Review of manuscript bgd-9-5011-2012

Description of the biogeochemical features of the subtropical southeastern Atlantic and the Southern Ocean south off South Africa during the austral summer of the International Polar Year

This manuscript presents a solid data set of biogeochemical parameters obtained during the BONUS Goodhop cruise along a transect from South Africa to the southern boundary of the ACC.
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.

Inappropriate/vague result description (some examples):

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.

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 :

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.

Page 5024 "Oligotrophic conditions were further supported by ^{15}N 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.

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.

Page 5029 line 27 : "On apropriate 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.

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 ?

Page 5030 : the paragraph ends with this sentence "Production of diatoms has been already reported south of the SBdy (Arrigo et al., 1999)." ➔ Yes ? Diatoms are often found a bit everywhere… Be more specific, with biomass data, relative contribution, dominance or not….

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.)

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.

Flawed interpretation / irrelevant discussion points (some examples):

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.

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.

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)."

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.

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).

1. 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.

2. 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 ?

3. 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 ?

Si and N production estimates

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 ? 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).

Spelling errors (non exhaustive list !)
5015 line 5 : "For instance biogeochemical divides ~~separate~~ the Antarctic domain where the air-sea balance of CO_2"

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

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.

5017 line 6 " The resolution between two station" Replace "resolution" by "distance"

5017 line 19 "Simatzu" spells "Shimadzu".

5017 line 20 "analyses was" correct to "anayses were"

5018 line 19 : "the retentive" ??

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

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 µg l^{-1}) were recorded at about 100m depth in the Weddell Gyre (Fig. 4)" I believe you meant 0.06 µg L^{-1} ? 0.6 is not visible on Fig 4.

5024 line 20 : "radionuclids" ➔ correct to "radionuclides"

5027 line12 : "a strong boom area" correct to bloom

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

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

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."

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 ?

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}.

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.