-parametric analyses? The authors must clarify this section.

Results. In general the authors should first clearly describe the results presented in the figures and then present statistical results. Also note that table 2 is cited before table 1. It makes no much sense presenting GPP and CR data before chlorophyll-a and phytoplankton abundance. Page 5942, lines 1-5. The authors present here correlations of GPP with nutrients before describing the general patterns of GPP. Page 5943, line 17. There must be an error in the units. Page 5944, line 26. As already commented, I do not find appropriate the use of these two models for BGE estimates. I suggest just presenting BP in leucine units. I would remove table 1, as these data are represented in figure 9. Page 5944, line 17. This is interesting, unfortunately the authors only have 4 pCO₂ profiles, 3 in productive and only 1 in un-productive periods.

Discussion. Overall the discussion is too much centered in the seasonal variability of environmental factors in relation to phytoplankton and GPP and much less in CR, GPP/CR, or BP. The authors should rather look at the correlation between BP rates (not depending on BGE estimates) and GPP and/or chl_a, and the ratio BP/GPP to address the degree of coupling between phytoplankton and bacterioplankton. The authors do not discuss that on average the sampling site shows a balanced GPP/CR, which contrasts with studies indicating that the Patagonian region is a net sink of atmospheric CO₂. The authors should better discuss this important issue. As already indicated, it is not adequate to discuss much on BGE variability as it is derived from two particular models. The authors should just discuss about seasonal variability in BP. Also, the discussion about what control GPP/CR is not adequate. Obviously both GPP and CR control the GPP/CR. In addition, the correlations presented in figure 6 might be spurious as the X-variable is part of the Y-variable.

Figures and tables. Table 1 can be removed as it is redundant. Table 2. The authors must provide an explanation about removing the March and November experiments from the analyses. Figure 5. The authors could add the GPP/CR. Figure 7. I suggest representing the contribution of different size classes to total chlorophyll as % for better clarity. Figure 9. I suggest representing raw BP rates (in leucine units).

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