

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 #1

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Review of the paper “The influence of iron and light on net community production in the Subantarctic and Polar Frontal Zones,” by Cassar, DiFiore, Barnett, Bender, Bowie, Tilbrook, Petrou, Westwood, Wright, and Lefevre, submitted for publication to Biogeosciences.

This paper reports a study to try to assess two major controlling factors, iron supply and irradiance, of net community productivity (NCP) in “high nutrient low chlorophyll” region at the Southern Ocean, based on the extensive observation including continuous monitoring of dissolved O₂/Ar ratios, GPPs by triple oxygen isotope and radiocarbon approaches, observation of various forms of iron, and observation of quantum yield at

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photosystem II. These results provided by this study are undoubtedly important, however, I do not think the manuscript publishable to Biogeosciences, mainly because 1) discussion of the manuscript is obscure and not quantitative, 2) the manuscript seems not to reach substantial conclusions, and 3) structure of the manuscript is not organized well, with some other minor disadvantages.

In section 4.1 and Fig. 2E, a significant positive correlation between NCP and dissolved iron concentration is presented. Then, authors soon mention that dissolved iron concentration may not reflect straightforward to iron availability. In the same section, they also mention that it is difficult to distinguish the role of dissolved iron because it also correlates with mixed layer depth, which seems to imply that NCP may be also controlled by mixed layer depth, not only by dissolved iron concentration. Subsequently, authors show a significant positive correlation between NCP and the ratio of variable fluorescence to its maximum (i.e. quantum yield; F_v/F_m) at shallower mixed layer, based on the above mentioned working hypothesis that dissolved iron concentration is not a good indicator of iron availability.

Considering straightforwardly from these description, it is easily read that the relationship between NCP and dissolved iron concentration seems indirect or superficial, suggesting less important. However, this relationship seems to be come up as one of the most important conclusions and is also found in the abstract of this manuscript. This puzzled me at first.

Another important conclusion seems the relationship between NCP and F_v/F_m at shallower mixed layer (section 4.2.3 and Fig. 2D). It is, however, based on limited observation (n=8). This positive correlation seems to be derived mainly by maximum and minimum F_v/F_m values. After excluding these values, the plot would be mostly vertical (i.e. constant F_v/F_m). More observation must be required to strengthen this hypothesis. Moreover, F_v/F_m may also correlate with mixed layer depth, although authors do not mention it. More additionally, relationship between F_v/F_m and GPP is more straightforward than F_v/F_m and NCP, so that I cannot understand why authors do not

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test and mention it. I agree it is possible to say that Fv/Fm is an important controlling factor of NCP, however I disagree it can be one of the most important conclusion in this study, based on limited observation, incomplete statistical treatment and insubstantial discussion.

Additional important conclusion seems the relationship between NCP and irradiance. This conclusion is quite qualitative without any statistical treatment. It is very difficult to believe.

After all, I cannot feel any progress after reading this manuscript, except for numerous NCP observations. Thorough statistical treatments for every dataset and much more detail discussion may be fundamentally inevitable.

Specific comments

2.1 NCP from oxygen mass balance

P5654L7-9: If your assumption that Ar can be always regarded as in equilibrium (saturation) for NCP estimation is correct, degree of total and biological oxygen super-saturation found in Fig. S1 should be same, because $\Delta(O_2/Ar)$ would be identical to saturation degree of oxygen if $[Ar]/[Ar]_{sat} = 1$ (eq. 2). However, as you described in section 3.2, it can be found that there seems to be significant positive difference from “optode” O₂ super-saturation to that derived by EIMS (“biological”), up to more or less 5 %, suggesting that Ar super-saturation would be around the same degree. Moreover, the difference seems to be variable between zero and 5 % through the north-bound transect (Fig. S1). If my understanding is correct, your NCP may be always underestimated in principle. Additionally, I am not sure what is a fundamental advantage of O₂/Ar analysis relative to “classical” O₂ concentration analysis based on the assumption of Ar equilibration.

P5654L10-14: Once you mention the possibility that your assumption may be inappropriate in some cases, you have to make out that your case is appropriate to this

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assumption. Otherwise, it would be hard for readers to distinguish what is correct, what is incorrect, what part may be correct with some difficulties, and what is possible to say it's correct, and so on.

2.3 Ancillary measurements

L5656L17-18: The procedure for atmospheric Fe deposition should be described more in detail.

3 Results

The data of horizontal wind speed with information of observed height must be indicated. The data of Fe concentration should be presented, not only in cross-plot (Fig. 2C) also in time-series or geographical distribution.

3.1 Description of the region under study

I think this section should move to “Material and methods,” because, for example, some nutrient results found in this section is not mentioned in “Material and methods,” meaning that they are not your original results but referred ones.

P5657L5-9: I could not find the exact positions for P1, P2 and P3 in Fig. 1. Please see also a comment on Fig. 1.

P5657L10: STZ is not defined beforehand.

3.2 Description of O₂ saturation, biological O₂ saturation, and NCP.

P5658L3: “Optode” is not yet so common to all over the world. Moreover, you have to describe this method in “Material and methods” or “Supplementary online material.”

P5658L3-9: Again, I feel it curious why you assume $Ar/Ar_{sat} = 1$ for NCP estimation, although you described here the significant difference that implies significant Ar super-saturation.

3.3 Comparison of NCP to other productivity measurements

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A Table including each two kinds of NCPs and GPPs are highly recommendable for these comparisons.

P5659L10-11: "better" should be replaced to "lower".

P5659L11-13: More explanations about the difference between NCPs derived from O₂/Ar and bottle incubation are necessary with associated literature, such as slope of Fig 2A, reasons lowering bottle NCP, and so on.

P5659L13-15: I cannot understand why 14C-based GPP was used in this section, although 17Δ-based GPP was used in previous section. Also I am not sure what the difference between Fig. 3 and GPPs found in Fig. 5B is. If you would like to indicate both of GPPs in a same paper, the discussion about its consistency, difference and reasons of difference must be required.

P5659L16-17: This sentence is too general. Because you can determine which one is correct using your own NCP and GPP data, you should do this.

4 Discussion 5 Conclusion

Need fundamental revision

Figure 1: It is incomplete. Lack of longitudinal scale. Should plot P1, P2 and P3 here. I cannot read out without help of Fig. 1 in Bowie et al. (2009).

Figure 2: Tabulation from identical dataset found here may be more helpful to understand, instead of these figures. Should displace color bars off XY planes in Fig. 2C-F.

Supplementary Material Figure 1: This figure seems essentially helpful to understand, so that it should be included main manuscript, not as supplementary material. Should add south-bound transect. Should put wind and MLD here.

Supplementary Material Figure 2: I think it is not necessary.

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

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