Anonymous Referee#1

We warmly thank the anonymous referee for his comments. All comments have been addressed here below and in the manuscript when needed. Authors responses are in italics

If the dataset alone merits publication, the manuscript however needs major revisions before it can be published. **First, it absolutely needs to be revised by a native English speaker** (which I am not), as many phrases sound really ackward and some “franglish” is sometimes used. A lot of spelling mistakes also need to be corrected. **But my major critic lays with the interpretation that is given of results** : it is at moments quite weak, and often too speculative. Inadequate references are often used for well known features of the Southern Ocean. Sentences like “nutrients show nutrient-type distribution”, and “algae need light and nutrient for growth” are really unecessary and feel a lot like filling text. Description of results and their intepretation is often vague and besides the point (see examples below), and need thorough rewriting before the manuscript can be published.

We have put additional effort to avoid any “franglish” and spelling mistakes. A native English speaker also revised the present manuscript carefully. We removed the unnecessary sentences. Furthermore we tried to further clarify the interpretation and re-wrote several sections when needed.

Inappropriate/vague result description (some examples):

1) Page 5015 line 25 : “The phytoplankton production of organic material is supported by the consumption of the macronutrients such as nitrate, phosphate and silicate (Koeve and Ducklow, 2001) and additional trace elements, such as iron (Martin et al., 1990), as well as light.” This sentence is quite trivial and the first reference inappropriate. I suggest deleting it.

This has been deleted in the revised version of the manuscript

2) Page 5023 : “The particulate organic carbon (POC) and the particulate organic nitrogen (PON) ranged from undetectable to 15 μM, and undetectable to 2 μM, respectively, in the mixed layers

along the section (Fig. 5). The distribution of POC and PON displayed higher levels in surface waters than in deep waters (Fig. 5), where almost undetectable levels were observed below 100m depth. The highest concentrations of POC and PON along the section were recorded in the upper 50m along the confluence zone of the subtropical and subantarctic domain with respective concentrations of 15 μM and 2 μM (Fig. 5).” Way too much space to simply say that POC ranged between LD and 15 μM and PON between LD and 2 μM and decreased from surface to bottom (obviously…). This is inefficient and carries no informative value.

We have skimmed off this paragraph in order to get a rid of any irrelevant/repetitive information (lines 335-342)

3) Page 5024 : “Such extremely low nutrients concentrations compared well with those previously observed at the same latitudes and season along 45_ E (Table 2; Mohan et al., 2008), suggesting no significant variations of nutrient concentrations (and thereby gradients location) in the subantarctic domain between S-STF and SAF through the productive season." □I don’t understand the point of this sentence. If concentrations were the same at the same latitude and season, then how does this show that there were no variations in nutrients and gradient location THROUGH the productive period? To say that you would need concentrations levels at the beginning and end of the productive season. If this is the case, than the sentence needs to be detailed, otherwise this makes no sense. A lot of results are described in a similar vague way and render the results/discussion as a whole quite unfinished.

Indeed the comparison (Table 1) did not allow to discuss the seasonal variation, as the nutrients concentrations available at these latitudes were recorded slightly earlier in the season (January; Mohan et al., 2008) as compared to our study (Feb.-March). We revised the text accordingly (lines 362-367).

4) Page 5024 “Oligotrophic conditions were further supported by 15N incubation experiments which showed that the new production rate was low in this domain, unlike the regenerated production (Joubert et al., 2011).” What is a low new production rate ? Cite at least and f-ratio if

the data is available…All statements saying this process is low or high should be substantiated by data or adequate references, otherwise it is all too vague.

We now gave the f-ratio values published in Joubert et al. 2012 (this issue) (lines 371-374)

5) Page 5025 line 3 “The accumulation of particulate organic material (Figs. 4–5) also occurred where the highest cell abundance was recorded along the section (Beker and Boye, 2010).” At which stations? Since data is available, why not cite abundance values matching your POC/PON peaks and add a quick description of which species were present ? This would confirm your hypothesis about non-mineralizing algae in this region.

We added a paragraph describing species observed at the confluence zone of the subtropical and subantarctic domains as described in Beker and Boye (2010). There nanoflagellates and dinoflagellates (Gymnodinium spp.) were the most abundant phytoplankton groups in the subtropical domain. Cyanobacteria that are often too small to be recognized clearly in light-microscope studies, also often dominate the picophytoplankton assemblage in oligotrophic regions (Partensky et al., 1999; Zhang et al., 2008). All these species are non-mineralizing species (lines 394-400).

6) Page 5029 line 27 : “On appropriate time scales, we assume that *P* Si and *P* N are valid estimation of the production of BSi and PON.” What is an appropriate time scale here ? And I find this assumption weakly substantiated in the paper.

*Please see response to comments section “**Si and N production estimates”** below**.***

7) Page 5030 line 19 : “Relatively high levels of BSi persisted in the upper water-column (Fig. 5), possibly suggesting a low dissolution rate of BSi leading to an accumulation of BSi.” By which mechanisms ?

Low temperature such as that recorded in the Weddell gyre could slow down the dissolution rate of BSi, as suggested by Natori et al. (2006) (now cited in the text, lines547-549). We have added a sentence to clarify this point.

8) Page 5030 : the paragraph ends with this sentence “Production of diatoms has been already reported south of the SBdy (Arrigo et al., 1999).”

We added “such” to link this statement with the previous sentence (line 555).

9) Page 5031 line 1 : “Hence it is possible that sea-ice melting stimulates the diatom production as recently suggested in the Weddell Sea (Smith et al., 2007), providing sea-ice can be source of iron to the surrounding waters (Boye et al., 2001; Lannuzel et al. 2008, Klunder et al. 2011) that can support local and episodic diatom production.” Boyd et al 2012 should definitely be cited here. (Boyd, P. W., Arrigo, K. R., Strzepek, R., and van Dijken, G. L.: Mapping phytoplankton iron utilization: Insights into southern ocean supply mechanisms, J. Geophys. Res., 117, doi:10.1029/2011JC007726, 2012.)

This reference has now been added in the revised version (line 564).

10) Page 5031 last sentence : “However we do not sufficient information to support this hypothesis.” This is probably the last sentence to use to conclude a paper. Plus there is a word missing.

We have deleted this sentence.

Flawed interpretation / irrelevant discussion points (some examples):

11) Page 5014 line 9 :”An accumulation of BSi up to 0.5 μM was recorded in the top 350m of the southern branch of the ACC and in the Weddell Gyre which may be seen as the presence of heavily silicified diatoms due to lack of iron in this HNLC area.” This sentence is quite speculative as it carries no other information about Fe concentrations, microscopic observations … The fact that 0.5 μmol L-1 BSi are found does not necessarily imply that they were Fe limited.

You might just have a large diatom bloom developping. Now, if you state that biomass was high, but diatom cell count was low, with a temptative Si cell quota back of the enveloppe calculation, and that heavily silicified species (which one) were observed in microscopy, this might be more believable. But 0.5 μmol BSi = heavily silicified cells due to Fe limitation is what I describe as the type of flawed/overly simplified reasonning that is the main issue with this paper.

We removed this statement from the abstract to avoid misinterpretations. However, please, see response for comment section ***"the Fe story"*** *for justification of large heavily silicified cells due to Fe limitations.*

12) Page 5025 "However, although mesoscale eddies episodically increase nutrient supply to relatively poor nutrient water, they may have an insignificant effect on export production and carbon sequestration (Benitez-Nelson and McGillicuddy, 2008)." Did you mean significant or insignificant ? The latter does not make much sense or needs to be better explained as it sounds counter intuitive.

We removed this statement for the sake of not confusing the reader.

13) Page 5025. What is the point of this whole paragraph ? "In addition to mesoscale dynamics, nutrient distributions are driven by the large scale circulation and their signatures in deep waters can be used to better characterize the water-masses (Pollard et al., 2002). For instance the core of SE-NADW that flowed in the northern Cape Basin of the section, along the southwest African continental shelf (Arhan et al., 2003) was depicted by relative low nutrients values (e.g. nitrate < 30 μM; phosphate < 2 μM; silicate < 60 μM), as well as by the salinity signature and oxygen maximum (Bown et al., 2011). In bottom waters the "old variety" of AABW was characterized, at around 36_ S, by extremely high concentrations in silicate (Fig. 2) as previously observed at this latitude (Gladyshev et al., 2008). The high levels in silicate in this variety of AABW probably find its origin in the formation region of AABW close to the Antarctic shelf (Weddell and Ross seas) where this water body deepens with the imprint of high surface silicate levels. While deepening, this water which already has a high silicate content, is enriched in silicate

thanks to exchanges with the Antarctic shelf which contains a lot of opal sediments (DeMaster, 2002)."

We removed this section in the revised version of our manuscript.

14) Page 5025 from line 10 and to the end of the paragraph. Again, what is the point of discussing the deep Si concentration here ? Why not that of PO4 or NO3 ? (see also comment about figure 6 below). This whole section may be interesting but the authors fail to show its relevance to the study of biogeochemical features along the transect, which seem restricted to the surface (0-300 m) layer, looking at the main figures that are discussed.

As suggested by the reviewer above, we removed this section speaking about the deep silicate maximum.

The Fe story. Since BONUS Goodhope was a GEOTRACE transect, I trust that Fe concentrations were thoroughly measured. In each of the three section, the authors cite Chever et al 2010 to support their assumption of whether Fe was limiting or not, based solely on in situ concentrations (which is wrong in the first place), but also fail to adequately document and compare real concentrations values with their own results. Therefore the following three statements seem quite speculative and need to be further substantiated with data (which should be available).

We have now completed our statements with comparisons of dissolved iron concentrations, Fe/C cellular quotas and half-saturation constant for DFe of Antarctic diatoms, flagellates and cyanobacteria published in the litterature.

15) 5025 line 4 STZ/SAZ : "Besides the nutrients nitrate and phosphate, the production was probably also sustained by iron which was not limiting in this frontal zone (Chever et al., 2010)." How can you be this affirmative ? Based on what limiting values ? Were phytoplankton Fe Ks values measured during BONUS? If not, you can only talk of "potential limitation".

Some adequate references about Southern Ocean plankton Fe Ks values or cell quotas compared to in situ concentrations might help here…

A recent paper (see Strzepek, 2011) actually sheds new light on the extent of Fe limitation in the SO and will likely contribute to revise what "limiting Fe concentrations" are in these regions to much lower values than previously considered.

Strzepe (2011) presents Fe Ks values or cell quotas for Antarctic diatoms and Pheaocystis Antarctica, and thus cannot be used in the STZ/SAF region as these species are not present at these latitudes. Instead we discussed potential Fe limitation of cyanobacteria and nanoflagellates/dinoflagellates at the confluence between STZ and SAZ using published studies of Timmermans et al. (2005) and Ho et al., (2003) (lines 400-415).

16) 5027 line 25 PFZ "However BSi export was probably stoechiometrically higher than POC export there, as diatoms were likely limited by dissolved iron (Chever et al., 2010…[ ]. Anyway the combined export of BSi and POC can further support the ballast theory" All too speculative again, cite data to validate this assumption. Do you have data for both BSi and POC export ?

We have now added the POC export data (Planchon et al, this issue) and the estimate of BSi annual export in the PFZ (Fripiat et al., 2012). Those values lead to an estimate of C/Si ratio>1.6 in the exported material in the PFZ, which is lower than those expected in diatoms (Brezinski, 1985). Hence it is suggested that BSi export can be stoechiometrically higher than POC export. Production of heavily silicified diatoms due to iron limitation (De La Rocha et al., 2000) may have caused the increase of BSi relative to organic carbon in the exported material within this region. We have now clarify those points in the revised version (lines 471-475).

17) 5030 line 26 Weddel Gyre "Surface dissolved iron concentrations were however low in the north-eastern Weddell Gyre (e.g. <0.2 nM; Chever et al., 2010) likely limiting the Antarctic diatoms growth." □We now learn that <0.2 nM is likely limiting (based on what?), so what were the concentrations elsewhere in the SAZ/STZ and PFZ ?

The DFe data recorded during the cruise (Chever et al., 2010) are now clearly compared to the half-saturation constant for DFe of Antarctic diatoms (Timmermans 2001, 2004) to discuss any potential Fe limitation during late summer. The reference to Strzepek (2011) is also now mentioned.(lines 557-567).

Si and N production estimates

18) I don't understand how these were calculated. There are no indications in the method section as to how this was done, and the paragraph page 5029 line 21 is quite elusive.
Did the authors use nutrient data from the winter period and from the end of the productive season do run their calculations ? If so, these are not BONUs data, where do they come from ?

We did not use the surface nutrient data form the winter period to run our calculations. We assumed that surface winter nutrient concentrations were similar to those recorded in the remnant winter water layer. We calculated the vertically integrated nutrient depletion during summer by reference to the remnant winter water concentrations, using nutrient profiles. We assume that the nutrient depletion by phytoplankton occurs over a period of 90 days in this area. To avoid any misunderstanding, the equations to estimate the rates are now presented in the method section (lines 181-217).

19) If BSi dissolution rate is close to 50 % in surface waters, then how does this affect your calculations ? Same comment for nitrate and regenerated production ? The authors seem to exclude a number of important nutrient supply processes, it is possible to assess an uncertainty associated to these (and I don't mean the 7% uncertainty thas is plotted in Fig 8, that I am not sure I understand either).

We agree that both BSi dissolution rate and regenerated production can be significant. Unfortunately those terms were not estimated during the cruise, and thus were not considered in our estimates of production rates, with the exception of the fraction of total nitrogen production sustained by ammonium or urea deduced from shipboard determination of f-ratio (Joubert et al., *2011) which was therefore integrated in our calculation. We have now indicated and discussed those points in the method section (lines 181-217).*

Spelling errors (non exhaustive list !)

5015 line 5 : “For instance biogeochemical divides separate the Antarctic domain where the air-sea balance of CO2”

This has been changed in the revised version of the manuscript (line 80)

5015 line 10 “ The conception of the Southern Ocean” franglish : means birth not concept

This has been changed into “dividing of the Southern Ocean” (line 85)

5016 line 8 to line 12 : the three sentences are ackwardly linked together. “Understudied” and “requires more investigation” is redundant. Then the authors tell us about things we know about this region, making the transition difficult to follow.

We deleted the second sentence to clarify this point.

5017 line 6 “ The resolution between two station” Replace “resolution” by “distance”

This has been changed (line 132)

5017 line 19 “Simatzu” spells “Shimadzu”.

This has been corrected in the revised version of the manuscript (line 145)

5017 line 20 “analyses was” correct to “analyses were”

This has been corrected in the revised version of the manuscript (line 148)

5018 line 19 : “the retentive” ??

This has been deleted in the revised version

5022 line 4 “Persistent concentrations” incorrect term line 8 “were less and less deep” incorrect formulation

This has been corrected in the revised version of the manuscript (line 309)

5022 line 24 : “The concentrations were the lowest in the top 100m in the southern side of the ACC, while relatively higher levels (0.6 $\mu g\ l^{-1}$) were recorded at about 100m depth in the Weddell Gyre (Fig. 4)” I believe you meant 0.06 $\mu g\ L^{-1}$? 0.6 is not visible on Fig 4.

Yes, this has been corrected in the revised version of the manuscript (in section 3.4)

5024 line 20 : “radionuclids” □□correct to “radionuclides”

This has been corrected in the revised version of the manuscript (line 377)

5027 line12 : “a strong boom area” correct to bloom

This has been corrected in the revised version of the manuscript

The authors start the paper using “silicic acid” then switch to “silicate”. Pick one and stick with it.

This has been corrected throughout in the revised version of the manuscript, we have replaced silicic acid by silicate

Cardinal directions (North, South, East, West) take capitals, southeast, northeastern etc… do not.

This has been corrected throughout in the revised version of the manuscript

Ackward sentences : 5024 line 17 : "PIC and BSi concentrations were extremely very low in this area, the C and N biomass was therefore not a production of calcifying or silicifying phytoplankton."

This sentence has been edited (section 3.5.2).

Figures :

It would be nice to have the station number on your transect map and all biogeochemical transects. In figure 6 for instance, you cite station number and do not explicitely say in which region they are. Since station number is not mapped anywhere, one has a hard time figuring out where these profiles are exactly. Where are eddy-S and eddy-M located on Fig 1 to 5 and 7 ?

We have added the stations number in figure 1 and the locations of both eddy S and M in figure 1 and figure 2

Figure 5 : units : in the legend in µmol L-1, on the figure in µM. Please indicate the same notation. It is usually admitted that dissolved species are expressed in µM while particulates are indicated in µmol L-1.

This has been corrected in the revised version of the manuscript

ODV citation "*Figure from ODV (Reiner Schlitzer) »* is incorrect in all figure legends. Adequate citation is "Schlitzer, R., Ocean Data View, http://odv.awi.de, 2012" as stated in ODV's user's guide.

This has been corrected in the revised version of the manuscript