Anonymous Referee#2

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

1) Oligotrophic conditions in the surface waters in the northern (subtropical) region are supported by relatively low export production estimates from cited literature. The authors suggest no significant variation in nutrient concentrations throughout the growing season, however this highly turbulent region (Agulhas retroflection), observed Eddy activity and the elevated and variable chl concentrations in the surface throughout the year possibly could indicate highly variable conditions through the growing season. In the Subantarctic where the eddy activity was observed, tongue-shaped density contours are called on in support of the observed nutrient distributions. It is not clear how these silicate from Fig6 (taking the individual profiles adjacent to and within the eddies) support this trapping of 'distant waters' (in the cyclonic eddy) and winter convection (in the anticyclone). I think this section could benefit from seeing (a figure) the actual density contours from the cited literature.

A close up of nitrate and silicate profiles superimposed to the density contours is already presented in Arhan et al. (2011) cited in the text. We therefore believe that including such a figure is not necessary as the paper cited already details the formation and origin of the eddies mentioned in our study.

2) A diatom bloom in the Polar Front zone is inferred from the silicate gradient across the Polar Front, the depletion of silicate, low concentrations of chl-a, and relative accumulation of ammonium, based on previous published data in other regions. It is however difficult to infer temporal variability in nutrient concentrations from the current dataset, particularly referring to the bloom as a single event.

Bloom of large heavily silicified diatoms reported in late spring (October-November) in the Polar Front region can lead to a massive depletion in silicate as documented in Quéguiner et al., (1997) and Bathmann et al., (1997). Comparison of silicate concentrations in the PFZ (see Table

1) suggests that the concentrations were lower in the late austral summer (our study) compared to spring (e.g., October 1992; Löscher, 1999). Southward move of the sharp gradient of silicate across the PF from spring towards late austral summer has also been observed in the Pacific sector of the Southern Ocean (Franck et al., 2000; Nelson et al., 2001). Hence we suggest that silicate can be depleted over the productive season in the PFZ.

3) Seasonal drawdown of silicate and nitrate well as daily production rates are estimated from seasonal drawdown of silicate and nitrate assuming a 90 day productive period, is useful, however the dataset gives stronger supports for spatial distribution patterns than for temporal variability. For instance Si* data support the spatial distribution and potential decoupling of Si and NO3 cycles. The spatial distribution is well supported the data does not support temporal removal of silicate which makes the temporal explanation speculative.

The dataset was recorded during the summer, hence the spatial distribution of nutrients (especially Si) will indeed reflect the situation at the end of the productive season. The comparison of Si before and after the productive season (Table 1) suggests a decrease of Si in the PFZ over the productive season, supporting a temporal removal of Si.

4) The final paragraph in the conclusions is also speculative, and does not warrant a conclusion, but should be integrated into the main text of the manuscript, instead of being tagged on at the end.

We have removed the last paragraph of the conclusion.

5) The conclusions should better highlight the relevance of this work with regard to the Southern Ocean biogeochemical zonation.

The conclusions have been re-written to better highlight those points (lines 571-574).

6) Specific and Technical comments Introduction:

P5015 line5 “biogeochemical divides separate” could read ‘biogeochemical features separate or divide’.

This has been corrected in the revised version of the manuscript (lines 79-80)

The use of “the conception. . .” (P5015 line10) is unclear.

This has been changed in the revised version of the manuscript (line 85)

P5015 line 28,: it is “annual production that is potentially limited to annual supply” not production in general.

This has been corrected in the revised version of the manuscript (line 100)

P5016 line 13,: what is meant by “biogeochemical functioning”? Does it mean biogeochemical features?

Yes, this has been corrected in the revised version of the manuscript (line 112)

Sampling: P5017 line 2,: Figures are mislabelled. The reference to Fig. 2 ($Si(OH)_4$, NO_3 and PO_4,) should be moved earlier in to distinguish it from Figs 3-4 which is upper 300m plots.

This has been corrected in the revised version of the manuscript (126)

P5018 line 6,: abbreviation for chlorophyll-a already defined earlier in text, and should be used consistently.

This has been corrected throughout the revised version of the manuscript

P5018 line 14,: replace “by acidic (HCl) smokes” with ‘with concentrated HCl’.

This has been corrected the revised version of the manuscript (line 170)

Results: P5019 line3,: ". ..domain 'which' extended. . ." . the word 'which' or 'that' is missing from the text.

This has been corrected in the revised version of the manuscript (line 221)

P5019 line13-14,: is the subtropical and subantarctic zones the same as subtropical and subantarctic domain? It is not clear where the boundaries of the specific zones/domains are.

Yes, the boundaries and front positions delimiting specific zones/domains are given in section 3.1 and indicated in figures.

P5019 line 19,: reference required for eddy-slope interaction.

The reference Baker-Yeboah et al. (2010) is now added for the eddy-slope interaction (Lines 235-237).

P5020 line 30: Winter water is not defined anywhere, and it is not shown from the data, so it needs a reference.

Winter Waters are now defined and a reference has been added (line 276).

P5021 line 14: "Nitrate and Phosphate increased southward. . .". This increase with latitude is only apparent in the surface not in the entire water column and should be stated as such. I think the reader would benefit by indicating/labelling the water masses on Figure 2, since although it is described in the text (and referenced) it is not always clear where these water masses are with respect to its nutrient characteristics, without having to go to a figure in the referenced manuscripts.

The term "surface" has been added in the text (line 286), and the water masses labels have been added to Figure 2 to include water masses described in section 3.1.

P5022 line4,: Rephrase "Persistent concentrations. . .".

This has been corrected in the revised version (line 308).

P5022 line23,: which concentrations is referred to in the top 100m?

It is referring to the concentrations of phaeopigments. This is corrected in the text now (line 328).

P5023 line 4,: replace "undetectable" with 'below detection limit". The latter speaks of the instrumentation limits whereas the former is absolute.

This has been corrected in the revised version of the manuscript (line 335)

Discussion: P5024 line 5: Silicate concentrations remained low north of the PF despite increases in the other major nutrient concentrations while all nutrients were depleted in the surface north of the Subantarctic Front. Consider revising this opening first sentence.

The sentence has been corrected (from line 361).

P5024 line 18,: ". . .extremely very low. . .". Use one or the other, not both.

This has been corrected in the revised version of the manuscript (line 346)

P5025 line 10,: 'upward pointing tongues" of silicate profiles within and adjacent to the observed cyclonic eddy are given as support for trapping of distant waters (and later in the text winter convection fig6b). However it is not clear from the individual silicate profiles. Is a similar pattern observed for nitrate and phosphate? Perhaps a reference to a section (ie fig2) would suffice for

the changes in nutrient isopycnals? However it is apparent from Fig 2 either, without looking at the density surfaces of Arhan et al 2011.

The upward pointing tongues of high Si values in the cyclonic eddy are clearly shown in Figure 6a, as the low Si values in the anticyclone ring in Figure 6b, where stations in the eddies are compared to outside the eddies. These features are indeed also discernable for N and P in Figure 2 which is now indicated in the text (lines 416-418).

P5027 line 25,: spelling of stoichiometry.

This has been corrected in the revised version of the manuscript

P5028 line 14,: 'extent' instead of "extend".

This has been corrected in the revised version of the manuscript (line 498)

P5028 line 22,: Several limiting factors (light, Fe, Si) not just Fe, could be responsible for augmented uptake ratios of Si:N away from 1. Although the Fe concentrations are low in the surface (<0.2 nM) since Fe limitation was not explicitly tested, the authors should highlight other possibilities.

See response to comment section "the Fe story" of reviewer #1

P5030,line12,: Figure 9 and 10 absent from manuscript? Plots referred are panels b and c in Figure 8.

This has been corrected in the revised version of the manuscript (line 528 and elsewhere)

P5031 line 2,: Sea ice inferred for possible Fe deposition. Is sea-ice melt possible for diatom blooms stimulation this late in the austral summer season at such northerly latitudes (55oS)?

We added a sentence to specify that icebergs were observed during the cruise, further supporting our hypothesis (Lines 563-568).