Biogeosciences Discuss., 7, C2900–C2902, 2010
www.biogeosciences-discuss.net/7/C2900/2010/

Biogeosciences Discussions

Interactive comment on "The influence of iron and light on net community production in the Subantarctic and Polar Frontal Zones" *by* N. Cassar et al.

Anonymous Referee #3

Received and published: 13 September 2010

The influence of iron and light on net community production in the Subantarctic and Polar Frontal Zones by Cassar et al.

This manuscript brings together net community production, gross primary production, macro and micro nutrients, Fv/Fm, mixed layer depths from the Southern Ocean south of Australia to explain the influence of iron and light on primary production, particularly net community production. This is a large and valuable dataset that identifies enhanced production in the frontal zones and is sufficiently topical, sophisticated and important to warrant publication. I have no problem with the fact that a definitive reason for enhanced production at the frontal zones couldn't be identified and enjoyed the sub-

sequent discussion. Science is about looking for answers but not necessarily finding them. However, before acceptance the subsequent analysis and discussion need to be improved. The main conclusions themselves seem unclear, at times contradictory and not well supported by the data. In light of the above the authors need to be more equivocal in their conclusions

The manuscript is mostly well written and typo free.

However, I have a real problem with the PAM data in particular. Because of the relatively low biomass, the gain settings were between 22-26 (i.e Very high), indicating likely very noisy data. There is no indication in the methods or on the diagram (Fig 2F) that these measurements were replicated and yet this is essential to show variance. They are not mentioned at all in the results! If there is no replication, this data CANNOT be included in the manuscript. If this data exists it must be shown. The total Fv/Fm dataset shown in Fig 2F shows no relationship with NCP. By arbitrarily dividing into < 50 m and >50 m correlations can be made but based on very few points. The subsequent discussion is unfounded. Given the high gain settings required to get a measurement, the resulting noise, and the lack of replication this is a very tenuous relationship.

It is unclear whether all the data presented in this manuscript is new or already submitted elsewhere. For instance are the 14C gross primary production measurements here different from those in Westward et al, in review, from the same voyage? Similarly, are the Fe and Chl-a data new or published elsewhere as part of the Voyage volume (Deep Sea Research II ?)

I found the placement of data and discussion in "Supplementary Material' irritating and unnecessary. Most of the data placed there has a direct bearing on the discussion and should be reinserted into the text.

The major conclusion that NCP in the SAZ and PFZ is limited by iron and light is not well supported by the data. It is interesting that they comment that Fe and MLD and correlated and that measured dissolved iron may not reflect iron supply. They then rely

on the questionable Fv/Fm data to demonstrate iron stress and a relationship with iron. If, as they say, dissolved iron does not reflect iron availability and iron concentration is correlated with mixed layer depth then their argument that iron is controlling NCP is weak. The observations made are clearly valid but this section needs to be revised to express the uncertainties of these relationships and their somewhat speculative conclusions

Minor Points

I am surprised at their selection of a photosynthetic quotient of 1.4 when most other use 1.2. This has only a minor effect on subsequent interpretations but some justification is required.

Interactive comment on Biogeosciences Discuss., 7, 5649, 2010.