**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 #3

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Review of Daneri et al 2012

Primary productivity and heterotrophic activity in an enclosed marine area of central Patagonia (Puyuhuapi channel: 44°S, 73°W)

Daneri et al report on the seasonal cycle of salinity, chlorophyll concentrations, community composition and rates of primary production, community respiration and bacterial production in an enclosed marine area of central Patagonia. The authors relate the water column characteristics to biological processes and community composition, and use the balance between production and bacterial removal of organic carbon to determine if the system is a source or sink of carbon dioxide.

General comments: Overall, the manuscript is poorly written. The use of terms is confusing e.g. bacterial production versus bacterial secondary production, and abbreviations are inconsistent. Firstly, I would like to suggest that the authors clarify their use of bacterial production and bacterial secondary production and that the authors report on the abbreviations for all parameters once in the introduction/methods and use throughout the manuscript. 'Chlorophyll' should not be capitalized, except at the beginning of sentences and the authors need to review their use of commas.

The goal of the study or motivation behind making these measurements is not clear. The first sentence of the introduction needs qualified. Why do fjords and estuaries play an important role in biological productivity and carbon cycles? What are the characteristics that lead to this statement? Is it their area or delivery of nutrient rich waters?

Methods: The authors need to add more detail to methods. What is the precision and limits of detection of their techniques? Were samples fixed in Lugols stored in the dark? Glass fiber filters were used to filter samples for silicate analysis, yet it is well known that GFF's release silicate. This may explain why the authors find high silicate concentrations at times when other nutrients are low (bottom of 5941, presently explained by input of freshwater, which should introduce phosphate also)? The authors use 'GF/F' on line 18 (pg 5963) but glass fiber filter on 28. What is 'good data' (line 3 and 10 of pg 5936). Why were there only 3 or 4 depths sampled for nutrients, chlorophyll, rates etc but samples for pH and total alkalinity were collected every meter?

Page 5939, lines 11 to 20. The authors use two approaches to convert leucine incorporation to carbon to derive bacterial production, then two estimates of bacterial growth efficiency to convert bacterial production to 'bacterial carbon utilization'. Firstly, this paragraph needs to be rewritten as it is not clear. Secondly, the authors need to state why they used these specific equations or conversion factors. Thirdly, if the authors want to use two different approaches/equations/conversion factors, they need to compare the output at some stage to determine if they agree, then use one data set in the remainder of the manuscript, or use both data sets and use the difference between

them as an estimate of the error. This processes needs to be very clearly described and clearly written as the rest of the paper, and conclusions based on the metabolism of the system, is sensitive to these conversion factors (see comments by other reviewers). For example, I suggest the authors correlate the data presently reported in Table 1 and insert the correlation statistics into the methods section before using this data to interpret carbon balances. The authors present a four way comparison for BSP using the combination of conversion factors in Figure 9. However, it would be more useful to have a quantitative measure of the comparison (i.e. correlation statistics) and the authors need to rationalize the spread in data in their interpretation (e.g. in January, BSP ranges from $\sim$ 0.8 to 1.8 g C m-2 d-1).

More specific comments are listed below. Overall, the manuscript requires major revision before I can recommend it for publication in Biogeosciences.

Specific comments Pg 5931 Abstract: This needs to be rewritten. The abstract is unclear and use of abbreviations is inconsistent, e.g. Bacterial Secondary Production (BSP) is stated on line 3 and 14, Chlorophyll (and other parameters) should not be capitalized. The purpose of the study is also not mentioned, i.e. why were these measurements made at this particular site over this specific time period?

Pg 5932 Line 10: remove comma after region. Line 12: remove comma after (41-48°S) and check on use of commas throughout. Line 13: PP needs to be defined here, i.e. primary production (PP). Chlorophyll should not be capitalized here or throughout manuscript. Line 27 to 29: change 'increments' to 'increase in' or 'enhanced'. Also on line 11, page 5933.

Page 5934 Line 20: should be 'in terms of' Page 5935 Line 8: names of rivers do not need to be in brackets Line 9: change 'set an' to 'leads to an'

Page 5936 Line 3 and 10: what is 'good data' See above comments on methods.

Page 5937 Line 4 to 15: inconsistencies in use of '-', e.g. 20 μm versus 20-μm Line

12: should read 'Chlorophyll a size fractions', not fractionation.

Page 5942 Line 8 – 10: The authors report the highest values as 4.8-698 μg C L-1 d-1 and lowest values 0.5-308 μg C L-1 d-1. These ranges are large and overlap so it is unclear why they are reported as the highest and lowest, respectively. Please clarify.

Page 5943 Lines 9 to 14: This section is rather descriptive yet data is available to be more quantitative here (in Fig 7). Line 15: Please check units here – cells Leu-1 103

Page 5946: Lines 1-5: This is a circular argument as freshwater input and hydrodynamics determine the light field as they affect water column structure and therefore the amount of light experienced by a phytoplankton cell. Please clarify this statement.

Page 5948 Lines 27: BSP 'decreased' rather than 'fell' from 1 $\pm$ 0.6 to 0.6 $\pm$ 0.3 g C m-2 d-1 and GPP decreased from 1.1 $\pm$ 1.12 to 0.1 $\pm$ 0.1 g C m-2 d-1. Firstly, these errors are large and if the errors are one standard deviation, the differences for both BSP and GPP are not significant between seasons. If this is correct, the authors need to clearly state this somewhere and be cautious in over interpretation of the data.

Table 1: Correlate the data in this table and present in the text. Table 2: Are the errors standard deviations, standard errors, confidence intervals? Please state.

Figure 1: The authors use satellites to construct a time series between SST, PAR and chlorophyll and statistical analysis to determine if a change in water column structure impacts of surface chlorophyll. From Figure 1, it looks like SST and chlorophyll data was taken from the same region (i.e. box) but PAR and wind stress were taken from elsewhere. Does it matter if these measurements were not taken from the exact same region? Is the wind experienced in box QS the same as is experienced in the SST/chl box? In Figure 1, the digits on the bottom right of each panel are not clear.

Figure 4: The poor vertical and sometimes horizontal resolution in sampling skews the data in this contour plot. I suggest either averaging data at each depth for 'summer' and 'winter' seasons and report a mean and standard deviation at each depth between

seasons, or plot vertical profiles

Figure 5 and 7: Do you have estimates of errors that can be added to the bar charts?