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Interactive comment on "Modelled estimates of spatial variability of iron stress in the Atlantic sector of the Southern Ocean" *by* Thomas J. Ryan-Keogh et al.

Thomas J. Ryan-Keogh et al.

thomas.ryan-keogh@uct.ac.za

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This paper addresses the long-standing question of the role of iron availability on the photosynthetic parameters of naturally occurring populations of plankton in the Atlantic sector of the Southern Ocean. Only 6 stations are studied, approximately positioned in the SAZ, the PFZ, the Antarctic zone and the MIZ. The experiments consisted of producing photosynthesis-light curves over 24 hours of incubation under iron enrichment conditions vs. no enrichment. There are (too) many major methodological problems associated with these results.

1) No details are given on the pre-treatment of the incubation vials (ultra-clean condi-

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tions?)

All experimental conditions were carried out in a class-100 clean container. Experimental bottles were pre-treated with detergent and acid (Hydrochloric) as per trace metal clean standards. Please see p.4 lines 115 – 118.

“Inside a trace metal clean laboratory class-100 container, bulk trace metal clean seawater was decanted unscreened into an acid-washed 50 L LDPE carboy (Thermo scientific) to ensure homogenization; this was then redistributed into acid-cleaned 1.0 L polycarbonate bottles (Nalgene). All experimental conditions were conducted and carried out following trace metal clean standards and conditions.”

2) The duration of the incubations (24 hours) does not make it possible to obtain an estimate of the in situ photosynthetic parameters of the natural phytoplankton because it is known that the adaptation time of these parameters in response to a change in light regime is on the order of the 2 to 6 hours. Within 24 hours, each incubated sample thus has ample time to adapt to the light intensity at which it is incubated. Nevertheless, these experimental values are used by the authors (apparently unaware of this major problem of different time scales between light acclimation and iron relief) in an extrapolation across the entire Atlantic area in order to evaluate the primary production of this sector.

The reviewer here is highlighting that the incubation time would make these measurements unsuitable estimates for the levels of community PP in the Southern Ocean, which we agree with. The results are designed to highlight the potential differences between the treatments, rather than absolute numbers of PP. As any long-term changes in iron limitation could ultimately lead to community structure change or potential secondary limitation by silica for example.

Light acclimation can change the iron quota of in situ phytoplankton – higher light is expected to decrease the iron quota in Antarctic phytoplankton species (Strzepek et al., 2012). However, the light ranges of the experiments (0 – 400) fall below the maximum

light intensities measured in situ at the time of the experimental set up (see Table 1). This would suggest that the design would be expected to increase the iron demand and we would expect to see larger increases in PP following iron addition.

Statements to highlight this shortcoming have been added into the text, see p. 13 lines 420-423.

“It should be noted however, that light acclimation can occur on time scales of between 2 – 6 hours and as such be reflected in the potential iron demand, with a lower demand expected at higher irradiances (Strzepek et al., 2012). Such incidences would impact the observed differences between PE parameters in control versus Fe addition experiments. However, since the light ranges of the experiments (0 – 400) fall below the maximum light intensities measured in situ (Table 1), acclimation responses are unlikely to dominate and indeed if occurring are would result in an underestimation of the differences between control and addition experiments. The experimental design of 24 hours, whilst suitable for investigating iron limitation, means that results are not truly representative of in situ photosynthetic parameters and should not be interpreted as such.”

3) The sampling strategy is curious with one of the samples (station 5) collected under the mixed layer. In addition, at the end of the manuscript, one discover that there were three occupations of the transect with a total 6 stations sampled (?)

A consistent depth was chosen between 35-50m was chosen to minimise changes between experiments. The density profile of station 5 showed 3 distinct layers of water, suggesting that there were 2 mixed layers (which is known to occur in the Southern Ocean). The depth for the mixed layer presented in the text was the first depth at the criteria T – 0.2C was met. A secondary mixed layer was determined at 56m and the text has been reflected to indicate this, please see p. lines 252-254.

“The CTD density profile at experiment 5 was indicative of 2 mixed layers present, with the experiment performed above the deeper of the mixed layers (∼56m).”

4) There is no measurement of DFe in the samples themselves. What about contaminations of controls?

DFe was measured from the initial set up water as listed in table 1, but due to constraints with water volumes in the bottles DFe was not measured in the experimental bottles. However, contamination of control bottles would be evident as outliers in the data reported, and since all experimental results showed good exponential fits consistent with theoretical predictions of the response of production to varying light, we can safely assume there is very little to no contamination in any of the incubation bottles.

5) The measurements are not carried out in triplicate and it is therefore impossible to evaluate the precision in the estimation of the photosynthetic parameters.

This is correct and as such a statement has now been added to the methods to highlight the reader to this problem. Please see p. 4 lines 126-127.

“Due to physical constraints, the experiments were not conducted as triplicates, and as such evaluation of the precision/error within experiments is not possible.”

6) Inconsistencies in the determination of the light attenuation coefficient (40% difference between the PAR profile and the empirical equation as a function of chlorophyll). It is then not known which estimate is used in the primary production model.

There are known caveats to using empirical calculations to derive Kd from Chl (Morel et al., 2007), and we highlighted this in the methods to draw attention to the readers. An additional line has been added to the discussion, p. 15 line 477.

“Biomass (Chl), as represented through Kd, did exert a large influence on PPwc (up to 59%, Fig. S4), but this influence could be overestimated due to potential errors in the calculation of Kd (Morel et al., 2007).”

As PAR profiles with depth were only available when CTDs were performed this limited the data set to only 6 profiles with latitude. Interpolation was performed using various methods to allow the calculation of PP at a higher resolution along the transect.

Also many problems in the expression of results:

1) a salinity change of 33.71 to 36.51 is considered "not distinct" (line 235). A chlorophyll range between 0.84 and 2.3 is considered as a "small range of variability", with individual values considered to be "low", indicating a total ignorance of the oceanography of this region.

I find the phrase 'total ignorance' highly offensive and a poor example of constructive criticism. Nonetheless, we thank the reviewer for drawing our attention to the salinity values, which were incorrectly reported from a malfunctioning CTD sensor. Bottle samples were instead used to provide the correct salinity range. Please see p. 9 line 244.

"Initial temperatures displayed a characteristic decrease from 10.80°C at the most northerly location to -1.51°C at the MIZ, whereas there were no distinct differences in salinity ranging from 33.70 to 33.88."

Chlorophyll concentrations in proximity to the islands and coastal regions can exceed 2.3 ug/L at the height of the growing season. Not displayed here but measured during the cruise were surface chlorophyll concentrations that exceed 5.0 ug/L up to a maximum of 11.5 ug/L. The text has been altered to reflect that this is a small range of variability in the chlorophyll concentrations relative to those measured during the transects. Please see p.14 lines 396-397.

"...when compared to the range of chlorophyll concentrations measured throughout the entire cruise (0.01 – 11.25 μg L-1)."

2) The presentation of the photosynthetic parameters (paragraph 3.2.) is surprising. It is written: "PE curves for carbon uptake (C) (Fig. 2, Fig. S1), summarised in Table 2, display consistent results with greater values of B and PB with the addition of iron compared to the control treatments (Fig. S2).", which is completely inconsistent with Figure S2. Moreover, the choice of the figure showing the effect of iron on the P-E

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curve (figure 2) is at the limit of what is acceptable: it is the only good relation of this type on the 6 curves presented on figure S2. On top of that I do not understand how the parameters have been inferred from experimental data. I defy anybody to see significant changes in the photosynthetic parameters, related to iron enrichment, from the curves presented as supplementary material.

The parameters were determined following the standard equation of Platt et al. 1980 as outlined in section 2.2.

I can find no literature which suggests what is the acceptable limits for PE curves. All the resulting fits from the data presented here had r2 values >95% following multiple iteration non-linear least square fits.

The full results of the PE parameters are presented in table 2. Perhaps this is not made clear in Figure S2 due to the choice of colour scale, as such a new figure (S3) has been added to the supplementary material to highlight the differences between the treatments (which are small in the first three experiments in the SAZ but more substantial in the last three experiment south of the PF) as a bar chart – see below.

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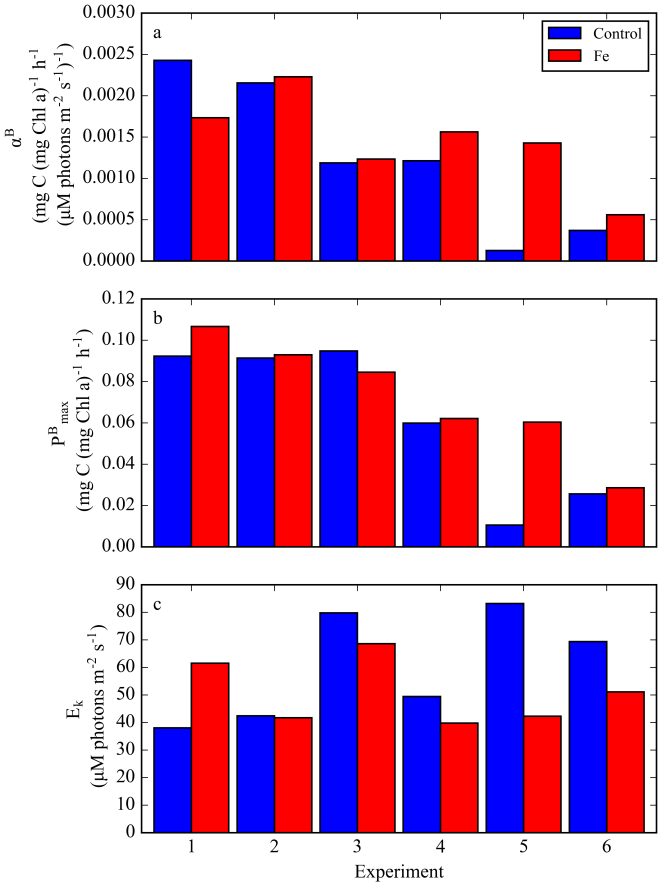
a
Control
Fe
α^B
(mg C (mg Chl a)$^{-1}$ h^{-1}
(µM photons m^{-2} s^{-1})$^{-1}$)
0.0030
0.0025
0.0020
0.0015
0.0010
0.0005
0.0000
b
P^B_{max}
(mg C (mg Chl a)$^{-1}$ h^{-1})
0.12
0.10
0.08
0.06
0.04
0.02
0.00
c
E_k
(µM photons m^{-2} s^{-1})
90
80
70
60
50
40
30
20
10
0
1
2
3
4
5
6
Experiment

Fig. 1.