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## ***Interactive comment on “A geographical and seasonal comparison of nitrogen uptake by phytoplankton in the Southern Ocean” by R. Philibert et al.***

### **Anonymous Referee #1**

Received and published: 29 August 2014

This manuscript presents an important data set from the southern Atlantic Ocean, covering a large geographic area. The biogeochemistry of this region is understudied, especially during winter, and this paper could provide critical information on nitrogen uptake in the open and ice-covered pelagic ocean. Unfortunately, the presentation of data lacks clarity and focus, appears to misuse central terms (e.g. primary production!) and only vaguely define others. Large portions of the discussion are speculative arguments about controls on the measured rates, but without any direct evidence to back up the claims. The manuscript would be much improved by focusing on the parameters that were measured, and presenting a clearer story. For example, much of the introduction is about carbon cycling in the Southern Ocean, but all of the data is

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about nitrogen uptake. While those parameters are certainly related, the relationship is never made explicit, nor are the points made in the introduction referred back to in the discussion.

At this time, the manuscript cannot be recommended for publication. I suggest the authors carefully review their interpretation of results, and clarify many of the terms used throughout. There are additionally so many sentences that are vague or unrelated that it made reading the manuscript a challenge. Given the importance of the data set, there is still great utility in publication of the work, and I hope that this review does not deter the authors from making the significant revisions necessary to get it published.

Specific comments (broken down by section) The title suggests that the manuscript will be talking about the entire Southern Ocean. In fact, the stations, while covering a broad geographic range, more likely represent sub-polar waters of the southern Atlantic and Indian Oceans. This could be greatly clarified by including physical data that was almost certainly gathered on the cruise.

#### Abstract

The first sentence of the abstract is so obvious as to render it useless. Additionally, this paper is not about primary production; there is no carbon uptake data presented. The authors present data on “new” and “regenerated” production. This is a critical distinction and cannot be overstated.

Page 1830, Line 4: “region to region” is unclear. Between different ocean basins? What scale is being talked about?

Page 1830, Line 5: which “region” is being referred to here? The Southern Ocean overall?

Page 1830, Line 20: what is a nitrogen uptake “regime”?

#### Introduction

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The first two paragraphs of the introduction are interesting, but it's not clear how they relate to the overall paper. There is little to no further discussion of carbon sequestration, flux rates, or even the impact of f-ratios on determining a nitrogen (or carbon) mass balance of the system. The most important point here is the paucity of winter data for this region.

The authors first focus on the importance of the Southern Ocean to the global marine carbon cycle, but fail to elucidate that point, and instead focus on carbon cycling within the Southern Ocean. These are two separate ideas. Surprisingly, neither idea is connected to deep water formation. Nor to how it relates to nitrogen uptake.

Page 1831, lines 16-17: "global studies" is not the right object for modeling. Besides, this sentence is phrased awkwardly, as is the following sentence.

Page 1830, lines 26-28: this statement is vague and doesn't add to the point being made above. Methods

Page 1833, lines 16-19: it is not clear what parameters were measured, nor how, with a float/glider.

Page 1834, lines 4-6: are the SAZ, PFZ, and AZ different from the STF, SAF, and PF given in Figure 1? There are too many acronyms, and they don't align. For someone unfamiliar with this system, it would add clarity to simply write out the names of the zones and fronts, and have them clearly defined.

Page 1834, Line 18: how do you know it doesn't damage cells? Are there any empirical data attesting to that?

Page 1834, line 25: that is a huge range for enrichment. There should be some serious discussion on potential impacts of 160% increase in NH<sub>4</sub><sup>+</sup> concentrations. What are the final atom%? This is not addressed in section 3.2, but needs to be. When it is finally discussed (page 1852), data is alluded to, but not shown.

What size filters were used? This is not a trivial omission, as it will determine what part

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of the microbial community was sampled, and greatly influence the interpretation of PN results.

Page 1835, line 4: The Gandhi et al. (2012) paper simply references Dugdale and Wilkerson. The authors should cite the original source. Additionally, this is a different reference than Dugdale and Goering (the more traditional citation for N uptake) given above (page 1834, line 12). Which was used? If it's the Dugdale and Wilkerson paper, why is that one (specifically for eutrophic systems) being used in this system?

### \_Results\_

Page 1836, line 17-18: this statement belongs in the methods.

All of section 3.2 likely belongs in the discussion. Either way, specific uptake rates are defined wrong! There is no normalization to PN... that's why they are independent of biomass. For absolute uptake rates, the authors state "it can introduce errors if the PN contains any non-phytoplankton nitrogen". Did it? How much did that impact your rates?

NH<sub>4</sub><sup>+</sup> regeneration and nitrification are separate processes, resulting in isotopic dilution of different N pools. They cannot be lumped together. See papers from Glibert and Bronk starting in the mid-1980s. Or the nice summary chapter from the 2nd edition of Nitrogen in the Marine Environment (2008) by Bronk and Steinberg.

Mere presence of NH<sub>4</sub><sup>+</sup> does not necessarily indicate regeneration. Why have the authors not tried to account for isotopic dilution? While not ideal, they could use the equations of Kanda et al. (J. Plankton Res., 1987). Of course "these omissions do not affect the new production estimates", but they could have a major impact on regenerated production, especially considering the large enrichment discussed above.

The discussion of nitrification here is unsatisfying. If it wasn't measured, but it may be important, how specifically could that influence the data being reported here?

Specific uptake rates span five orders of magnitude. How do you explain that? Even if

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it can't be explained, it makes the utility of an average very close to nil. Discussion

Page 1843, lines 1-2: If there is previous data from Cota et al. and Nelson, why is it not included in Figure 6?

Section 4.2.2: the Q10 guideline is not a proven metric, and Smith and Harrison do not argue for it to be used as an absolute. This is especially true in polar regions where rates don't respond linearly to temperature changes. What about the so-called "Wiebe hypothesis", or the Kirchman et al. (2009) paper arguing against DOM controls on uptake at low temperatures? Do the authors believe these temperature influences are present for inorganic N sources?

Page 1846: when discussing nutrient limitation and alleviation of it, it would be extraordinarily helpful to include NO<sub>3</sub><sup>-</sup> concentrations measured.

Page 1846, line 11: what "other factors" specifically? This is maddeningly vague.

If iron measurements were not available, why dedicate so much space to discussing iron impacts? Focus on the data you do have.

Page 1849, line 13: is there a reference for enhanced iron dependence of ammonium uptake?

Page 1849, line 16: if nitrate is not limiting, why doesn't that account for increased uptake? Why invoke some other limiting nutrient?

Page 1850, lines 7-11: these statements are vague and inconclusive. What specifically did the other paper find? What is the specific connection to this work? The last sentence of this paragraph is not useful and should be eliminated.

Although there is an entire section devoted to silicic acid, there is only a single concentration given in the entire manuscript. More results are needed to evaluate this parameter. And this is only a useful discussion if there is some indication of phytoplankton community composition, as alluded to but not clarified. On page 1851 (line

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2), silicate (not silicic acid) profiles are mentioned, but not shown.

### Tables & Figures

Table 1 should likely be a supplemental table. It doesn't add anything substantive to the discussion.

Why do tables 2 and 3 have different headers? They are presenting the same data, but simply from different seasons.

How are the fronts defined in figure 1? Why are they simply straight drawn lines? Are they just guesses as to the location of these fronts? There should be data backing up the locations. And why is there no corollary information for the stations to the east?

In figure 2, there appears to be much more detail in the color gradients than is possible from the limited number of stations. How were these interpolated?

Figure 5 would likely be more informative as a table. Also, which of these correlations are statistically significant?

### Technical corrections

Listed below are the technical corrections I was willing to transcribe from my notes. They should not be considered comprehensive.

Page 1830, Line 1: this sentence is awkwardly phrased, making it difficult to read.

Page 1831, line 2: however is not necessary because the authors are not contradicting a point.

Page 1831, line 27: "by" should be changed to "to"

Page 1833, line 24: the GoodHope Line needs to be defined and added to the figure, or at least have Ansorge et al. (2005) cited. What is the track between the ice-shelf and Marion Island? The map only shows stations.

Page 1833, line 26: don't define new acronyms (XBT and uCTD) if they are never used

again.

Page 1834, line 4: SAZ was already defined above (page 1833, line 14).

Page 1834, line 12: This statement is in the wrong paragraph. Uptake rates are discussed later.

Page 1834, line 29: what is meant by “appropriate”?

Page 1836, line 24: the authors switch between naming CTDs and stations, which is confusing for the reader. Maybe it would be better to include the station name each time the CTD number is referred to.

Page 1838, line 10: Are station 6 in the text and station 15N-6 in figure 1 the same? Be consistent.

Page 1842, line 23: what does SANAE stand for? Define and give credit.

Page 1844, line 21: if there are two, it should be “analyses”.

Page 1845, lines 26-28: Using “furthermore” to start two consecutive sentences is one sentence too many.

Page 1849, lines 27-28: strange double-negative (“no such difference” and “not limiting”) make this sentence difficult to parse.

Page 1852, line 9: don’t use a non-standard term for atom% enrichment.

The tables are out of order from the text. Table 3 is mentioned first (page 1838), then table 2 (page 1839), and then table 1 (page 1843)

Table 1: rho needs to be defined in the table blurb. Significant figures are inconsistent. Capitalization of seasons is inconsistent. “SOSCEX” and “Winter” are not references. It’s not appropriate to cite unpublished data in a table like this. What depth were the studies integrated over? Why bother noting the season if the actual month is given? And why is December summer for one study but spring for another?

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Table 2: all of the terms need to be defined. Are the concentrations in  $\mu\text{mol N L}^{-1}$  or  $\mu\text{mol NH}_4^+ \text{ L}^{-1}$ ? This is an important distinction because the rates are defined as  $\text{N L}^{-1}$ . The second mention of  $\text{NH}_4^+$  lacks [ ]. The depth integrated rates should be  $\text{m}^{-2}$ . New production and total primary production are mentioned but not shown (nor are they defined).

Table 3: All of the same mistakes from Table 2 are repeated, with the additional mention of f-ratio, which is not actually in the table. Also, they are  $^{15}\text{N}$  uptake rates. “Below dl” needs to be defined both as a definition (tell the reader this means below detection limit) and what that limit is ( $0.3 \mu\text{M}$ ). Is the depth over which the rates are integrated ever defined?

Figure 1 needs an inset map giving a sense of the greater area. STF, SAF, and PF need to be defined.

What are the contour lines in Figure 2?

Figure 3, panels c, d, e, and f: are the numbers in the legend CTD numbers?

In figure 4, why can't the axis titles have subscripts? Or at least closely line up with how these parameters are defined in the text?

Most of the data (>95%) in figure 6 is less than  $\sim 7$  along the x-axis. Make a break or leave off the outliers so the most salient information can be seen more clearly.

Are there plot points within the legend of figure 7?

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Interactive comment on Ocean Sci. Discuss., 11, 1829, 2014.

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