

## ***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.***

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### General comments

This study examines the responses of phytoplankton photosynthesis versus irradiance parameters to iron additions across a Southern Ocean transect, and addresses an important set of classic questions about iron and light limitation interactions in this region. Although quite a number of related studies have been performed throughout the region in the past, this one is unusual in incorporating such a long latitudinal transect spanning the major Southern Ocean biogeochemical provinces. The authors also attempt to integrate their results with a simple model describing the influence of iron and light on primary productivity. In general, the work was carefully done and the results are

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definitely worthy of publication.

Their iron addition incubations during the PE curve experiments were deliberately short, only 24 hours, in order to avoid changes in biomass. While this is probably long enough to see initial changes in photophysiology, as they note on p. 13, it does seem possible that iron-mediated photosynthetic responses might not be fully completed in that time. More importantly, the increases in biomass and shifts in communities that would have happened had they drawn their iron addition experiments out longer are important to consider too- as they say when discussing the results of their sensitivity analysis on lines 425-426, ‘Biomass. . . did exert a large influence on PP (up to 59%...)’. Since their incubations weren’t long enough to evaluate this biomass increase, the +Fe depth-integrated productivity calculations and model they present in Figs. 5 and 6 are undoubtedly considerably lower than would be the case if the community experienced sustained relief from Fe limitation. I think the results and conclusions obtained here need to be qualified as applying only to the initial responses of these communities to iron additions, and it should be explicitly recognized in the text that they cannot be applied to understand longer term community responses (which would also include taxonomic composition shifts).

Likewise, the fact that in some of these short iron addition experiments PE parameters did not change (for instance, stations 1 and 2 in the SAZ, p. 10 lines 271-272 and Table 2) cannot be taken as evidence for lack of iron limitation at these stations. Much longer duration SAZ iron addition experiments published way back in 2001 show similar lack of changes in  $\alpha_B$  and  $P_{Bmax}$ , but in those same experiments the iron additions led to much higher biomass and to community composition changes- clear evidence that the community actually was iron-limited (Hutchins et al. 2001 JGR 106). Many other past studies have also conclusively confirmed ecological and physiological iron limitation of SAZ communities; one of the most highly cited is the SoFex in situ iron fertilization study of Coale et al. (2004, Science 304).

By the way, these older papers also carefully considered the effects of Si limitation

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(or iron/Si co-limitation) of diatoms in the SAZ, which is not considered at all in the present paper. Clearly though, the gradient in Si availability this study covered was much greater than any gradient in N, P, or even Fe (Table 1, p. 5), and this is probably the reason that according to their pigment analyses haptophytes were dominant in the SAZ and diatoms south of the Polar Front (lines 245-247). Could changes in the makeup of these communities driven by Si availability have any influence on their PE results? This is probably worth considering briefly in the discussion. Finally, as they briefly acknowledge on line 398, this cruise spanned two full months, and so in practice examined a seasonal shift as well as a latitudinal gradient. The relative importance of iron and light limitation (yes and Si limitation!) changes across the growing season in different ways in the various Southern Ocean regimes they examined (see a simplified diagram of this seasonal pattern in Fig 2 of Boyd et al 2010, L&O 55). It would be worthwhile to discuss this aspect of their study in more detail in the text.

#### Specific comments

Abstract lines 19-22. These quantitative values need to be better linked to the specific photosynthetic parameter each belongs to, it requires quite a bit of peering back and forth for the reader to figure out which number goes with which parameter. A simple rewording would be helpful.

Line 43. The Arrigo et al. 2013 reference on ice cover changes given here deals with the Arctic, not the Antarctic, and should be replaced.

Line 123 and Figure 2. Obviously the maximum irradiance of 400 used in the PE curves was still below photoinhibiting levels. It would have been interesting (if logistically challenging, as I admit!) to extend it out to higher irradiances to add some perspective on this end of the curve.

Line 171, section 2.5. Another thing I wish the authors had done was to get better taxonomic information than can be obtained by the simple class-level distinctions possible through HPLC pigment measurements along with CHEMTAX. Just because there

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were diatoms all along their transect doesn't mean they were ecologically or biogeochemically equivalent. In fact, SAZ diatoms tend to be small, delicate, lightly silicified pennates while diatoms south of the Polar Front are typically much more robust and silicified, and much more likely to be significant in export. It seems a shame to do all this work, and then be limited in the wider inferences that can be drawn due to having only bulk measurements of productivity and broad general classes of phytoplankton. Some more detailed taxonomic and functional information would have made the paper more useful and interesting.

Line 348. The Shi et al. 2007 reference is a study on the tropical N<sub>2</sub>-fixing cyanobacterium *Trichodesmium*, and is not appropriate here. Please add a study on Southern Ocean phytoplankton, or at least on eukaryotic phytoplankton in general.

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