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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.

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Referee#1

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

As I understand, the authors use the term bacterial secondary production (BSP) torefer to bacterial carbon demand; I suggest using the standard terminology to avoidconfusions (bacterial production, BP, bacterial respiration, BR, and bacterial carbón demand, BCD). Moreover, the authors do not measure BR and they use a BGE derivedfrom the equations of del Giorgio and Cole (1998) and Kritzberg et al (2005). It iswell known that different models may provide contrasting BGE estimates (e.g. Rivkinand Legendre 2001, López-Urrutia and Morán 2007, Robinson 2008), and it is not clearwhy the authors choose these two models, which provide very similar and rather lowBGE estimates. For this reason I do not find appropriate the discussion about BGEand the %BCD/GPP. On the other hand, they also use two different leucine to carbonconversion factors (CFs). As the authors may know, a high variability in empirically determined CFs has been reported in many ecosystems; therefore, it is always preferredto estimate the conversion factors rather than using literature values. For the purposesof the paper, I suggest just providing raw BP rates (in leucine units). If the authorswish to have an estimation of the BP/GPP ratio, as a measure of the importance ofheterotrophic bacteria in consuming primary production, they can use a range of publishedCFs, and provide the corresponding range of BP/GPP ratios. In any case, theymust clearly address the limitations of using literature CFs in the discussion.

Author reply: The Chilean Patagonia is one of the most extended fjord regions in the world, located on the southeastern border of the Pacific Ocean extending from 41.5˚ (Reloncavi Fjord) to 55.9˚ (Cape Horn) covering 240,000 km2 with a rugged coastline of about 1000 km in a straight line but of ca. 84.000 of coastline (Pantoja et al. 2011). This area, that represents the third largest freshwater reservoir after Antarctica and Greenland is largely undersampled and is highly susceptible to climate driven changes in freshwater input due to accelerated glacial melting. Changes in freshwater dynamics in the area may result in a reduced ability of Patagonian fjord waters to absorb atmospheric CO2 and may fundamentally alter the structure and function of coastal marine ecosystems most of which we are only beginning to understand.

The paper describes the plankton productivity cycle in the Puyuhuapi channel ecosystem; a semi enclosed marine areathat receives important freshwater run off from the Cisnes River. The main objective of the paper is a) to determine the annual plankton productivity cycle using a combination of in situ and satellite derived data, b) to describe the phytoplankton species succession associated to plankton productivity cycles, c) to describe the main forcing factors that control planktonic production, d) to assess the relative contribution of bacteria to pelagic carbon fluxes by examining the coupling between autotrophic and heterotrophic production and taking into account the potential role of allochtonous organic matter in enhancing bacterial production, particularly during the winter,e) to determine the upper layer metabolic balance over a seasonal cycle as a contribution to the understanding of the processes that allow a highly subsidized area (in terms of organic matter input from terrestrial sources) to be a sink rather than a source of atmospheric CO2.

In a revised version of the paper the main objectives and the justification of the work presented will be substantially improved.

Regarding the use of standardised terminology in a revised version we will use the terminology suggested by reviewer # 1; BP: bacterial production, BR: bacterial respiration and BCD bacterial carbon demand.

Regarding the use of literature models to estimate BGE and leucine to carbon factors we are fully aware that this approach have important limitations that do not allow to fully constrain the carbon flow through bacteria. In a revised version of the paper we will follow the reviewer advice in terms of using a wider range of literature derived conversion factors. The revised version of the manuscript will contain a more in depth analysis of the limitations of the methodologywe used.

Referee # 1 Specific comments:

Referee # 1: Specific comments. Abstract. The abstract should be more concise, clearly indicatingthe aim of the study and their main results and conclusions. The authors even donot mention the mean GPP/CR ratio, which appears to be balanced

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during the productiveseason and heterotrophic during the non-productive season. A balanced orheterotrophic GPP/CR contrasts with what the authors state in the conclusions (page5950, lines 16-20). They also do not mention the correlation between BP and riverdischarge and/or DOC concentration.

Author reply: The abstract will be “tighten up” as suggested by the reviewer. The apparent contradiction seem to arise because GPP and CR were estimated on an annual basis by integrating discrete measurements of GPP and CR down to 20 m (roughly corresponding to the depth of the euphotic layer). Within the mixed layer (the mean depth of the mixed layer was ca 6 meters) the GPP/CR ratio was consistently >1 during the productive season.

The correlation between BP and river discharge and/or DOC concentration will be incorporated as suggested

Referee # 1: Introduction. The authors should more clearly indicate the purpose of their study, indicatingclear hypotheses and/or objectives, not just listing what they did.

Author reply: A revised version of the manuscript should indicate more cogently the main aim of the paper

Referee # 1 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? Didthey check that for the sampling area?. Page 5939, line 21. Why did the authors usenon-parametric analyses? The authors must clarify this section.

Author reply: We incubated only during the light period because poaching by artisanal fishermen did not allow us to keep the floating array in the water after dawn (we lost a whole bottle array during our first field work).

Calibration curves to check the validity ofthe concentration (50 nM)we used were performed during the most productive period.

We use non parametric because in the majority of the cases the distribution of the data

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was not normal (Kolmogorov-Smirnov). The bacterial production data however has a normal distribution. This will be re-examine in the revised version of the manuscript.

Referee # 1: Results. In general the authors should first clearly describe the results presented inthe 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 chlorophyllaand phytoplankton abundance. Page 5942, lines 1-5. The authors present herecorrelations of GPP with nutrients before describing the general patterns of GPP. Page5943, 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 dataare represented in figure 9. Page 5944, line 17. This is interesting, unfortunately theauthors only have 4 pCO2 profiles, 3 in productive and only 1 in un-productive periods.

Author reply: The suggestions of reviewer # 1 regarding the order of the presentation of the results will be followed. Tables and figures will be thoroughly checked so as to avoid replicating the presentation of results. The suggestion of avoiding the presentation of BCD based on few BGE results will be replaced by a table with values based on a range of BGE. This will be followed by a more in depth analysis of the potential errors associated with variable BGE and CF values.

The use of Leu instead of L-1 was an editorial mistake.

The pCO2 profiles will not be presented as part of the results but will be mentioned in the discussion section together with references to Torres et al 2011 paper.

Referee # 1 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 chla, and the ratio BP/GPP to addressthe degree of coupling between phytoplankton and bacterioplankton. The authors donot discuss that on average the sampling site shows a balanced GPP/CR, which contrastswith studies indicating that the Patagonian region is a net sink of atmosphericCO2. 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 particularmodels. The authors should just discuss about seasonal variability in BP. Also,the discussion about what control GPP/CR is not adequate. Obviously both GPP andCR control the GPP/CR. In addition, the correlations presented in figure 6 might bespurious as the X-variable is part of the Y-variable.

Authors reply: As suggested by the referee we will discuss in more depth the seasonal variations in heterotrophic activity and the potential role of bacterioplankton carbon flow cautioning about the potential errors associated to model derived BGE values. The degree of coupling between carbon synthesis and usage by the planktonic community will be stressed. The metabolic balance (defined as the GPP/CR ratio) will be discussed in terms of mixed layer and euphotic layer trophic status. The mixed layer GPP/CR ratio was consistently$>$1 during the productive season while for the same season the euphotic layer GPP/CR ratio seems to be balanced. During the non productive season the GPP/CR ratio was $<$1. The importance of an autotrophic mixed layer will be discussed in terms of the demonstrated capacity of Patagonian fjords to sequester atmospheric CO_2.

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

Authors reply: we will follow the recommendation of referee # 1 regarding Table 1 if in the revised version of the manuscript the presentation of data is duplicated.

The (*) of table 2 was meant to indicate that during March and November we only had Primary Production and Community Respiration values. We will modify this legend

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accordingly

Referee#2

This is my second review of this manuscript. The first review was two years ago forProgress in Oceanography. The paper has now been improved in some features, but itsill 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, Iassume that the main objective is to identify the degree of coupling-decoupling betweengross primary production (GPP) and microbial community respiration (CR) in a highlyproductive coastal zone. As the manuscript is written right now an additional motivationwas 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? Andwhy bacterial secondary production (BSP), in fact bacterial carbon demand (BCD), isestimated and accepted as correct when two factors derived from the literature areused to obtain BSP values? One of this factors is required to convert leucine units tocarbon units (BP) and the other factor (BGE, bacterial growth efficiency) is needed toestimate 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 iswhy 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) andlow or undetectable ammonium levels, a situation when synthesis of proteins shouldbe important. PQ would be close to 1 under very low nutrient levels, with almost allsynthesised organic matter being carbohydrates. Consequently, RQ will be 1 when theorganic matter respired is just composed by carbohydrates. In fact, these conversionsare not required to define the metabolic balance, which can be better characterizedthrough oxygen units directly. For the specific case of this manuscript, the systemwill tend to be more autotrophic or less heterotrophic than the authors say. Carbonconversions could be used in the discussion to obtain primary production values incarbon 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 authorsuse these variables to define the metabolic balance of the system and so infer whenthe system acts as source or sink of CO2”: 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 discussionshould also be written considering a general perspective, always bearing in mindthat only BP production was determined. It should also be considered that leucine incorporation was converted to carbon using factors taken from the literature, not usingfactors experimentally determine in the system.As the main message of the paper and the discussion are based on the relationshipsbetween these 3 variables (GPP, CR and BSP), and the authors have not taken intoaccount my previous (PiO) suggestions, I cannot recommend this manuscript for beingaccepted in Biogeosciences.

Authors reply:several general observations made by Referee#2 coincide with observations made by referee #1.

As indicated in the reply to reviewer # 1 the objective of the research will be more clearly highlighted in the revised version of the paper. The main objectives/aims of the paper have been described in our reply to referee #1 above.

As requested by referee#2 the balance between GPP and CR will be analysed in terms of dissolved oxygen concentration. This exercise has already been done and the oxygen data show that overall, the system is autotrophic within the mixed layer during the productive season becoming heterotrophic below the mixed layer and during the non productive season.

The annual estimated GPP and CR for the euphotic layer needed to compare the productivity of the Chilean fjord with other ecosystems (including the Humboldt current system to the north)will include a table with the range of values obtained under different conversion factors (i.e. range of PQs from 1 to 1.4 and RQ from 0.7 to 1).

The trophic balance of the mixed layer is important to understand why the fjord area that seems to be heavily subsidised by terrestrial organic matter is a sink of atmospheric CO2. This situation may change as climatically driven changes may alter the levels of freshwater input to the fjords.

The concerns expressed by referee#2regarding the estimation of BGE and Leucine to carbon conversion factors are similar to the concerns expressed by referee # 1. The changes that will be adopted in a revised manuscript are indicated in the reply to referee#1 above.

Referee # 2 Specific comments

Referee #2: 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. Icannot see the relevance of stating bacteria and archea.

Authors Reply: Chla will be added in the abstract. Since bacterioplankton is a very heterogenous group and sincearchea is an important component of the planktonic community in Chilean fjords we consider that it is correct to define bacterioplankton as a group composed of bacteria and archea

Referee#2 Introduction Page 5932, lines 3 to 10. Input of freshwater, precipitation and freshwaterinputs 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 isimportant to carry out this type of research in this area. It is also interesting to highlightthe importance of the research in a general context. I understand that the introductionnow masks the importance of the study, which I understand is to know the metabolicbehaviour of the microbial plankton community.

Author reply: The main aim of the work goes beyond the study of the metabolic behaviour of the microbial plankton community (please see our comments to referee#1 above). The introduction will be revised and "tighten" attending the comments of the referees and making more explicit the principal aims and objectives of the research we undertook.

Referee # 2 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.

Author reply: We had severe problems with the CTD during some intensive sampling.

Referee # 2 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.

Author reply: In this version of the manuscript we chose to work with the sampling depth that roughly corresponded to the photic layer hoping for a more "neat" interpretation of the data.

Referee # 2 Page 5937, line 4. Post-incubation should be removed from here.

Author reply: we will remove "post incubation"

Referee# 2 Page 5937, line 21. Instead of experiments, it should read determinations.

Author reply: we will replace experiments for "determination"

Referee # 2 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.

Author reply:the correlation between nitrate and GPP will be discussed in the appropriate place

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Referee#2 Page 5942. Line 20. The system is in balance on an annual base due to the conversionto 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 varyingbetween 1 and 1.4. In addition, these annual values of GPP and CR are estimatedfrom the average values given in table 2. Nonetheless, these mean values can containstrong deviations owing to sampling bias. High values of GPP were obtained fromonly 1 sampling (April, August and November), while lower GPP values were derivedfrom more samplings. Then, the question is: To what extent these single samplingsrepresent the real situation?

Author reply: The reviewer is correct in indicating that it is difficult to conclude whether the system (top 20 meters of the water column) is autotrophic, heterotrophic or in trophic balance. Following reviewer # 2 own early reasoning if we change the RQ we could demonstrate net heterotrophy (using an RQ of 0.7 and keeping a PQ of 1 we could get a net community production of –8.3 mol O2 m-2 y-1). In this context,the suggestion of all the referees that we should adopt far more cautionary approach when we interpret the data will be followed.

Referee#2 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.

Author reply: Page 5943, lines 9-10 and figure 7the figure will be improved

Referee # 2 Line 16. Leu-1 should be read L-1

Author reply: Line 16 Leu-1 will be changed for L-1

Referee#2 This section 3.4 on chlorophyll and phytoplankton should be located before the section on GPP and CR.

Authors reply: The section 3.4 on chlorophyll and phytoplankton will be located before the section on GPP and CR

Referee#2 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 below0.4 according to Kritzberg et al.

Authors reply: Page 5944 section 3.5 To avoid the bias produced by the use of literature derived BGE in the new version of the manuscript the result section will only contain Bacterial Production estimates. The potential BCD will be analysed in the discussion section.

Referee#2 Page 5944, section 3.6. Although interesting, this section should be removed from theresults. Only 3 profiles on pCO2 were determined and they seem very few to developa specific section.

Authors reply:the section will be removed from the results as suggested by referee # 1 and referee # 2

Referee # 2 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.

Authors reply: The discussion will suffer a major revision to take into account the potential bias/errors introduced by the use of literature estimates of BGE, CF, PQ and RQ.

Referee # 3

Referee#3 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, anduse the balance between production and bacterial removal of organic carbon to determineif the system is a source or sink of carbon dioxide.

General comments: Overall, the manuscript is poorly written. The use of terms isconfusing e.g. bacterial production versus bacterial secondary production, and abbreviationsare inconsistent. Firstly, I would like to suggest that the authors clarify theiruse of bacterial production and bacterial secondary production and that the authorsreport on the abbreviations for all parameters once in the introduction/methods anduse throughout the manuscript. 'Chlorophyll' should not be capitalized, except at thebeginning 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 playan important role in biological productivity and carbon cycles? What are the characteristicsthat lead to this statement? Is it their area or delivery of nutrient rich waters?

Authors reply: The terminology and abbreviations used will be standardised, we will use the terminology suggested by reviewer # 1. "Chlorophyll" will not be capitalised throughout the text. The goals and motivation behind the work undertaken will be explained in more detail (see our reply to reviewer # 1 and reviewer #2 above).

Referee # 3 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 etcbut samples for pH and total alkalinity were collected every meter?

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Author reply: The suggestions made by referee # 3 will be incorporated; more details will be given to explain the methodology used. The propagated error of GPP and CR will be given as in Williams et al 2004. Regarding silicate: Chilean fjords receive an important input of silicate via river discharge. The silicate is associated to glacial melt. Patagonian rivers however do not carry measurable levels of neither phosphate nor nitrate. Nitrate and Phosphate enter the fjord system via subantarctic water mass intrusions.

The alkalinity and pH data will not be incorporated in the result section following the recommendation of reviewer #1 and reviewer # 2.

Reviewer # 3 Page 5939, lines 11 to 20. The authors use two approaches to convert leucine incorporationto carbon to derive bacterial production, then two estimates of bacterialgrowth 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 tostate why they used these specific equations or conversion factors. Thirdly, if the authorswant to use two different approaches/equations/conversion factors, they need tocompare the output at some stage to determine if they agree, then use one data set inthe 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 describedand clearly written as the rest of the paper, and conclusions based on the metabolismof 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 Table1 and insert the correlation statistics into the methods section before using this data tointerpret carbon balances. The authors present a four way comparison for BSP usingthe combination of conversion factors in Figure 9. However, it would be more usefulto have a quantitative measure of the comparison (i.e. correlation statistics) and theauthors need to rationalize the spread in data in their interpretation (e.g. in January,BSP ranges from _ 0.8 to 1.8 g C m-2 d-1).

Authors reply: the manuscript will be changed to incorporate reviewer # 1 and re-

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viewer # 2 suggestions regarding the use of literature derived BGE and leucine to carbon conversion factors.

Reviewer # 3 Specific comments

Reviewer # 3 Pg 5931 Abstract: This needs to be rewritten. The abstract isunclear 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 becapitalized. The purpose of the study is also not mentioned, i.e. why were thesemeasurements made at this particular site over this specific time period?

Author reply: the abstract will be rewritten following all the reviewer comments

Referee # 3 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 throughoutmanuscript. Line 27 to 29: change 'increments' to 'increase in' or 'enhanced'. Also online 11, page 5933.

Author reply: The grammar will be checked by a English speaking person. Chlorophyll will not be capitalized. Grammatical changes will be carried out whenever is needed.

Referee # 3 Page 5934 Line 20: should be 'in terms of' Page 5935 Line 8: names of rivers do notneed to be in brackets Line 9: change 'set an' to 'leads to an'

Author reply: it will be done as requested

Referee #3 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 _m versus 20-_m Line 12: should read 'Chlorophyll a size fractions', not fractionation.

Author reply: it will be corrected as suggested

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Referee #3 Page 5942 Line 8 – 10: The authors report the highest values as 4.8-698 _g C L-1 d-1 and lowest values 0.5-308 _g 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.

Author reply: it will be corrected in the revised version

Referee #3 Page 5943 Lines 9 to 14: This section is rather descriptive yet data is available to bemore quantitative here (in Fig 7). Line 15: Please check units here – cells Leu-1 103

Author reply: a more quantitative approach will be used for this section in the revised version of the paper. The units will be corrected.

Referee # 3 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.

Author reply: the reviewer is correct we should delete "light field" and rather use "changes in solar radiation".

Referee #3 Page 5948 Lines 27: BSP 'decreased' rather than 'fell' from 1 _ 0.6 to 0.6 _ 0.3 g Cm-2 d-1 and GPP decreased from 1.1 _ 1.12 to 0.1 _ 0.1 g C m-2 d-1. Firstly, theseerrors are large and if the errors are one standard deviation, the differences for bothBSP and GPP are not significant between seasons. If this is correct, the authors needto clearly state this somewhere and be cautious in over interpretation of the data.

Author reply: the numbers will have to be revised. The significance of the mean differences can be tested statistically.

Referee #3 Table 1: Correlate the data in this table and present in the text. Table 2: Are the errorsstandard deviations, standard errors, confidence intervals? Please state.

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Author reply: the table will be clarified

Referee# 3 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

Author reply: the figure will be improved

Referee#3 Figure 5 and 7: Do you have estimates of errors that can be added to the bar charts?

Author reply: propagated errors of GPP and CR estimated will be added

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