**Biogeosciences
Discussions**

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 #2

General comments

This is my second review of this manuscript. The first review was two years ago for
Progress in Oceanography. The paper has now been improved in some features, but it
sill retains the two main concerns that I had already identified in my former review.

Although the objective of this research is not clearly highlighted in the introduction, I
assume that the main objective is to identify the degree of coupling-decoupling between
gross primary production (GPP) and microbial community respiration (CR) in a highly
productive coastal zone. As the manuscript is written right now an additional motivation
was to know how much of this GPP was processed by bacteria. If these are the main

objectives, then the question is why GPP and CR are converted to carbon units? And
why bacterial secondary production (BSP), in fact bacterial carbon demand (BCD), is
estimated and accepted as correct when two factors derived from the literature are
used to obtain BSP values? One of this factors is required to convert leucine units to
carbon units (BP) and the other factor (BGE, bacterial growth efficiency) is needed to
estimate BSP = BP + BR.

At this stage, I would like to partially reproduce my old comment on these issues.

GPP and CR are converted to carbon units using a PQ =1.25 and a RQ = 1. Certainly,
these PQ and RQ values are within the range of acceptable values, but the question is
why these values and not other values, which can also be real, are used? For example,
PQ approaches 1.4 in waters with high nitrate concentrations (like in this case) and
low or undetectable ammonium levels, a situation when synthesis of proteins should
be important. PQ would be close to 1 under very low nutrient levels, with almost all
synthesised organic matter being carbohydrates. Consequently, RQ will be 1 when the
organic matter respired is just composed by carbohydrates. In fact, these conversions
are not required to define the metabolic balance, which can be better characterized
through oxygen units directly. For the specific case of this manuscript, the system
will tend to be more autotrophic or less heterotrophic than the authors say. Carbon
conversions could be used in the discussion to obtain primary production values in
carbon units that could be then compared to primary production values reported for
other coastal zones or to compare GPP and BP. For these comparisons, it would be
better to use a range of possible PQ values to give a range of plausible carbon values,
rather than use a fixed PQ.

As I already mentioned in my old review "This is not a trivial issue, since the authors
use these variables to define the metabolic balance of the system and so infer when
the system acts as source or sink of CO_2": section 3.6 and figure 10 in this manuscript.

The estimates of BSP (or BCD) should be used for discussion purposes only, but not

presented as results, because bacterial respiration was not determined. This discussion should also be written considering a general perspective, always bearing in mind that only BP production was determined. It should also be considered that leucine incorporation was converted to carbon using factors taken from the literature, not using factors experimentally determine in the system.

As the main message of the paper and the discussion are based on the relationships between these 3 variables (GPP, CR and BSP), and the authors have not taken into account my previous (PiO) suggestions, I cannot recommend this manuscript for being accepted in Biogeosciences.

Specific comments

Abstract Line 1. Chl a should be added here, the first time that chlorophyll a is mentioned. Consequently "chlorophyll a" should be removed from line 8-9. Lines 16-17. I cannot see the relevance of stating bacteria and archea.

Introduction Page 5932, lines 3 to 10. Input of freshwater, precipitation and freshwater inputs are mentioned repeatedly in this paragraph. It should be re-written. The introduction should be arranged to clearly show what is going to be studied and why it is important to carry out this type of research in this area. It is also interesting to highlight the importance of the research in a general context. I understand that the introduction now masks the importance of the study, which I understand is to know the metabolic behaviour of the microbial plankton community.

Material and methods Page 5935, line 15. According to figure 3, the number of observations conducted in January were 6, in May were 4, in July 4 and in October 3.

Page 5936, line 16. Surprising, only 3 depths were sample here, when in the version submitted to Progress in Oceanography were 4; 30 m was included at that time. This depth still remains in figure 4 depicting the nutrient distributions.

Page 5937, line 4. Post-incubation should be removed from here.

Page 5937, line 21. Instead of experiments, it should read determinations.

Page 5940, line 6. "Stepwise regression analysis" was not performed or is not shown in this version. This type of statistical analysis was done for the previous version submitted to PiO.

Results Page 5942, lines 3 to 5. This is not the right place to comment about correlations between nitrate and GPP, because GPP was not still shown.

Page 5942. Line 20. The system is in balance on an annual base due to the conversion to carbon units. However, if oxygen units are maintained, the system is autotrophic (GPP = 55.5 mol O2 m-2 y-1 and CR = 44.8 mol O2 m-2 y-1) with a net community production of 10.78 mol O2 m-2 y-1. This oxygen value corresponds to carbon values varying between 129.3 and 92.3 g C m-2 y-1 when PQ is considered also varying between 1 and 1.4. In addition, these annual values of GPP and CR are estimated from the average values given in table 2. Nonetheless, these mean values can contain strong deviations owing to sampling bias. High values of GPP were obtained from only 1 sampling (April, August and November), while lower GPP values were derived from more samplings. Then, the question is: To what extent these single samplings represent the real situation?

Page 5943, lines9-10 and figure 7. It is not easy to follow this description on chlorophyll size-class dominance. Labels in the figure are not clear and sometimes.

Line 16. Leu-1 should be read L-1

This section 3.4 on chlorophyll and phytoplankton should be located before the section on GPP and CR.

Page 5944, section 3.5. This section should include BP only. It should be though that BSP estimated from BGE deduced from del Giorgio and Cole (1998) and Kritzberg et al (2005) did no show differences because BP was probably low, lying on the region where BGE and BP are linearly related. At higher BP values the two estimates will

produce very different BGE values; 0.6 according to del Giorgio and Cole and below 0.4 according to Kritzberg et al.

Page 5944, section 3.6. Although interesting, this section should be removed from the results. Only 3 profiles on pCO2 were determined and they seem very few to develop a specific section.

Discussion Page 5949, lines 1 to 4. All of this is consequence of the factors (BGE and BP) used.

Lines 11 to 21. BGE was not determined and so this discussion is not relevant.

Lines 22 to 29. All of this discussion is biased by the PQ value used.