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DUKE UNIVERSITY

DIVISION OF EARTH & OCEAN SCIENCES

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Dear Editor of Biogeosciences,

We would first like to thank you and the reviewers for careful examination of our manuscript and valuable comments. Attached is the revised manuscript, which takes into account comments from the reviewers and the requested formatting changes.

Below is a response to each of the Reviewers' comments. Please do not hesitate to contact me should you require more information. I do not plan to travel in the next several weeks, and will be reachable by email or phone.

Sincerely,

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We would first like to thank reviewers for their careful examination of our manuscript and their insightful comments. Our responses to the reviewers' comments are in blue.

Reviewer # 3

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 subsequent 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

We have now modified the conclusions to better acknowledge that our results should be interpreted with caution.

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.

F_v/F_m measurements were performed in triplicates. Following the reviewer's recommendation, we now include standard error bars in the figure. We also added a sentence in the manuscript to the effect that although a pattern is present, the relationship is based on fewer data points than optimal and therefore must be treated with caution. We agree with the reviewer that 50m is arbitrary. The main objective of the discussion on NCP vs. F_v/F_m is to explain why a simple linear relationship between the two properties is not expected. However, the depth at which the system switches from iron/light limitation to dominated by light limitation is dependent on various properties in addition to MLD.

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.

Following the reviewer's recommendation, we now 1) better acknowledge the source of the data presented, and 2) reinsert the figures from the supplementary online material into the manuscript. The figure describing iron deposition was kept in the supplementary material as it is based on Bowie et al. 2009, and does not depict any original material. The supplementary discussion was unnecessary and was removed.

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

We again agree with the reviewer. The strong correlation between Fe and NCP is at first exciting, but we judge that it is important to expose the limitations of such a correlation. We believe that our discussion on the inadequacy of the concentration of a limiting nutrient in predicting availability is an important contribution to the field, and which many previous studies have failed to recognize or acknowledge.

Following the reviewer's recommendation, we have modified the section to acknowledge the speculative nature of some of the 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.

We cite Laws 1991 who calculates that the photosynthetic quotient of new production is 1.4 ± 0.1.

Reviewer #2
This paper presents the results of a comprehensive study of net community production and iron availability in the ocean south of Tasmania across the subtropical, subantarctic and polar fronts in Jan/Feb of 2007. The key point is the intense maximum of NCP just south of the subtropical front and a discussion of the process that causes it. It is rare to

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have comprehensive results of NCP and dissolved iron measurements together, and I believe this study represents a true advance in the understanding of processes that limit productivity in this part of the world. This is important because the boundaries of the subtropical/subpolar fronts are regions of intense uptake of CO2 by the ocean.
The paper is pretty well written, and I definitely think it should be published. However, I think it could be improved. I would like to try to convince the authors to make some changes that I believe will make it more convincing and a little easier to read. I have two main points and then some less important ones.

(1) It reads as though it is not completely clear in the authors' minds what hypothesis they wish to forward for the enhanced NCP they observe at the front. The reason I say this is the discussion around line 10 of pg 5695, which I think is a critical discussion in the paper. First it is stated that, "Mixing of macronutrient poor/micronutrient rich subtropical waters with macronutrient rich/micronutrient poor subantarctic waters probably enhances primary production at the front." The next sentence states that this may not be what is causing the enhanced NCP in the same area. There are inconsistencies here from previous statements: First, it is stated on pg 5657 that nitrate and phosphate are high through the whole region, but Si is low everywhere. How can these statements support the horizontal nutrient gradient hypothesis, and why do we not get to see the latitudinal nutrient gradients? Second, on pg 5659 it is stated that there is no latitudinal trend in NCP/GPP. So, if GPP is higher because of the nutrient gradients then so is NCP higher because of the nutrient gradients. I think this most important section could be rethought and improved.

The enhanced export at certain fronts is now becoming more evident (e.g. Ribalet et al 2010, Howard et al 2010). We rephrased the sentence after "Mixing of macronutrient poor/micronutrient rich subtropical waters with macronutrient rich/micronutrient poor subantarctic waters probably enhances primary production at the front" to clarify our reasoning. Our argument is that although we believe the mixing is responsible for the enhanced NCP observations at the front, it is difficult with our current observations to evaluate whether this is true. As waters flow northward through Ekman transport, Si is preferentially drawndown (see for example Sarmiento et al 2004). The unusual Si drawdown relative to N is in fact characteristic of subantarctic mode waters. We present a brief summary of nutrient concentrations, and cite Bowie et al for further description of the nutrient distribution during the SAZ Sense cruise.

The GPP data (including NCP/GPP) is now the subject of another paper (effect of particle size distribution on export ratio). We have now limited our focus to the putative role of light and Fe availability and sufficiency in influencing NCP.

(2) My second major point has to do with hiding much of the critical information used for the conclusions in the supplementary material. I believe supplementary material is the place for details that are not critical to the main arguments. In this paper we have to go to the supplementary material to see that GPP is high at the subtropical front and that there

is no clear trend in the NCP/GPP ratio, which are critical to the arguments. Real O2/Ar data are not presented anywhere, nor are horizontal gradients of NO3, PO4 and H4SiO4. There seems to have been an emphasis on making the paper short at the expense of the presentation. I believe this would be a much better paper if critical information were presented in the main text.

Following the reviewer's recommendation, we now include most of the figures in the main text (except for the Fe deposition data). Because the main objective of the paper is to look at NCP in the region, we have now removed GPP information, which is now the focus of another paper (see above). Nutrient data is presented in Bowie et al. (in review in DSRII).

Other less significant but not trivial improvement comments follow:

(a) Pg. 5651 Introduction. The latitudinal extent of the SAZ and PFZ are never defined. It is important to provide this information because everyone that reads this will not know these terms and the value given for NCP in this zone depends entirely on its definition because of the strong horizontal gradients observed.

Following the reviewer's recommendation, the latitudinal extents of the SAZ and PFZ are presented in Fig.1.

(b) A similar comment comes up on pg. 5661 where processes stations P1, P2, and P3 are pulled out of the air. These stations should be described somewhere and put on Figure 1. Also, on Figure 1 it would be very helpful to have one more line of longitude and one more of latitude to give scale to the map for those less obsessed with the Antarctic.

The latitudinal positions of the stations are included in the main text. Following the reviewer's suggestion, Figure 1 now includes the position of P1, P2, and P3, and more longitudinal and latitudinal lines.

(c) pg. 5654, line 13. I do not think it is DOC production that might cause an error but rather DOC accumulation.

DOC production has been changed to DOC accumulation.

(d) pg. 5657, line 13. ": : : dominated by non-diatoms: : :" (?) What is a "non-diatom"? There must be a better way to say this.

We changed "non-diatoms", to "non-siliceous" phytoplankton groups, a term more commonly used in the literature.

(e) pg. 5661, I find the one paragraph discussion under 4.2.1 to be much too glib. I think it should be deleted or the discussion expanded so the reader knows why it is there.

We decided to include this paragraph based on a colleague's recommendation. We expanded the paragraph to clarify our discussion: "The ratio of pFe export to NCP at the base of the mixed layer is expected to be greater than 1.3 μmol mol-1 because of attenuation of the particulate iron flux between the mixed layer and 150 meters. For reference, Fe:C in laboratory cultures varies greatly, but is generally greater than 2.5 μmol mol-1 (Sunda and Huntsman 1995)."

(f) pg. 5664, line 16. Missing an, "in the"

"in the" was added.

pg. 5664, line 20. I think this sentence is confusing. Maybe a restatement of the point in different words would help?

We modified the sentence following the reviewer's recommendation.

Reviewer # 1

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; Fv/Fm) 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.

It is practically impossible to infer causation by studying a natural system. The correlation of NCP to iron concentration is strong, but we believe that correlation to a limited nutrient (in this study and previous studies) should be interpreted with caution. We think this is an important point we are raising. As mentioned in response to Reviewer 3, the strong correlation between Fe and NCP is at first exciting, but we judge that it is important to expose the limitations of such a correlation. We believe that our discussion on the inadequacy of the concentration of a limiting nutrient in predicting availability is an important contribution to the field, and which many studies have failed to recognize or acknowledge.

Our approach allows us find general patterns, but as pointed out by the reviewer, cannot

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lead to firm conclusions. We thought that this was made clear in our original paper, but we have now modified the manuscript to make it clearer. We have also added a word of caution in the abstract and in the conclusion about the interpretation of the correlation between NCP and dissolved iron concentration.

Another important conclusion seems the relationship between NCP and Fv/Fm 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 Fv/Fm values. After excluding these values, the plot would be mostly vertical (i.e. constant Fv/Fm). More observation must be required to strengthen this hypothesis. Moreover, Fv/Fm may also correlate with mixed layer depth, although authors do not mention it. More additionally, relationship between Fv/Fm and GPP is more straightforward than Fv/Fm and NCP, so that I cannot understand why authors do not 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.

Although we agree with the reviewer that the number of observations are limited, as far as we know, we are the first documenting such a relationship (based on 17 points, which is more than what is currently available in the literature). We think that our observations, as limited as they are, are the first reporting such a correlation, as both reviewers 2 and 3 point out. We therefore judge that our report, which for the first time links the biogeochemical carbon fluxes to the autotrophs physiological state, is a significant contribution because no such data was available in the literature. We hope that others will build on our findings.

F_v/F_m measurements were performed in triplicates. We now include standard error bars in the figure. We also added a sentence in the manuscript to the effect that although a pattern is present, the relationship is based on fewer data points than optimal and therefore must be treated with caution.

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.

See comments above, and comments by reviewers 2 and 3.

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 supersaturation found in Fig. S1 should be same, because _(O2/Ar) would be identical to

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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.

The reviewer’s interpretation of the method is wrong. In the equation presented in the manuscript, the factor which is neglected is the argon saturation, and we are taking into account the O2/Ar <u>super</u>saturation. Supersaturation is equal to (saturation-100). For example, if the argon saturation is 102%, our NCP estimates will be underestimated by 2%, well within the error bounds of NCP measurements. On the other hand, if we use a total oxygen <u>super</u>saturation of for example 4%, with a biological <u>super</u>saturation of 2%, the NCP would be overestimated by a factor of 100%. A more thorough explication of the method is given in the method papers cited in our manuscript.

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 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.

See comments above.

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

The atmospheric deposition measurements are the subject of another paper. The author of these measurements graciously shared his data. In the interest of space, we have decided not to include the method for atmospheric Fe deposition. The method is fully described in Wagener et al. 2008, and observations described in Bowie et al. 2009, which we cite in our manuscript.

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.

See comment above.

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.

We disagree with the reviewer, but this is ultimately the decision of the Editor as to whether geographical setting should be included in the Material and methods section.

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.

Following the reviewer's comment, we added the location of P1, P2 and P3 on Figure 1.

P5657L10: STZ is not defined beforehand.

STZ is now defined.

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."

Following the reviewer's recommendation, the optode measurements are now described in the Material and methods section, with reference to Reuer et al. 2007.

P5658L3-9: Again, I feel it curious why you assume Ar/Arsat = 1 for NCP estimation, although you described here the significant difference that implies significant Ar supersaturation.

See comments above regarding saturation vs supersaturation.

3.3 Comparison of NCP to other productivity measurements
A Table including each two kinds of NCPs and GPPs are highly recommendable for these comparisons.

We believe the current figure is more pertinent. However, data will be made available in the auxiliary material if requested by the Editor.

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

We changed the sentence to "O_2/Ar NCP observations are generally within a factor of two of the oxygen incubation derived NCP (Fig. 3A)."

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.

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We believe this is not the purpose of this figure. The main objective with this figure is to show that upwelling of O2 undersaturated waters is not biasing our in situ O2/Ar measurements.

P5659L13-15: I cannot understand why 14C-based GPP was used in this section, although 17_-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.

Following the reviewer's recommendation, we have removed the triple isotope GPP. The latter is now the subject of another paper in preparation.

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.

This sentence referred to a theoretical prediction of the relation between NCP and GPP. The sentence was modified for clarification.

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).

Following the reviewer's recommendation, Stations P1, P2 and P3 have now been added. Longitudinal and latitudinal lines have been added.

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.

We think the figure is much better for visualization. A table will be made available in the supplementary online material if required by the Editor.

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.

The figure was included in the main text. The purpose of the figure is to show the temporal variability. Adding the south-bound transect would not help our discussion. However, if recommended by the Editor, the data from the manuscript, including wind and MLD will be made available in the supplementary material.

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

Following the reviewer's suggestion, this figure has now been removed.

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