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Discussions

# *Interactive comment on* "Phytoplankton distribution and nitrogen dynamics in the Southwest Indian subtropical gyre and Southern Ocean Waters" *by* S. J. Thomalla et al.

**S. J. Thomalla et al.**

sandy.thomalla@gmail.com

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**Response to reviewer comments**

I would like to thank my reviewers for the time they have taken for the thorough review of this paper. I would in particular like to thank reviewer 1 for their detailed and substantial comments and suggested changes have been insightful and useful in improving this manuscript for publication.

In the process of attending to the reviewers comments, I came across an error in the $NO_3$ uptake spreadsheet for station NP6. The equation in one of the uptake calculation columns was out of alignment and instead of referring to the previous cells in the



same row referred instead to the cells one row below. This has been corrected and the revised $NO_3$ uptake data for station NP6 now appears in Table 1 and Figure 8f. Unfortunately however, this correction in the $NO_3$ uptake data now results in an unrealistic f-ratio for the 0.1% light depth of 0.83. Integrated uptake rates for this station were therefore only calculated for the water column up to the 1% light depth.

The reviewer's comments are presented in italics, with the response to each comment following below.

**Response to anonymous reviewer 1 comments**

*Amongst the weaknesses of this manuscript I found it to be rather convoluted or repetitive in places and unnecessarily long, making it hard to identify the major conclusions of this study.*

I have shortened the paper significantly (by >1000 words) and removed pointed out repetitions.

*Also, the aim of the study is not clearly stated in the introduction, and it is thus very difficult to grasp the idea behind it.*

I have added the following aim into the introduction:

"This study aims to identify and characterize different hydrographic regimes wherein the observed phytoplankton distribution and production is interpreted in terms of both the physical and biogeochemical control mechanisms regulating phytoplankton growth. Regions of enhanced biomass and production are similarly investigated in order to better understand the relationship between the physical forcing mechanisms responsible for an improved biological response."

*Also, the way the paper is structured should be mentioned to make it ?ow over the general idea. Some parts of the introduction, results and discussion seem to have as an aim, explaining every single detail or feature found in the data or literature, and there does not seem to be a clear question or set of questions that are being addressed.*

*There are several inconsistencies that generate considerable confusion, such as the depth samples were collected from (to 150m or the 0.1% light level) and how appropriate the comparisons really are. I urge the authors to consider the following carefully if they choose to proceed with a revised version of this manuscript.*

In shortening the paper considerably, I hope to have removed the detailed description of every feature of the data and literature. I have clarified the issue with regards to the different sampling depths by identifying the two different types of CTD sampling in the methods section (see the following response to major comment number 1 for further details).

**Major comments**

*1) There is much confusion over whether samples and/or calculated integrals were collected at 6 ?xed depths to 150 m or 6 variable depths to the 0.1% light level (across a varying euphotic zone). This makes it quite unclear if many of the comparisons are appropriate. For example table 3 shows the 0.1% light depth to be located at 150m at all stations but ?gure 7 clearly shows the euphotic zone varies and is shallower than 150 m at several stations. Section 3.4 states chl-a was integrated to 150m, yet section 3.6 discusses nutrient uptake rates to the 0.1% or 1% light levels. This inconsistency must be addressed.*

I have clarified in the methods section the two types of CTD sampling, i.e. set depth sampling for chlorophyll versus % light depth sampling for production stations. To avoid confusing the readers, I have added the % light depths to Table.1 (formerly Table 3) relevant to the nutrient concentrations, uptake rates and f-ratios. These now appear in addition to the set depths for chlorophyll which were the only depths listed in the original Table. I have clarified in all areas of the text what the integration depths were. e.g:

"Size fractionated chlorophyll concentrations for the six productivity stations are integrated over the euphotic zone (1% for NP1 and 0.1% for NP2-NP6) and represented



as a percentage of total integrated chlorophyll (Figure 7a-f). As chlorophyll concentrations were collected at standard depths as opposed to light depths, the chlorophyll concentration at 0.1% light (1% for NP1) had to be interpolated to allow a euphotic zone integration."

"$\rho$N uptake rates were integrated to the 0.1% light depth for four of the six productivity stations (NP2-NP5). Exceptions occur at stations NP1 where data at the 0.1% light depth is absent and station NP6, where regenerated uptake rates were relatively low at the 0.1% light depth, compared to the remaining water column and new production rates relatively high, resulting in an unrealistic f-ratio for this depth. Uptake rates for these two stations were therefore integrated to the 1% light depth (Table 1)."

*2) The manuscript should be signi?cantly shortened and clarity improved. There is far too much discussion of the literature and data within the results section, much of this can be omitted, or if retained should be moved to the discussion section. The discussion is also rather long and convoluted. I suggest efforts be made to shorten the discussion.*

I have made a concerted effort to shorten the discussion. The manuscript has been shortened by $>$1000 words. I have also removed any points of discussion from the results section.

*3) There is far too much repetition in places (such as the chl-a data which appears in section 3.4 and is then revisited in section 3.6) which could be removed by carefully restructuring the manuscript.*

I have restructured the manuscript to incorporate the two nutrient sections into a single section 3.4 and likewise with the two chlorophyll sections 3.4 and 3.6 into a single chlorophyll section 3.5.

*4) The role of iron is highly relevant but the overly long discussion of this point, without supporting data, tends to smother the actual uptake data presented here and sections* *often read like a literature review. The authors should refocus the manuscript to lessen the bias and emphasize the relevance of iron on the data they are presenting.*

The following Fe discussion has been removed from the manuscript:

"Iron limited phytoplankton production in HNLC environments is now unequivocally established through a number of artificial iron fertilisation experiments (De Baar et al., 2005, Boyd et al., 2007) while two natural iron fertilization experiments in the Southern Ocean (KEOPS: Blain et al., 2001 and CROZEX: Pollard et al., 2007) have further assessed the impact of Fe-fertilisation on carbon export and the carbon to iron (C:Fe) export efficiency. Regions of relative iron availability are characterized by larger average cell sizes, a dominance by diatoms, higher $f$-ratios, faster rates of specific nitrate uptake ($V_{NO3}$ $d^{-1}$), lower POC:chl-a ratios and improved photo-physiological competency as revealed by FRRf values for Fv/Fm. All these parameters provide physiological and taxonomic evidence for the impact of Fe availability, which increases with proximity to sub-Antarctic Islands such as the Crozet and Kerguelen archipelagos (Blain et al., 2001, Lucas et al., 2007, Moore et al., 2007a,b, Pollard et al., 2007, Poulton et al., 2007, Seeyave et al., 2007). Indeed, ocean colour satellite imagery demonstrates perennial blooms around all sub-Antarctic Islands, including the Prince Edward Islands (Pollard et al., 2007)."

"Such experiments include SOIREE (Boyd et al., 2000), EISENEX (Gervais et al., 2002), SOFEX (Caole et al., 2004) and EIFEX (Hoffmann et al., 2006). Naturally iron fertilised regions around the Crozet and Kerguelen Islands show very similar responses, including increased POC export in the Fe-enriched downstream regions (Blain et al., 2001, 2002, 2007, Pollard et al., 2009). Elevated chlorophyll concentrations downstream of these islands are also clearly visible from SeaWiFs ocean colour in early spring (Pollard et al., 2007), as it is around the Prince Edward Islands (Pollard et al., 2007). Although no iron measurements were made during this study, it is not unreasonable to suppose that the downstream regions of the Prince Edward Islands are also Fe-enriched, particularly during winter and in early spring (September, October).



Around Crozet, the impact of Fe-enrichment on phytoplankton N metabolism waned significantly by early to mid summer (November to December) as surface Fe pools were depleted, resulting in a decline in the average $f$-ratio from $\sim$0.45 to $<$0.2 (Lucas et al., 2007). A similar seasonal trend is also to be expected at the nearby and very similar latitude of the Prince Edward Islands. Given that this study was undertaken in April / May, in late summer, it is therefore not surprising that an expected iron mediated response in the $f$-ratio was barely observed."

*5) The sensitivity of the nutrient techniques is not discussed and may be critical in the low nutrient environment of the subtropical Indian Ocean. The method for measuring NH4 may not be suf?cient to resolve in-situ concentrations and may distort the importance of NH4 uptake as a result if ambient NH4 concentrations are overestimated. I suggest a short sentence on analytical sensitivities of the techniques be added.*

The following sentence on analytical sensitivities of the techniques was added:

"The lack of any isotopic dilution corrections for these experiments means that the regenerated uptake measurements are underestimated, particularly for those stations where ambient $NH_4$ concentrations bordered the limit of detection. Such underestimations of $\rho NH_4$ by as much as a factor of two (Glibert et al., 1982c) would further reduce the f-ratio, which raises questions over the sensitivities of the analytical techniques used to measure $NH_4$ and urea concentrations. The commonly used indophenol blue method for $NH_4$ analysis often yields inconsistent results, particularly when ammonium concentrations are low. The importance of $\rho NH_4$may therefore be distorted if ambient $NH_4$ concentrations are overestimated and this needs to be considered when interpreting the results particularly with regards to the low f-ratios."

*6) The lack of isotopic dilution corrections, I suspect, cannot be addressed but there is no discussion of the impact of this omission on the uptake rates or on the interpretation of the data. A section addressing this must be added to the discussion given the overwhelming indication of the importance of regenerated nutrients. If anything this will* *drive f-ratios to even lower values, which then further raises questions over the sensitivities of the analytical techniques used to measure NH4 and urea concentrations.*

The following text does appear in the methods section 2.3 addressing the lack of isotopic dilution.

"Isotopic-dilution of $^{15}$N-$NH_4$ in particular by $NH_4$excretion in vitro will underestimate the computed $NH_4$uptake rates (Harrison and Harris, 1986, Donald et al., 2001, Varela et al., 2005), particularly in oligotrophic oceans (Harrison and Harris, 1986)."

In addition, a short section has been added to the discussion addressing the sensitivities of the analytical techniques used to measure NH4 and urea. Please see response to previous comment number 5 for more details.

**Minor comments**

*1) Page 1349 Line 8 should be redrafted for clarity (Subtropical waters were. . ..), are the low concentrations Carbon or Chl-a? Given that it is the abstract, what does the $>$87% mean, relative to what?*

Sentence has been adjusted to read as follows:

"Subtropical waters were characterised by low chlorophyll concentrations (max = 0.27.3 mg m$^{-3}$) dominated by pico phytoplankton cells ($>$81%) and very low f-ratios ($<$0.1), indicative of productivity based almost entirely on recycled ammonium and urea."

*2) Page 1349 Line 11 - Si(OH)4 not SiO4*

Done

*3) Page 1349 Line 15 - . . .total chlorophyll increased ($<$0.74 mg m-3). . . Increased relative to what value?*

Sentence adjusted to read as follows:

"In subantarctic waters, total chlorophyll concentrations increased (max = 0.74 mg



m$^{-3}$) relative to the subtropical waters and larger cells became more prevalent,"

*4) Page 1349 Line 16 – greater than sign. Should this be less than? Or, why not writing a range of 'low' values?*

Adjusted to read "max = 0.74 mg m$^{-3}$" and changed $>$ sign to "($<$0.14)"

*5) Page 1349 Lines 19 – 22. This sentence needs to be redrafted to make it clearer. Are the percentages indicative of the changes in microplankton cell abundances (which incidentally were not measured directly in this study so technically this sentence should re?ect this) or indicative of changes in new production? Also, the word 'and' appears twice on line 19-20.*

The sentence was restructured to read as follows:

"The percentage of micro phytoplankton cells and rates of new production did however increase at oceanic frontal regions (58.6% and 11.22% respectively), and in the region of the Prince Edward archipelago (61.4% and 14.16% respectively)."

*6) Page 1350 Line 2 – I suggest this reads 'provides a link' rather than 'provides the link' as you do not consider the solubility pump.*

Done

*7) Page 1350 Line 6 – This estimate of the carbon pump seems low. Suggest you check Field et al*

Adjusted the estimate to the following:

11 to 16 GtC (1GtC = 10$^{15}$g of carbon) with the following updated reference (Falkowski et al., 2000).

*8) Page 1350 Paragraph 2 – Redraft as it is rather confusing, particularly the change from discussing the infrequent sampling of the SW Indian Ocean to discussing various techniques for measuring export. ?uxes Many of the studies do not come from the SW*

*Indian Ocean and the relevance is not immediately obvious.*

The following sentence discussing the various measuring techniques has been removed:

"Measurements of export production have been made directly by sediment trap (e.g. Honjo et al., 2000, Salter et al., 2007) and indirectly by thorium isotopes (e.g. Cochran et al., 2000, Morris et al., 2007), by calculating $NO_3$ 'draw down' (Koeve, 2001, Rubin, 2003, Sanders et al., 2005, 2007) and by using $^{15}$N stable isotopes that differentiate between 'new' (export) and 'regenerated' (recycled) production (Dugdale and Goering, 1967, Eppley and Peterson, 1979, Bury et al., 1995, Waldron et al., 1995)."

*9) Page 1350 line 24 to Page 1351 Line 16 - It reads more like a review and could possible be of more use in the discussion section.*

I have tried to place this paragraph into the beginning of the discussion section but find that it does not fit well in the discussion. I prefer it's inclusion in the introduction where I first introduce the method and with this paragraph follow through with highlighting it's limitations for inferring carbon export.

*10) Page 1350 lines 22 – 25 – higher, faster, lower improved. . . These adjectives are relative to, what?*

This sentence falls within the paragraph of too much Fe discussion and has been removed accordingly. See major comment number 4.

*11) Page 1350 – I consider the last paragraph should clearly state the aim/hypothesis of the study.*

I have added the following aim into the introduction:

"This study aims to identify and characterize different hydrographic regimes wherein the observed phytoplankton distribution and production is interpreted in terms of both the physical and biogeochemical control mechanisms regulating phytoplankton growth.



Regions of enhanced biomass and production are similarly investigated in order to better understand the relationship between the physical forcing mechanisms responsible for an improved biological response."

*12) Page 1350 Lines 16-17 – Why is the 'median' important here?*

The median is not important here, I have misrepresented the methods. I have corrected the sentence to read as follows:

"The fronts were identified as the maximum gradient in temperature range from 12-16$^o$C for the AF, 8-12$^o$C for the STF and of 4-8$^o$Cfor the SAF.

*13) Page 1353 Line 20 – Replace was with 'were'.*

Done

*14) Page 1353 Line 26 – The reference to Table 1 should be removed and the stations instead shown in Figure 1.*

Done

*15) Page 1354 Section 2.1 – Were all samples collected at the same ?xed depths to 150 m or at varying depths as determined by the irradiance levels? It is unclear.*

This has been clarified. See major comment number 1.

*16) Page 1354 Line 23 - SI unit for volume is 'L' not 'l' (2 L instead of 2 l).*

Done

*17) Page 1355 Line 1 – If NO3 was measured later in the lab, how the spikes at 10% of ambient concentrations were worked out?*

The following sentence was added to the methods section

"As no on board measurements of $NO_3$ were available, spikes were estimated according to different oceanic regimes using historical data (Schiltzer, 2000)."

With the following reference was added to the reference section:

“Schlitzer, R. 2000. Electronic Atlas of WOCE Hydrographic and Tracer Data Now Available, *Eos Trans. AGU*, 81(5), 45”

*18) Page 1355 Line 4 – Were deep samples also flushed with surface seawater? What impact did the temperature differential have on uptake rates?*

The following sentence has been added to the methods section:

“The incubators were cooled with a constant supply of surface seawater to simulate i*n situ* temperatures. In subantarctic stations, where mixed layers are isothermal to below the euphotic zone, no temperature differential is expected to affect uptake rates. However, in the subtropics, it is possible that subsurface samples are exposed to higher than ambient *in situ* temperatures. Although the effect of this temperature differential on phytoplankton uptake rates was not measured, one can expect it to be small.”

*19) Page 1356 Line 6 – It is not possible to see the eddy the authors refer to nor the eastward movement of the eddy discussed later (Page 1357). I suggest reference to the eddy be removed.*

All references to the eddy have been removed

*20) Page 1357 Paragraph 1 – There is no way you can see the isopycnals referred to on a section of temperature. I suggest this paragraph be removed or a section of density added.*

Any references to isopycnals have been removed.

*21) Page 1357 Lines 4-9 - How was the ?ow observed? Are there any velocity measurements? Different latitudes are mentioned when referring to characteristics in the water column structure, but there are very few Latitude tick marks in the ?gures and thus it is rather hard to follow descriptions.*

Any reference to the eastward flow has been removed.



*22) Page 1357 Line 10 to 21 – There is a lot discussion here of features that cannot be seen in the ?gures. The authors should consider a TS plot, particularly as reference is made to water masses on Page 1358.*

The discussion and description of hydrographic features and structures has been simplified and reduced. The reference to the TS relationship of the mixed water mass has been removed.

*23) Page 1357 Line 13 – seasonal or permanent thermocline?*

This paragraph was removed when simplifying and shortening the hydrogrphic description.

*24) Page 1357 Lines 15-16 – Where’s the evidence for the Ekman layer driven from the subantarctic zone, how was this estimated?*

Reference to the Ekman layer driven north was removed when simplifying and shortening the hydrogrphic description.

*25) Page 1357 Lines 19-21 – Where can we see the TS relation showing the mixing?*

The reference to the TS relationship of the mixed water mass has been removed. The following sentence was instead added to highlight the transition region between two water masses.

“On this transect the STF could be placed at the northern (42.5$^o$S) or southern (43.25$^o$S) edge of a transitional region where temperatures at the base of the mixed layer ($\sim$12$^o$C at $\sim$80 m) were significantly less than those of Agulhas (15$^o$C) or Subtropical (19-20$^o$C) Water but higher than Subantarctic Waters ($<$9$^o$C).”

Given reviewer 2’s comments of drastically reducing the number of graphs and tables it was decided not to add an additional figure showing the TS relationship of the mixed water mass in the transition region.

*26) Page 1358 Paragraph 1 – Too much discussion. Move this to the discussion sec-*

*tion.*

The following sentence was removed from this section and added into the discussion.

“Whether these were mesoscale eddies, filaments, or larger frontal structures, is not possible to determine from a single section. Such structures have been observed previously (Read and Pollard, 1993) and attributed to eddies, which often give rise to double step frontal features (e.g., Pollard and Regier, 1992).”

*27) Page 1658 Lines 3-5 – What depth range? What is warm and what is cold? It looks colder southwards.*

I added the depth of 200 m where the structure is most obvious. I added the word mesoscale to describe the size of the structure the reader is looking for. I think from Figure 3 a and b the mesoscale structure at 200 m at the latitudinal positions listed in the text, it is sufficiently clear to the reader the structure that I am referring to.

*28) Page 1658 Lines 11-19 – This section is confusing, de?nitely a TS diagram would be most useful.*

This section was removed in accordance with simplifying and shortening the hydrogr-phic desciption.

*29) Page 1358 Section 3.4 – This section needs to be rewritten as it is confused and unclear. For example, there is further discussion of isopycnal surfaces which are not shown, repeated reference to the westward ?ow which is inferred and not shown and it is not clear if the reference to a series of higher and lower concentrations refers to surface values or to vertical concentrations. Why were chl-a concentrations integrated to 150 m when the euphotic zone was often shallower? Do you mean the euphotic zone?*

This section has been rewritten. Reference to isopycnal surfaces has been removed, as has the reference to the westward flow. The integrated chlorophyll is for the euphotic zone and this too has been made evident in the text.



*30) Page 1359 Line 3 – Here potential temperature is mentioned. Is the contour plot showing temperature or potential temperature? In either case, this should be clearly stated and used consistently.*

The contour plot is showing temperature. This has been clarified throughout the text and any reference to potential temperature removed.

*31) Page 1359 Line 7 – Reference to Figure 4b should be moved to the end of the sentence.*

Done

*32) Page 1359 Line 17-19 – Where can the distribution to greater than 100 m can be seen?*

This has been corrected and the sentence now reads as follows:

“Pico phytoplankton are ubiquitous and the dominant size fraction on the transect apart from at the SAF and the Prince Edward Island Plateau, where micro phytoplankton dominate.”

*33) Page 1359 Line 23 – It is clear from Table 3 that nitrate concentrations in the subtropical waters were “ 1 mmol m-3 and very possibly in the nanomolar range. A more appropriate estimate of the subtropical concentration should be presented.*

The following information was added in parenthesis as a more appropriate estimate of low $NO_3$ concentrations.

“Nitrate values in subtropical surface waters are low (0.01 – 0.49 mmol $m^{-3}$in the surface 50 m)”

*34) Why is the SF chl data not discussed within section 3.4? These two sections should be combined to improve the clarity and structure of the manuscript.*

Both sections have been combined into section 3.5.

*35) Page 1359-1360 – Why is Si not described in more detail? The contour plot shows rather dubious data. Can you describe why it looks so different than nitrate?*

I cannot explain why the Si section looks so anomalous when compared to the $NO_3$ sections. I feel that it is very unlikely for the Si and $NO_3$ plots to look so different. The divergence of the Si section from the $NO_3$ section resulted from one CTD station with comparatively high SiO4 concentrations when compared to the surrounding waters. Although I cannot explain this increase in SiO4 concentrations I find it unlikely without a similar increase in $NO_3$ concentrations and as such have removed the single station of relatively high SiO4 concentrations from the SiO4 section.

*36) Page 1360 Line 6 – What exactly is meant by 'nutrient' and 'N' uptake? Is it N-nutrients and total N uptake?*

The sentence has been clarified and now reads as follows:

"Nutrient ($NO_3^-$, $NH_4^+$, urea) uptake data ($\rho$N ) are presented for the nominal euphotic zone to the 0.1% light depth (1% for Station NP1)"

*37) Page 1360 Line 14 – What do you refer to as the 'total nitrogen pool'?*

This has been clarified with parenthesis to read as follows:

"Ambient nitrate, ammonium and urea concentrations from the six productivity stations are integrated over the euphotic zone (1% for NP1 and 0.1% for NP2-NP6) and represented as a percentage of the total nitrogen pool ($NO_3$ + $NH_4$ + urea) (Figure 6a-f)."

*38) Page 1360 Lines 21-23 – The distribution of urea doesn't seem to show a gradual decrease southwards, to me it looks more like NH4, that is, it does not show any clear spatial pattern.*

I'm afraid I have to disagree here. The sentence in question reads as follows:

"ambient urea decreased with southerly latitude, from a maximum in the north (NP1, 130.2 mmol $m^{-2}$; 68% of total N), to a minimum in the south (NP6, 11.5 mmol $m^{-2}$; just



1% of total N). Ambient $NH_4^+$ concentrations were variable and displayed no obvious spatial trends."

The latitudinal distribution from North to south for the six productivity stations for urea are as follows: 130, 44, 81, 91, 11. Although not consistent, the urea concentrations to me tend to decrease with increasing latitude with highest concentrations in the north and lowest in the south. The latitudinal distribution of NH4 on the other hand is as follows: 2.2, 22, 39, 15, 11, 59 and to me shows no discernible spatial trend with latitude.

*39)Section 3.6.2 – What is the point of this paragraph?*

Agreed, this paragraph has been removed.

*40) Section 3.6.3 – Is it total integrated chlorophyll?*

The distinction between total chlorophyll as opposed to total integrated chlorophyll have been made more clear.

*41) Section 3.6.4 – The uptake data is presented both in Figure 9 and Table 3. I suggest omitting Figure 9 as it is barely discussed after its introduction on Page 1362.*

Done

*42) Should not the symbol used to represent integrated uptake rates (rho integral N), be Integral rho N?*

Done

*43) Page 1362 Line 17-19 – With the integrated values that would seem to be the case, however chl-a and uptake are higher close to the surface, while nitrate concentrations are high at depth.*

The profile figures for uptake rates, nutrient and chlorophyll concentrations have been removed from the papers. Only the integrated results are being discussed in this section.

*44) Page 1363 Lines 1-2 – I disagree with this statement, the ratio 'kind of' increases with depth. Besides, in the ?gure, as it is, f-ratio symbols do not match nitrate concentration symbols in terms of positions at depth.*

The statement referred to reads as follows:

0.0.1 "At subtropical stations NP1-NP3, the f-ratio generally increased with depth, tracking ambient $NO_3$ concentrations (Table 1)."

The profile figures of f-ratios have been removed from the paper. I refer instead to Table 1 where the f-ratio data is presented in conjunction with the NO3 data at the same depths. Although the increase in f-ratio with depth is not consistent, the general trend in the subtropics is to have higher f-ratios at the deepest depths compared to the shallowest depths. For stations NP1 and NP2, minimum f-ratios are at the surface and maximum f-ratios at depth. Station NP3 is not as consistent but still shows a general increase in depth from 0.07 at the surface to 0.13 at the 1% light depth. For all three subtropical stations, minimum NO3 concentrations are at the surface and maximum NO3 concentrations at the 0.1% light depth. I thus feel that this statement can remain in the text.

*45) Page 1363 Line 4 – According to table 3 there are no uptake measurements at 15 m.*

The depths of the %light levels for the six productivity stations have now been added to Table 1, together with the set depths for chlorophyll that were originally in the Table.

*46) Page 1363 Lines 5-8 – I'm not sure about this statement. NP4 looks like NP1 and NP2. It is dif?cult to appreciate with such small graphs, you could always restrict the scale to 0.5 or to the maximum value.*



The statement in question reads as follows:

"At the subantarctic stations NP4 and NP5, the situation was reversed and $f$-ratios generally decreased with depth, indicating a shift from ?$\rho NO_3$ in surface waters to ?$\rho$N based primarily on reduced N at depth."

The profile graphs have been removed. The reader is instead asked to refer to Table 1 for depth relations in f-ratios. From this Table it is clear that the f-rations at station NP4 are higher in the surface three light depths (0.08, 0.07,0.1) compared to the deeper three light depths (0.03, 0.04, 0.06). The same is true for stations NP5. I thus feel that the statement may remain in the text.

*47) Page 1363 Line 10 – In your plots it looks more like at 120 m depth.*

The depths of the % light levels have now been added to Table 1.

*48) Page 1363 Lines 11-14 – How come you integrated the f-ratio? Did you work the ratio out using integrated uptake rates? Or did you actually integrate the ratio? If the second case were true, well, that would be an odd thing to do and you would end up with units actually.*

I calculated the integrated f-ratios from the integrated uptake rates, and not by integrating the f-ratios themselves. I added the following in parenthesis to clarify this:

"Integrated $f-$ratios (calculated from $\int \rho$N),"

*49) Page 1363 Line 23 – "generally exceedingly low" should be replaced.*

The word "generally" has been removed from the text

*50) Section 4.1 – Some of this is repeated from the results section, which is not necessary. There seems to be a slight confusion over the seasonal and permanent thermocline. The suggestion that Fe replete conditions exist north of the STF and in the subtropical waters requires clari?cation – what are the sources/concentrations/evidence for this statement?*

The following paragraph on the description of the chlorophyll distribution has been removed from the text to prevent repetition:

"Chlorophyll biomass in subtropical waters was low, with maximum concentrations <0.3 mg $m^{-3}$. Throughout the region, pico-phytoplankton dominated chlorophyll size distribution (>81%), followed by nano-phytoplankton (~16%) and an exceptionally low biomass of micro-phytoplankton (~3%) (Table 2)."

"The second statement regarding Fe replete conditions north of the STF reads as follows:

For stations NP1 to NP3, north of the STF, the mean integrated $\rho$N* value was > 4 times that of the three subantarctic stations (NP4-NP6). These results could suggest that combined $\rho$N and photosynthesis in the subantarctic is either Fe-limited or co-limited by Fe and light, where the latter encourages chl-a packaging to compensate for lowered light intensities, therefore resulting in lowered $\rho$N* values. Conversely, the region north of the STF appears to be freed from these influences."

I feel that I have in this sentence I do not suppose that conditions north of the STF are Fe replete. Instead I infer that low $\rho$N* values south of the STF could suggest Fe and light co-limitation whereas the high $\rho$N* north of the STF suggest that these waters are not Fe and light co limited. I do not imply a source of Fe or presume to know the concentrations of Fe but simply use the high $\rho$N* values to suggest this as a possibility.

I have added the following caveat to the above sentence to hopefully satisfy the reviewers comment:

"Having no Fe measurements however means that we are unable to substantiate this argument."

*51) Page 1364 Line 28 – The statement regarding elevated ambient nitrate at depth due to diffusive ?ux is slightly misleading, could it not simply be due to sampling within the thermocline? How do you identify the diffusive ?ux? The increase in the f-ratio with*



*depth might be due to an increase in NO3 (this can be con?rmed from the data) but could it not also be due to a relative reduction in NH4 or urea?*

I deleted "diffusive flux" from the text and I added the following sentence as an alternate suggestion to the increasing f-ratios with depth:

"An alternative explanation is that increasing f-ratios with depth were consistent with a relative decrease in ambient $NH_4$and urea (Table 1)."

*52) Page 1366 Line 9 – Again, if potential temperature is used, this should be clearly indicated when ?rst mentioned and also in the ?gures and ?gure captions.*

Done

*53) Page 1366 Line 17 – f-ratios is ?gure 12.*

Done

*54) Page 1366 Lines 24-26 – Where is the 72 m average depth of the euphotic layer coming from? By looking at table 3 it would seem 150 m. By other hand, by looking at ?gure 7, the depth of the 0.1% light level is so variable and within a large range, that averaging it would not be representative.*

The confusion with the euphotic depths integrated depths has been dealt with, see major comment number 1.

*55) Page 1367 Line 4 – Nitrate uptake is an 'energetically'. . .. What?*

Added the word "expensive"

*56) Section 4.2.2 – The RPI is a discredited and unreliable indicator of nutrient preference due to the impact variations in ambient nutrient concentration have on the calculated RPI value. I suggest reference to the RPI be removed from the manuscript.*

Done

*57) Page 1368 Line 3 – Nitrogen or Nitrate uptake?*

Changed to "$\rho$N" rates

*58) Page 1368 Line 26 – 'is also to be expected' sounds odd, why not, 'would also be expected?' By the way, why?*

Changed to "would be expected".

A similar seasonal trend at Prince Edward and Marion Island would be expected as both are sub antarctic Islands with an expected natural Fe fertilization and in addition, the similar latitude would imply a similar seasonal trend in PAR. I have not added the "why' to the text but am happy to do so if the reviewers feel it is necessary.

*59) Page 1369 Line 10 – Add a suitable reference to support the claim regarding biogenic silicate deposition.*

The information referring to the biogenic silicate deposition has been removed. Not because there isn't a reference to substantiate it but because it was not necessary information. The more relevant information is that "the Southern Ocean exports (to 1000 m) the highest proportion ($\sim$3%) of its total production (Honjo et al., 2000), making it disproportionately important as a biologically mediated sink for atmospheric $CO_2$."

*60) Page 1370 Lines 2-6 – Yes, but you also said the nitrate uptake needs light and nitrate has been depleted at the surface, so integrated nitrate does not really convey this information.*

It is not clear to me what the reviewer requires from this comment?

*61) Page 1370 Line 13 – The reference to the diffusive nitrate ?ux is misleading. Could the increase in the f-ratio in the AF region not simply be due to increased nitrate concentrations rather than due to an active supply mechanism? Otherwise, where is the observed diffusive nitrate ?ux? Numbers?*

I removed the word "flux" and replaced it with "concentrations" so that the sentence now reads as follows:



"The increased $f$-ratio within the AF implies a slight increase in $\rho NO_3$ that could result from the observed increase in $NO_3$ concentrations and / or from a more favourable light environment in the frontal region (Grundlingh, 1979; Lutjeharms et al., 1981, 1985) ."

*62) Page 1370 Line 26 – If you integrate chl normalized uptake you do not end up with the units you show.*

Changed all units in the text and graphs to read as follows: mmol N (mg chl)$^{-1}$d$^{-1}$

*63) Page 1371 Line 15 – The two mechanisms most likely. . .*

This paragraph on the two mechanisms most likely . . . . has been removed in order to shorten the paper.

*64) Page 1371 Line 19 – Does it actually explain the observed increase in chl, or it would explain?*

This paragraph has been removed in order to shorten the paper.

*65) Page 1372 Line 16 – Is it 42.5 or 43.5 deg South, or do you mean, now at 43.5 deg South?*

I mean 43.5 deg South, I have adjusted this sentence for clarification and it now reads as follows:

"On the Northbound transect 10 days previously; the STF was located at 43.5$^o$S (Figure 2a), the same position as station NP4. However, with the migration of the STF by one degree north (42.5$^o$S) on the Southbound transect (Figure 3a), the favourable conditions associated with the front, that are responsible for initialising and essential in maintaining enhanced productivity are no longer present at station NP4 (43.5$^o$S)."

*66) Page 1372 Line 18 – Integrated chl?*

I added the word "integrated". The sentence now reads:

"A peak in integrated chlorophyll concentration..."

*67) Page 1372 Line 25 – ..in the euphotic surface layer of . . ..*

Done

*68) Page 1373 Line 18 – What is meant by rNH4? Or, is it that the 'r' was not converted to the Greek symbol?*

This discussion has been removed from the text in order to shortened the paper.

*69) Conclusions Page 1374 Line 15 – Do you mean South Atlantic or SW Indian? Line 22-25 – This sentence appears out of nowhere, and discusses issues which are not really present in the manuscript (POC export, respiratory CO2 losses, biological CO2 drawdown). I suggest this sentence be removed.*

Corrected to SW Indian.

I feel that this sentence may remain in the conclusion. I do not feel that it appears out of nowhere. I state in the introduction that "the $f-$ratio can be an instructive diagnostic tool for evaluating the potential for carbon export." I similarly mention at the beginning of the discussion that "...the potentially significant impact on POC export and $CO_2$ draw-down". Also, the end of Section 4.1 describing the region north of the STF ends with the following sentence:

"One consequence of this ecosystem structure is that N is conserved, but respiratory $CO_2$losses are high, and only a small fraction of the fixed POC is exported into deep water (Tremblay et al. 2000; Salter at al. 2007), not least because of the absence of any silicate ballasting effect (Thomalla et al., 2008, Sanders et al., 2010)."

I therefore feel that the following sentence may remain in the conclusion:

"While respiratory $CO_2$ losses are probably high, POC export and biological $CO_2$ draw down is expected to be minimal."

I did however change the text to read "probably" and "expected to be" rather than "is" and "are"



*70) Page 1375 Line 1 – How do you know it was maintained by turbulent mixing and large scale upwelling? Or do you mean, was 'likely' maintained?*

Inserted the word 'likely'.

*71) The reference list requires attention as several of the references were presented out of order, notably the 2 Falkowski references appearing between Hoffmann and Honjo.*

Done

*72) Page 1376 Line 32 – Jickells.*

Done

*73) Page 1380 Line 9 – Iron deficiency. . .*

Done

*74) Page 1381 Line 10 – Is it not The fourth cruise?*

Done

*75) Page 1382 Line 12 – Equatorial Pacifi*c.

Done

**Tables**

*1) Table 1 serves no purpose and the relevant information it contains (station positions) can easily be incorporated into a revised version of Table 3, or omitted altogether (see comment regarding Figure 1)*

Done

*2) Table 2 is also unnecessary as much of the information (max chl-a concentrations) appears in the text or figures already. The contribution the various size fractions make to the total chl-a is important but is it appropriate to show results to 150 m as the* *euphotic zone was generally shallower than this at most of the stations sampled? It is also unclear if the SF values represent the percentages at the same position as the max chl-a concentration or average contributions in the two regions.*

This table has been deleted

*3) Table 3 contains the real results of this study, but not only repeats data that is shown in Figures 7 and 9 it is also misleading. Were samples collected at fixed depths as seems to be the case from the depth column or were samples collected from light depths (which vary with latitude) as shown in Figure 9. Table 3 gives the misleading impression that the 0.1% light depth was at 150 m at all 6 stations. Also, a couple of values seem to be missing.*

The depths of the % light depths was added to the table to avoid confusion. The chlorophyll samples were collected from set depths as shown in the second depth column.

*The presentation of NH4 and urea uptake rates with up to 9 decimal places is overkill and unnecessary.*

This was most certainly not the intention and has been corrected.

**Figures**

*In general the figures were of sufficient clarity but the number of figures could easily be reduced by either combining some figures or by omitting them altogether.*

The number of figures have been reduced from 16 to 10.

*1) I suggest a new version of Figure 1 be created which includes the Prince Edward islands, labels on the major landmasses, better bathymetric contours and the approximate positions of the fronts and currents discussed in the text. It should also include markers to identify the position of the stations NP1 – NP6, thus removing the need for Table 1.*



Done

*2) Figure 2. Is it temperature or potential temperature? It is very difficult to appreciate the variability of the size-fractionated chlorophyll. You could always include a break point, say between 0.3 and 0.8 and slightly squeeze the three higher values toward the upper part of the plot.*

All images plotted are of temperature and this has been clarified in the text. I feel that it is not necessary to include a break point in the y axis, but feel that one is able to identify the dominant size fractions in the different regions from this figure.

*3) Figure 3. What is the white line at about 24 degrees south? Also, make sure you have the same font size of labels in all contour plots, and less contour labels would be better since it is very distracting to see a figure with so many numbers inside.*

The white space represents a data gap in the CTD transect. I have added this information to the legend of the figure. If I increase the interpolation scheme I can remove the white space however remove all the finer scale details within the section at the same time.

*4) In the legend of Figure 4 it states that the positions of station NP1-NP6 are indicated. I could not see them in the figure. Or, by this do you mean AF, STF and SAF.*

They are not in the figure, I have removed this information from the figure legend.

*5) Figures 2c, 2d and 4b and 4c could be combined to highlight the differences between the two transects more easily.*

I tried this and combined the two figures, but instead of making it easier to highlight the differences in the two transects it made it more difficult. The figure was very messy with much of the data from one cruise overlying the other. The detail of any changes within each transect were overshadowed by the complexity of all the different plots on each graph. I would thus prefer to leave it as is.

6*) Figure 5b appears to show a major upwelling of silicate rich waters at 35S but there is no indication of a similar injection of nitrate at the same position. Why is this?*

See minor point 35)

I cannot explain why the Si section looks so anomalous when compared to the NO3 sections. I feel that it is very unlikely for the Si and NO3 plots to look so different. The divergence of the Si section from the NO3 section resulted from one CTD station with comparatively high SiO4 concentrations when compared to the surrounding waters. Although I cannot explain this increase in SiO4 concentrations I find it unlikely without a similar increase in NO3 concentrations and as such have removed the single station of relatively high SiO4 concentrations from the SiO4 section.

*7) Figure 6. What are the numbers at the top of the bars? I know it is sort of obvious, but these should also be indicated in the caption of the figure. The same applies to Figures 8, 10 and 12.*

The following text has been added to

Figure 6) Numbers at the top of each bar represent the integrated nutrient concentration for each nutrient, with the total N concentration ($NO_3$+ $NH_4$ + urea) at the bottom of each figure.

Figure 7) Numbers at the top of each bar represent the percent contribution of each size fraction, with the total integrated chlorophyll concentration (micro + nano + pico) at the bottom of each figure.

Figure 8) Numbers at the top of each bar represent the percent contribution of each nutrient, with total N uptake ($NO_3$ + $NH_4$ + urea) at the bottom of each figure.

*8) Figure 7 clearly shows that the depth of the euphotic zone varied between stations. This raises the question of why integrals to 150 m are frequently presented. What is the justification for integrating to depths that may not be considered biologically relevant? In the caption I would suggest deleting 'Together with' and replacing this with 'Dotted*



*lines show. . .. And dashed lines show. . .' Why figure 7f does not show a SML? Is it deeper than the depth range cover by the plot?*

This figure has been deleted in an attempt to shorten the number of figures presented

*9) Is Figure 9 necessary given that the data is also presented in table 3? I suggest the data either be tabulated or presented as a Figure, not both. Units used in the caption of this figure are incorrect.*

This figure has been removed

1*0) The contents of figure 10 could be tabulated without loss of detail particularly as not all values are presented in the text*

I believe that this figure could not be adequately represented in a table. I feel that it's presentation in a similar manner to that of figures 6 and 7 make it valuable for comparisons as discussed in the text.

*11) The nutrient concentration data shown in Figure 11 is also presented in table 3, this is not necessary. The units given in the legend (mg-at m-3) need to be updated. The profiles of f-ratios and some N-nutrient profiles look to be on different depth scales. For example, in Figure 11c it looks like you have urea and ammonium measurements to 100 m, yet somehow you have f-ratios to 130m. Why are the profiles of f-ratio not present in Figure 9 with the more relevant uptake rate data?*

This figure has been deleted together with the profiles of f-ratios have. The f-ratio data with depth has instead been added to table 1.

1*2) Are the f-ratios based on integrated uptake profiles or derived in some other manner? It is not clear.*

The integrated f-ratios are based on integrated uptake data. This has been clarified in the text and the figure legend adjusted to read as follows:

Figure 9. $f$-ratios calculated from integrated euphotic zone (1% for NP1 and NP6, 0.1%

for NP2-NP5) uptake rates for the six productivity stations of the Southbound Transect.

*13) Figure 14 should be omitted as the RPI has been shown to be severely biased by nutrient concentrations and the results misleading.*

Done

**Response to anonymous reviewer 2 comments**

**General comments:**

*The topic of the ms is potentially an interesting one: phytoplankton distribution and physiology in otherwise largely ignored oceanographic regions. Unfortunately, the way the ms is organized now, it is almost inaccessible for the reader. The ms is very "wordy" with often repetitions, it is unclear what the specific scientific goals were, what methods were used, where samples were collected, and how the results should be interpreted. The authors seem to follow many sidelines, creating confusion for the reader. Finally, it seems that simply all the data is shown (16 figures, with 61 (!!) graphs), apparently without any attempt by the authors to "digest" the data. Publication is only warranted in case all these issues are addressed. The ms should be made shorter, focused on the research questions and the data, with a proper interpretation of the main results.*

The length of the manuscript has been reduced considerably ($>$1000 words). All efforts have been made to remove any repetitions in the text. The main aims of the paper have been added to the end of the introduction in order to clarify the specific scientific goals of the paper. The methods section has been made clearer, in particular with regards to the confusion in different sampling depths. The amount of data presented has been reduced significantly, from the original 16 figures and three tables to 10 figures and 1 table. Please see the response to reviewer 1 comments for more detail on all of the above.

*Introduction: should be limited to being relevant for the research question(s), for example deliminating the paragraph on effects of iron. No measurements on iron were done*



*in this study, and the role of iron could briefly be mentioned in the discussion.*

Any reference to Fe in the Introduction has been removed. The lengthy discussion on Fe in the discussion section has been significantly reduced (see reviewer 1 major response number 4).

*Sampling and analytical methods: nutrient measurements play a critical role in the ms. Yet, details on accuracy and precision, use of certified reference materials is lacking. I have great doubts on measuring ammonium concentrations in preserved samples (in other words: I believe this is impossible). How could 10% of the ambient N be added as 15N when the concentrations of total N were measured in the home laboratory?*

I did not measure ammonium concentrations on preserved samples. This is stated clearly in the text in that ammonia and urea analysis were carried out on board. Only NO3 and silicate samples were stored frozen for future analysis at the home laboratory. 10% spikes for NO3 additions were estimated according to different oceanic regimes using historical data. The following text and reference has been added to the methods section for clarification:

"As no onboard measurements of $NO_3$ were available, spikes were estimated according (Schiltzer, 2000)."

"Schlitzer, R. 2000. Electronic Atlas of WOCE Hydrographic and Tracer Data Now Available, *Eos Trans. AGU*, 81(5), 45"

*Results: even the most interested reader is lost in the 61 graphs, the lengthy text, the confusing indications of the sampling depths, etc., etc. As often, the authors measure Chl a and subsequently this is interpreted as biomass. How do the authors distinguish between different taxonomical groups of the phytoplankton? As far as I can judge, size fractionated samples for Chl a analyses were taken. This allows for distinction between large and small phytoplankton, but how were diatoms recognized ?? The data should be reduced. What were the main findings ? For example focus on the two distinct* *oceanographic regions as end members. Sections discussing the results should be removed here and transferred to the discussion section.*

The number of graphs have been reduced from 16 figures and 3 tables to 10 graphs and 1 table. The confusion of the different sampling depths has been clarified. In the methods section I have made clear the two types of CTD sampling, i.e. set depth sampling for chla vs % light depth sampling for production stations. I have also added the % light depths to Table.1 (formerly Table 3). I have clarified in all areas of the text what the integration depths were. I have deleted any reference to different taxonomical groups and there is no longer any inference of diatoms being a representative of the micro-phytoplankton size fraction. Any sections discussing the results have been removed and appear instead in the discussion section.

*Discussion. Two different oceanographic regions (subtropical versus subantarctic with temperature and nutrient concentrations different) can be discerned, with the phytoplankton distribution rather uniform, nitrogen uptake low everywhere (regenerated production dominant), but the authors speculate and hypothesize for page after page, until the reader gets the impression that huge differences are present. Chl a is what it is: Chl a and NOT a reliable indicator of phytoplankton biomass, see for example (Behrenfeld et al. 2005; Kruskopf and Flynn 2006). Not a single Fe measurement was made by the authors, still one paragraph is used to discuss the role of Fe discussion. Similarly, Si uptake not measured during the expedition, but Si limitation discussed. The authors should focus on the experimental results, and leave out all speculations.*

I hope I have satisfied my reviewer by significantly reducing the size of the paper and the discussion section in particular. I have removed the lengthy discussion on Fe and Si limitation. I hope to have removed all speculative comments that do not at least have a reference to back them up as likely explanations. I have added the following paragraph to the beginning of the discussion to clarify our intentions to the reader:

"In the following discussion, we investigate phytoplankton distribution and primary production in the two distinct provinces where despite regional differences in physical forcing mechanisms, the biological responses were similar. In addition we examine the potential mechanisms responsible for regions of enhanced chlorophyll biomass, changes in community structure and nitrogen dynamics that have a potentially significant impact on POC export and $CO_2$ draw-down."



**Response to anonymous reviewer 3 comments**

**Major Comments**

*1. The results section is somewhat verbose and should be either shortened or merged fully into 'Results & Discussion'.*

The results and discussion section has not been merged but has been shortened significantly. Any discussion appearing in the results section has been removed.

*2. Sections 3.3, 4.2 and 4.3 include discussion of biogeochemical parameters in respect of stratification but referring only temperature profiles. These arguments would be greatly strengthened by using buoyancy frequency, preferably shown as separate temperature and salinity contributions to the buoyancy frequency (see e.g. Gill, Physical Oceanography), or density sections. I recommend inclusion of Nˆ2_T and Nˆ2_S sections in an additional figure or as an extra 2 panels in figure 3. The sections mentioned may then require revision.*

I don't think it is necessary to compute buoyancy fluxes in order to state that there was stratification. Density is dominated by the temperature signal at temps $>$ 4 deg C and we simply wanted to establish a degree of stratification. From the closely spaced isotherms in subtropical waters at 50-100m on the temperature section it is sufficient to state that the water column is well stratified, therefore separating the euphotic zone from the nutricline.

*When discussing chlorophyll concentration in the various water masses, it should be stressed that the water mass classification adopted here refers to conditions at 200 m* *depth. Because of the marked longitudinal depth gradients, conditions at 200 m are not the same as conditions at the surface. This should be considered, particularly in section 4.3.1 - Subtropical Front lines 10 – 15.*

Although the surface and subsurface (200 m) expressions of fronts such as the PF are often very different in latitudinal space, this is not generally the case for the STF. This could be seen in the original figure 3b (this figure is no longer included in the manuscript as it was not referred to in detail and reviewer 2 recommended I drastically reduce the number of plots I present) of surface temperature and surface chlorophyll along the southbound transect. This figure shows the sharpest drop in surface temperature coinciding with the position of the STF which was determined from a subsurface (200 m) temperature expression. I therefore feel that the discussion of the peak in chlorophyll concentration occuring 1 degree south of the STF holds true.

*4. Clearly the fieldwork undertaken here is insufficient on its own to account for the nutrient and growth dynamics across the sample region. Some suggestion as to what is still lacking in terms of parameters and temporal coverage, or modelling, that would enable these processes to be more fully explained, would be useful.*

The following sentence has been added to beginning of the discussion section:

"Although the fieldwork undertaken during this study alone is insufficient to represent nutrient and growth dynamics across the sample region. Planned future studies will take into account the observational spatial and temporal scales of sampling that are required in order to provide key input to regional models."

**Minor Comments**

*- pg 1354 add a reference for 1% light level (e.g. Kirk, Light & Photosynthesis in Aquatic Ecosystems)*

The following reference was added:

"Kirk, J.T.O., 1994. Light and photosynthesis in aquatic ecosystems (2nd Edition ).



Cambridge University Press,Cambridge."

*- pg 1354 line 6: was microscopy used to check that no large diatoms were retained on the zooplankton filter?*

No we did not do this.

*- pg 1367 line 12: please add equation for RPI.*

Any RPI results and discussion have been removed from the paper

*- pg 1372 last paragraph: If the STF northward migration is accounted for by the physical motion of the eddy, wouldn't the phytoplankton population be advected with the front? In that case, the senescent bloom would still be found at the STF location on the southward transect.*

As we cannot prove that there was an eddy at this location, any reference to the eddy in both the results and discussion section has been removed. The proposal however, that the senescent phytoplankton bloom was not advected with the front remains.

*- pg 1374 lines 24-25: Since no CO2 budget measurements were made, state 'probably' instead of 'are' and 'is'.*

I have adjusted the sentence to read as follows:

"While respiratory $CO_2$ losses are probably high, POC export and biological $CO_2$ draw down is expected to be minimal."

*- figure 11: scales for the f-ratio subplots are unclear: are the log or linear? I can't tell whether the third x-axis tick label is 1.0 or 10.*

This figure has been removed from the paper.

Interactive comment on Ocean Sci. Discuss., 7, 1347, 2010.