

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

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done and the results are 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).

A statement to this effect has been added to the discussion to highlight that these results only reflect initial responses and do not take into account community shifts and longer term responses. Please see p. 13 lines 371-373. “However, it should be noted that a time-length of 24 hours may not be sufficient to complete alleviate the iron-mediated photosynthetic response and as such these results may only reflect initial responses rather than longer term community level responses to relief from iron limitation.”

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 alphaB and PBmax, but in those same experiments the iron additions

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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).

A statement has been added to discuss the potential for longer term relief from iron addition that may not have been achieved in this study. Please see p. 14 lines 410-412. "This may not reflect a lack of iron limitation in the SAZ, as it has been demonstrated previously that there is ecological and physiological iron limitation (Coale et al., 2004), with longer experiments demonstrating increases in P_{max} and α following iron addition (Hutchins et al., 2001)."

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

A statement to address this has been added to the discussion, see p. 13 lines 389-397. "A proxy for the community structure that utilized the ratio of the 2 dominant groupings (Diatoms and Haptophytes) also indicated strong significant relationships with the PE parameters, which is potentially driven by Si availability controlling community structure. Indeed, it has been demonstrated that in the SAZ, where haptophytes dominated during this study, there is evidence for Fe-Si co-limitation. In a study by Hutchins et al. (2001) it was demonstrated that the addition of both Fe and Si resulted in the greatest responses in chlorophyll and the photosynthetic parameters. The relationship here may not be driven by Fe availability on the PE parameters, but rather community level

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limitation."

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.

The seasonal shifts in limitations have been added to discussion, please see p. 15 lines 477-482 and lines 494-495. "It must be noted that the transects will not only reflect latitudinal gradients but will also contain a seasonal signal as the cruise spanned 2 months across the austral summer. A seasonal shift in community structure of haptophytes increasing their dominance beyond the SAZ into the PFZ was evident from underway measurements of community structure (data not shown); indicative of seasonal Si limitation for this region (Boyd et al., 2010). Moreover, the complex seasonality of this region represents shifts between varying co-limitations that will be represented not only in the PE parameters measured but also in the additional components utilized to calculate PPwc."

"As such, we can conclude that the primary driver of the latitudinal trend in $\Delta PPwc$ is the result of changes in the maximum photosynthetic capacity (P_{max}) to iron addition, however, regions along the transect may be experiencing seasonal co-limitation of Fe and Si, particularly during the third transect conducted during late summer."

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.

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Wording has been restructured to make it clearer, see p. 1 lines 18-22. “A series of iron addition productivity versus irradiance (PE) experiments utilising a unique experimental design that allowed for 24 hour incubations were performed within the austral summer of 2015/16 to determine the photosynthetic parameters α_B , P_{Bmax} and E_k . Mean values for each photosynthetic parameter under iron-replete conditions were α_B : 1.46 ± 0.55 ($\mu\text{g} (\mu\text{g Chl a})^{-1} \text{h}^{-1} (\mu\text{M photons m}^{-2} \text{s}^{-1})^{-1}$), P_{Bmax} : 72.55 ± 27.97 ($\mu\text{g} (\mu\text{g Chl a})^{-1} \text{h}^{-1}$) and E_k : 50.84 ± 11.89 ($\mu\text{M photons m}^{-2} \text{s}^{-1}$); whereas mean values under the control conditions were α_B : 1.25 ± 0.92 ($\mu\text{g} (\mu\text{g Chl a})^{-1} \text{h}^{-1} (\mu\text{M photons m}^{-2} \text{s}^{-1})^{-1}$), P_{Bmax} : 62.44 ± 36.96 ($\mu\text{g} (\mu\text{g Chl a})^{-1} \text{h}^{-1}$) and E_k : 55.81 ± 19.60 ($\mu\text{M photons m}^{-2} \text{s}^{-1}$).”

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.

The Arrigo et al. 2013 reference has been removed and the following references have been inserted into this section.

Close, S. E. and Goosse, H.: Entrainment-driven modulation of Southern Ocean mixed layer properties and sea ice variability in CMIP5 models, *Journal of Geophysical Research-Oceans*, 118, 2811-2827, 10.1002/jgrc.20226, 2013.

de Lavergne, C., Palter, J. B., Galbraith, E. D., Bernardello, R., and Marinov, I.: Cessation of deep convection in the open Southern Ocean under anthropogenic climate change, *Nature Climate Change*, 4, 278-282, 10.1038/nclimate2132, 2014.

Zhang, J. L.: Increasing Antarctic sea ice under warming atmospheric and oceanic conditions, *Journal of Climate*, 20, 2515-2529, 10.1175/jcli4136.1, 2007.

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.

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Indeed, the limitations of the incubator set up did prevent us from being able to determine potential levels of photoinhibition. Future experiments planned will encompass a change in experimental set up to try and achieve higher irradiances. Please see p. 10 lines 274-276. “Due to constraints in light levels for the incubator set up, light levels that may result in photoinhibition ($>400 \mu\text{mol photons m}^{-2} \text{s}^{-1}$) were not achieved and as such no measurements of β were determined.”

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 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.

We agree with the reviewer that further taxonomic data would enhance certain aspects of this paper, as such a further study is being conducted using microscopy counts alongside coulter counter and HPLC data. However, this data analysis is ongoing and will not be available for this manuscript.

The taxonomic data we do report however states that the SAZ is dominated by haptophytes not diatoms. In addition we also present information on the dominant size structure from coulter counter effective diameter, which ranges latitudinally from a minimum in the SAZ (4.29 μm) to a maximum in the MIZ (8.59 μm)— please see p. 9 lines 249 – 256. Diatoms only become dominant from experiment 4 onwards with subsequent changes in effective diameter.

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Line 348. The Shi et al. 2007 reference is a study on the tropical N₂-fixing cyanobacterium *Trichodesmium*, and is not appropriate here. Please add a study on Southern Ocean phytoplankton, or at least on eukaryotic phytoplankton in general.

Changed the reference to Raven 1990, Twining & Baines 2013, Quigg et al. 2003, Strezpek and Harrison 2004.

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