Biogeosciences Discuss.,
doi:10.5194/bg-2017-74-RC2, 2017

Biogeosciences
Discussions
Open Access

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.

B. Quéguiner (Referee)

bernard.queguiner@univ-amu.fr

Received and published: 28 April 2017

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

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. 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 αB 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 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.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-74, 2017.