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Interactive comment on "Nutrient regimes control phytoplankton ecophysiology in the South Atlantic" *by* T. J. Browning et al.

Anonymous Referee #2

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General comments:

The study by Browning et al. focuses on the photophysiology of phytoplankton in the complex environment surrounding the SSTC (South of the South Subtropical Convergence) in the South Atlantic Ocean. The photophysiological response of phytoplankton is estimated via Fast Repetition Rate fluorometry measurements as well as a series of shipboard incubation experiments with Fe addition. Several factors suspected to influence the algal photophysiological response are analyzed, including the composition of phytoplankton communities and the macro- and micro-nutrient regime.

This is a good paper that presents a nice dataset and interesting outcomes. Nevertheless the paper has weaknesses that need to be addressed to make the conclusions clearer and more robust. I found the analysis of the relationships between the

photophysiological and the biogeochemical measurements essentially qualitative. The conclusions could be significantly strengthened by a more quantitative analysis, for example using simple linear regression or multiple regression. There should be clearer connections between the authors' results and their hypotheses and conclusions. On some occasions the conclusions are quite speculative and need to be supported by data (examples below).

Specific comments:

There are nice features that are totally omitted from the description of the results, even with certain inaccuracies. I understand, and support, the authors' wish to focus the analysis and interpretation on some specific features relevant to Fe-fertilization etc. However I feel it is important that they describe with accuracy the field data as they may be of great interest to some readers (as in a sense would be a "cruise report"). This is also needed to derive robust conclusions.

- For example there is no mention of the most striking feature in the distribution of macronutrients, i.e. a maximum of nitrates and phosphate at depth at -45E (Figs. 2a and b). This maximum appears to coincide with an increase in the concentration of silicate (Fig. 2c). Theses conditions could to some extend explain the SCM of fucoxanthin (Fig. 5e)?

- p. 11982 l. 10-13 "Concentrations of silicate were uniformly low in surface waters [. . .] apart from near the South African and American coasts": I do not fully agree with this. The concentration of silicate increases dramatically near the South American coast (reaching max values of $\sim$12 uM and probably higher?). In contrast, near the South African coast, the concentration remains relatively low at surface but increases significantly at a depth >50 m ($\sim$5-6 uM?).

- Fig. 2d shows a clear increase in the concentration of DFe just north of the ACC (with values probably close to those observed near the South American coast). Why is not that mention in section "3.2.2 Micronutrients"?

-P. 11984 section 3.4 “Phytoplankton community structure”: It is not clear what the authors mean by “19’-Hex also dominated diagnostic pigments”. I am assuming they calculated the fraction contribution of each pigment to the sum of several diagnostic pigments. Which diagnostic pigments are the authors referring to? Is this sum used as a proxy for the total algal biomass? It is important to clarify this point otherwise the fraction contributions shown in Fig. 5 are quite difficult to interpret. In addition, I suggest introducing more details regarding the distribution of the pigments. It would be informative to provide the fraction contributions of the major pigments in the study area. The current description is relatively qualitative. For example, 19’Hex contributes up to 50% to the diagnostic pigment pool in the western basin and 10-30% in the eastern basin. As written the algal community in the eastern basin sounds dominated by small cells. However Fig. 5 shows that zeaxanthin and divinyl chlorophyll a contribute a significant yet not dominant portion of diagnostic pigments (e.g. up to 15% only for divinyl chlorophyll a and 20% for zeaxanthin). The contribution of fucoxanthin reaches maximal values that cannot be guessed from Fig. 5e at the very end of the transect (close to the South American coast: 100%?) and large values at ∼-45E, especially at depth. . . 19’But, a biomarker pigment of chrysophyceae and pelagophyceae, is also an important pigment in the eastern part of the transect (contribution up to 45%?).

- P. 11984 l. 19-20 “Apart from the station sampled close to Gough Island, low values of Fv/Fm (Fv/Fm < 0.3) are seen throughout the mixed layer in the sub-Antarctic ACC waters of the eastern basin, with increases at greater depth (Fv/Fm > 0.3)”: Again the description of the results is relatively convoluted. I cannot see any special feature in Fv/Fm around Gough Island from Fig. 6a. I would rather say that, in the eastern basin, Fv/Fm values are low within the mixed layer (∼0.3 and lower) and increase with depth (e.g. >0.4 below 80 m), except nearby the South African coast where Fv/Fm show high values throughout the entire water column.

- P. 11984 l. 26-27 “higher values in the eastern basin than in the western basin and coastal waters are seen”: This is true and I would even add that the lowest values of

σPSII are observed at the coastal stations.

- P. 11986 l. 6-9: The authors do not mention the surface sample located in the eastern basin with very high Ek value (∼350 umol photons m-2 s-1). Why?

- P. 11986 section “3.6 Fe addition experiment”: This section is extremely short whereas Fe-fertilization is one of the main focuses of the paper. I think a detailed description of Figs. 7a and b would be most appropriate.

- P. 11986 l. 23-24 “Several factors are thought to control values of Fv/Fm, including light climate [. . .] Using accessory pigment, AFC, irradiance. . .”: Where are the irradiance data? Where are they discussed and how do you come to the conclusion that they cannot explain the spatial variations in Fv/Fm?

- P. 11989 l. 28-29 and p. 11990 l. 1-8 “RLC parameters showed [. . .] instead being dominated by vertical gradients within the gyre-type waters, likely related to photoacclimation. . .”: This is quite speculative considering that RCL parameters were measured in the mixed layer exclusively, except for a few gyre-type stations where measurements below the MLD were also performed.

- P. 11990 section “4.2 Controls of the development of the SSTC bloom” – Hypothesis related to the narrowing of the SSTC chlorophyll band: First, the “narrowing” is difficult to observe from the chlorophyll images (Fig. 3). To me the most striking feature is a sort of filament of enhanced chlorophyll concentration (∼ > 1 mg m-3) along the South American coast that expands in the western basin in November and December. I suggest displaying a region that is larger than just the study area so the increased chlorophyll band is obvious. Second, I do not understand how the authors conclude that the “narrowing of the chlorophyll band [is . . .] caused by the bloom-induced depletion of macronutrients to the north of the SSTC and Fe to the south”. Wouldn’t you need seasonal data to come to this conclusion? I may have missed something here, but then how the results lead the authors to this conclusion should be clarified. I suggest adding to the chlorophyll map the currents and additional geographic features as shown on the

temperature map (Fig. 1a).

- P. 11989-11990 section “4.2 Controls of the development of the SSTC bloom” – Fe-fertilization around Gough Island: It would be interesting to know the % difference in DFe, Chl and Fe/Fm between the station located at Gough Island relative to the stations located nearby in the Fe-limited area. For example, Fig. 8 shows an important increase in DFe ($\sim$ 0.35 nM at Gough Island as compared to $\sim$0.1 nM in surrounding waters). The corresponding increase in chlorophyll, which would support the hypothesis of natural Fe-enrichment from the island, does not seem as elevated (0.9 to 1 mg m-3?). Also it would be interesting to look at the composition of the algal community around the island as small diatoms often dominate the community in Fe-fertilized waters.

Technical corrections:

- Although it may seem obvious I suggest adding the unit of longitudes on the figures where applicable. - Fig. 1a: y-axis has no label. - Fig. 4a title: Units should be m2 (mg Chl)-1 instead of m2 mg-1 Chl. - Fig. 8: I recommend drawing both x- and y-axes as the current figure is extremely difficult to read. This figure is important as it summarizes the results and help with the conclusions. I recommend make it a little nicer (axes easier to read, larger etc.).

Interactive comment on Biogeosciences Discuss., 10, 11969, 2013.