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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.

**R. Philibert et al.**

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**Reviewer comment**: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 produc- tion!) 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



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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 about nitrogen uptake. While those parameters are certainly related, the relationshipis 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 manuscriptwill 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.

**Response**: We would like to thank the reviewer for their constructive criticism of this paper and appreciate the encouragement to make the revisions to the manuscript. The links between the carbon and nitrogen have been made clearer. Some of the more speculative parts of the manuscript have been removed but others kept (as will be discussed below). Some of the physical data gathered during the cruise has now been added as supplementary material and is referred to in the revised manuscript. In addition, brief descriptions of the cruise tracks have been added both to the abstract and introduction.

**Reviewer comment**: _Abstract_ The first sentence of the abstract is so obvious as to



OSD
11, C906–C927, 2014

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. **Reviewer comment**: Page 1830, Line 4: “region to region” is unclear. Between different ocean basins? What scale is being talked about? **Reviewer comment**: Page 1830, Line 5: which “region” is being referred to here? The Southern Ocean overall?

**Response**: The first two sentences have been modified for more clarity. The carbon uptake data has not been presented but can be estimated from the nitrate and ammonium uptake rates. We hope that the changes made clarify this important distinction.

**Changes to the manuscript**: The impact of light and nutrients (such as silicate and iron) availability on nitrogen uptake and primary production vary seasonally and regionally in the Southern Ocean. The seasonal cycle of nitrogen uptake by phytoplankton in the Southern Ocean is not fully resolved over an annual scale due to the lack of winter in situ measurements. In this study, nitrate and ammonium uptake rates were measured using $^{15}$N tracers during a winter cruise in July 2012 and a summer cruise in February/March 2013. The winter cruise consisted on two legs: Leg 1 extended from Cape Town to the ice margin along the GoodHope Line and Leg 2 stretched from the ice margin to Marion Island. The summer cruise was mostly focused on the subantarctic zone of the Atlantic sector.

**Reviewer comment**:Page 1830, Line 20: what is a nitrogen uptake “regime”?
**Response**: We used ”regime“ to mean the combination of nitrogen uptake rates found at each location i.e the rates themselves and whether there was a preference for ammonium or nitrate. This has been changed to "nitrogen uptake" only.

**Reviewer comment**:



OSD
11, C906–C927, 2014

_Introduction_
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 ma- rine 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.

**Response**: The first two paragraphs have been modified to emphasise the role of the Southern Ocean in the marine carbon cycle. The statement that the Southern Ocean accounts for "about 4% of global carbon fluxes" is placed immediately has been moved. Additional discussions about the links between the nitrogen and carbon cycles have been added.

**Changes to the manuscript:** "In the Southern Ocean, low temperature, low light, strong vertical mixing and iron limitation restrict the uptake of nitrogen and ultimately phytoplankton growth. The concentrations of iron in the Southern Ocean are low due to the lack of terrestrial inputs. The role of these low iron concentrations in limiting nitrogen uptake is well-established (Martin et al., 1990; Moore et al., 2007; Falkowski et al., 1998; Cochlan, 2008; Boyd et al., 2010; Boyd, 2002). Furthermore, phytoplankton in a strongly mixed environment, such as the Southern Ocean, are not exposed to light sufficiently long for efficient nutrient uptake and growth. (Mitchell et al., 1991; Venables and Moore, 2010). This is compounded by the low incident light . The combination of these “bottom-up” controls and “top-down” controls such as grazing (Behrenfeld, 2010) results in the High Nutrients Low chlorophyll (HNLC) conditions for which the Southern



OSD
11, C906–C927, 2014

Ocean is well-known. Despite this, the Southern Ocean plays an important role in the global marine carbon cycle. Carbon fluxes in this region accounts for about 4 % of global carbon fluxes (Takahashi et al., 2009) and $300\,\mathrm{Tg\,C\,y^{-1}}$ of export production (Henson et al., 2011; Gruber et al., 2009). This is achieved through a combination of the solubility pump and the biological pump. The solubility pump encompasses the physical processes, such as mixing of surface water masses to the deeper layer,which remove carbon dioxide from the surface. The biological pump is driven by the sinking and subsequent sequestration of organic matter produced by phytoplankton through photosynthesis. In this process, phytoplankton convert inorganic nutrients (carbon, nitrogen and others) into organic matter (Volk and Hoffert, 1985). Given the cellular demands (Hedges et al., 2002), the elements are generally assimilated in a fixed ratio, which was first observed by Redfield (1934). This ratio is very useful in linking the various biogeochemical cycles. For instance, by knowing the rates at which nitrogen is assimilated, carbon assimilation rates can also be estimated. Only part of the organic carbon formed by phytoplankton is exported and sequestered below the permanent thermocline (Falkowski et al., 2003). This fraction can be estimated by distinguishing the sources of inorganic nitrogen nutrients (Dugdale and Goering, 1967; Eppley and Peterson, 1979). Nitrate was considered to be a "new" nutrient which is only formed outside of the euphotic zone and brought to the surface through physical processes. Ammonium and urea were considered to be "regenerated" nutrients formed only within the euphotic zone. Nitrate being used by phytoplankton has to be in balance with the rate of organic nitrogen export for the phytoplankton to maintain itself. Therefore, the fraction of primary production fuelled by nitrate over total primary production would correspond to the amount of exported organic matter. This rests on the assumption that nitrification, the oxidation of ammonium to nitrate, is completely inhibited by light and does not occur within the euphotic zone (Dugdale and Goering, 1967; Joubert et al., 2011). This is, however, a false assumption as nitrification as well as nitrifying organisms have been observed in the euphotic zone in various regions (Yool et al., 2007).“



OSD

11, C906–C927, 2014



**Reviewer comment:**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.
**Response**: These two sentences have been rephrased.
Changes in the manuscript: ”Researchers are turning to remote sensing and modelling in order overcome the logistical constraints of ship-based measurements (e.g poor spatial and temporal resolution) (Henson et al., 2011; Vichi et al., 2007) but these two approaches have their limitations. Remote sensing data needs to be calibrated against observational data and in the Southern Ocean, satellite observations for the winter season can be limited by the sun angle as well as cloud cover (Vernet et al., 2012).

**Reviewer comment**:Page 1830, lines 26-28: this statement is vague and doesn’t add to the point being made above.
**Response**: The statement has been removed.

**Reviewer comment**: _Methods_
Page 1833, lines 16-19: it is not clear what parameters were measured, nor how, with a float/glider.
**Response**: The variables measured by the float and glider included conductivity, temperature, pressure and photosynthetically available radiation using built-in sensors. This has been added to the text.

**Changes to the manuscript**: "The aim of the process stations was to sample the same parcel of water repeatedly. As such, each process station consisted of several Conductivity, Temperature, Depth (CTD) stations where nitrate uptake was measured. Process Station A was initialised by deploying a float on 25 February 2013 at 4239′ S 841′ E. However, the float was deployed incorrectly and a new float had to be deployed



OSD
11, C906–C927, 2014

two days later when this station was next occupied. This langrangian float measured conductivity, temperature, pressure and photosynthetically available radiation using built-in sensors. Process Station B did not have a float but was sampled continuously by a glider. The latter measured the same variables as the float as well fluorescence and two wavelengths ($\lambda$=470 and 700) of optical backscattering. Each dive cycle took approximately 5 hours to complete and covered an average horizontal distance of 2.8 km, rendering a temporal resolution of 2.5 hours and spatial resolution of 1.4 km between profiles."

**Reviewer comment**: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 thezones and fronts, and have them clearly defined.
**Response**: The names have been written in full. The definition of the zones is given e.g "the Subtropical zone (defined as the region north of the Subtropical front)"

**Reviewer comment**: Page 1834, Line 18: how do you know it doesn't damage cells? Are there any empirical data attesting to that?
**Response**: The mono pump is the recommended pump for research vessels and is purported to minimise damage. However, we have not found empirical data confirming this.
**Changes to the manuscript**: "Underway samples were collected from 5 m using a mono-pump. This type of pump recommended for supplying major research vessels with uncontaminated seawater supply as it minimises the damage to phytoplankton cells. "

**Reviewer comment**: Page 1834, line 25: that is a huge range for enrichment. There should be some serious discussion on potential impacts of 160% increase in NH4+



OSD
11, C906–C927, 2014

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.
**Response**: The discussion about the impacts of the tracers on ammonium uptake has been moved to section 3.2 The plot of enrichment vs ammonium uptake is attached nd will be provided aas supplementary material rather than in the revised manuscript for conciseness.

What size filters were used? This is not a trivial omission, as it will determine what part of the microbial community was sampled, and greatly influence the interpretation of PN results.
**Response**: Samples were filtered onto Whatman GFF filters. This has been added to the methods

**Reviewer comment**: 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?

**Response**: The paper by Gandhi et al. (2011) made the equations very clear, which is why they were initially cited. The major difference between Dudgdale and Goering (1967) and Dugdale and Wilkerson (1986) is that the latter uses particulate organic nitrogen to calculate the uptake rates whereas the former uses total particulate nitrogen. In this study, total PN was used and the citation has been changed. The equations are also shown as recommended by the reviewer number 2.



OSD
11, C906–C927, 2014

**Reviewer comment**: _Results_ Page 1836, line 17-18: this statement belongs in the methods.
**Response**: The statement has been removed as there is a similar description in the method.

**Reviewer comment**: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?

**Response**: This has been rephrased as " Specific nitrogen uptake rates ($\nu$) allow for comparison of uptake and growth rates independent of biomass whereas the absolute uptake rates represent the uptake in relation to the particulate nitrogen." The statement "it can introduce errors if the PN contains any non-phytoplankton nitrogen" has been removed. Comparing the concentrations of total PN vs POC (as measured after fuming filtered samples with ..) did not provide conclusive results. Furthermore, Dudgdale and Goering (1967) argue that this is accounted for within the calculation as the specific uptake represents uptake by the organic fraction of the organic matter.

**Reviewer comment**: NH4+ 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.
**Response**: Indeed, NH4+ regeneration and nitrification are two separate processes. The mention of nitrification from this specific paragraph has been removed.



OSD
11, C906–C927, 2014





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**Reviewer comment**:Mere presence of NH4+ does not necessarily indicate regeneration. Why have the authors not tried to account for isotopic dilution? While not ideal, they could use theequations 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 regener-ated production, especially considering the large enrichment discussed above. **Response**: Indeed, they can have a major effect on regenerated production. Using the corrections by Kanda et al. (1987) does not make a significant difference. They estimate that the values of a to be between 0.5 and 2 in the open ocean. Figure 1 in this comment shows the corrected rates vs the uncorrected rates.

**Reviewer comment**: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? **Response**: This has been added to the text.

**Changes to manuscript**: " The use of new production as a proxy for carbon export is based on the assumption that all the nitrate present in the euphotic zone is formed below the permanent thermocline and brought to the surface through upwelling and vertical mixing (Dugdale and Goering, 1967). It is therefore considered a "new" nutrient as opposed to a "regenerated" nutrient like ammonium or urea, which are produced within the euphotic zone. In the case of significant euphotic nitrification, part of the nitrate used by phytoplankton is likely to be part of the "regenerated" nutrient pool. Nitrification can also lead to the isotopic dilution of the nitrate pool and a resulting underestimate of nitrate uptake.".

**Reviewer comment**:Specific uptake rates span five orders of magnitude. How do you explain that? Even if it can't be explained, it makes the utility of an average very close to nil.
**Response**: As discussed further down, the wide range in winter nitrate uptake was due to an outlier at station 15N-6 and this does skew the averages. The range of



OSD
11, C906–C927, 2014

ammonium uptake rates is also very wide and the only explanation for this would be the wide geographical range.

**Reviewer comment**:Page 1843, lines 1-2: If there is previous data from Cota et al. and Nelson, why is it not included in Figure 6?
**Response**: The locations of the stations in the paper by Cota et al were not clear and it was therefore not possible to accurately match the uptake rates to the latitude. In Nelson and Smith (1990), integrated uptake rates were provided for each specific location.

**Reviewer comment**: 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? **Response**: The first two sentences have been changed to "Temperature is expected to have the same effect on nitrogen uptake by phytoplankton as on photosynthesis. In some cases, the growth rate is halved for every drop of 10 C (Smith Jr. and Harrison, 1991; Tilzer and Dubinsky, 1987)". We hope that this makes it clearer that Q10 is not an absolute. Kirchman (2009) focused on bacterial growth and argued that only 20% of the variations could be attributed to low temperatures. We would similarly argue that temperature was not the main control for nitrogen uptake by phytoplankton.

**Reviewer comment**: Page 1846: when discussing nutrient limitation and alleviation of it, it would be extraordinarily helpful to include NO3- concentrations measured.



OSD
11, C906–C927, 2014

**Response**: Nitrate concentrations have been added in the discussion.

**Reviewer comment**: 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. **Response**: This has been rephrased as " Other factors, such as light and nutrient availability,"

**Reviewer comment**: Page 1849, line 13: is there a reference for enhanced iron dependence of ammonium uptake?
**Response:** As we did not have any data relating to the iron concentrations, this has been removed. It also seems that this sentence was not clear. We meant that nitrate uptake has a stronger dependence on iron than ammonium uptake **?**.

**Reviewer comment**:Page 1849, line 16: if nitrate is not limiting, why doesn’t that account for increased uptake? Why invoke some other limiting nutrient?
**Response** : This statement has been removed.

**Reviewer comment**: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.

**Response**: This paragraph has been modified. The point here was to highlight that in winter, nitrogen uptake does not seem to be affected by iron but that light limitation might be playing an important role. We hope that the small modifications to this paragraph makes this clearer.



OSD
11, C906–C927, 2014





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**Changes to the manuscript**: " Furthermore, during this season, an iron-light colimitation, in which light might play a more important role than iron, is plausible (Moore et al., 2007; Van Oijen et al., 2004; Strzepek et al., 2012). In autumn, when mixed layers have started deepening and nutrient supply increased, Van Oijen et al. (2004) observed no changes in uptake rates after iron additions. This could either be due to the fact that in autumn, the ambient iron concentrations were sufficient to sustain the phytoplankton community. It could also be linked to interactions between light limitations and iron suppy. For instance, Strzepek et al. (2012) found in laboratory experiments that the effects of iron addition on phytoplankton growth were less pronounced under light limitation."

**Reviewer comment**: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 2), silicate (not silicic acid) profiles are mentioned, but not shown.

**Response**: We agree with the reviewer that the discussion on silicic acid is only useful if data on the phytoplankton commnunity structure were available. However, as this is not the case, linking the Si and nitrogen cycle properly is not possible. This section has therefore been removed.

**Reviewer comment**: _Tables Figures_ **Reviewer comment**:Table 1 should likely be a supplemental table. It doesn't add anything substantive to the discussion. **Response**: This table has been moved to the supplementary materials.

**Reviewer comment**:Why do tables 2 and 3 have different headers? They are



OSD
11, C906–C927, 2014

presenting the same data, but simply from different seasons.
**Response**: This has been corrected.

**Reviewer comment**: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?

**Response**: The temperature and salinity profiles obtained from the CTDs, XBT and uCTD were used to establish the position of the fronts on Leg 1 of the winter cruise (west side). These data indicated where the fronts were crossed on this particular transect. However, for the eastern stations, only CTDs were taken and the spatial resolution was not fine enough to establish the position of the fronts.

**Reviewer comment**: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?
**Response**: These were interpolated using a built-in function in Matlab. It is indeed important to take such plots with caution and recognise that while they show the change from one station to the next, what happens in between is not necessarily a reflection of reality.

**Reviewer comment**: Figure 5 would likely be more informative as a table. Also, which of these correlations are statistically significant?
**Response**: Figure 5 has been changed to a table with associated p-values. A comment has been made on the fact that these p-values have not been adjusted for the multiple comparison.



OSD
11, C906–C927, 2014





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

**Reviewer comment**:Page 1830, Line 1: this sentence is awkwardly phrased, making it difficult to read.
**Response**: has been changed

**Reviewer comment**: Page 1831, line 2: however is not necessary because the authors are not contradicting a point.
**Response**: However has been removed
Page 1831, line 27: “by” should be changed to “to”
**Response**: Has been changed
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.
**Response**:The tracks and the citation added.

**Reviewer comment**: Page 1833, line 26: don’t define new acronyms (XBT and uCTD) if they are never used again. **Response**: Have been removed
**Reviewer comment**: Page 1834, line 4: SAZ was already defined above (page 1833, line 14). **Response**: This has been corrected

**Reviewer comment**: Page 1834, line 12: This statement is in the wrong paragraph. Uptake rates are discussed later.
**Response**: This has been changed to say “Water samples for the measurement of $NO_3^-$ and $NH_4^+$ uptake rates were collected ....”



OSD
11, C906–C927, 2014

**Reviewer comment**: Page 1834, line 29: what is meant by “appropriate”?
**Response**: this has been corrected to "simulated in situ light depths corresponding to the sampling depth (55, 30, 10 and 3 %)"

**Reviewer comment**: 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.

**Response**: For the summer cruise, each process station consisted of several stations. In order to clarify the distinction between the “CTD stations” and the “Proces station”, the following sentence has been added to the description of the cruise track and stations: “The aim of the process stations was to sample the same parcel of water repeatedly. As such, each process station consisted of several Conductivity, Temperature, Depth (CTD) stations where nitrate uptake was measured.” For the winter, the use of the “renamed” 15-N stations is prefered to the cruise CTD number in order to avoid confusion with the summer CTD numbers.

**Reviewer comment**: Page 1838, line 10: Are station 6 in the text and station 15N-6 in figure 1 the same? Be consistent.
**Response**: This has been changed as they were refering to the same station.

**Reviewer comment**: Page 1842, line 23: what does SANAE stand for? Define and give credit.
**Response**: SANAE stands for South African National Antarctic expedition. The data is available from the Greenseas database (www.greenport.nersc.no) and this database has been cited.



OSD
11, C906–C927, 2014





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**Changes in manuscript:** The historical data includes data along the GoodHope Line (Joubert et al., 2011), the Indian sector (Thomalla et al., 2011) and data collected during three South African National Antarctic Expedition (SANAE) cruises. SANAE cruises are yearly cruises which take place between December and February in the Atlantic sector of the Southern Ocean. The data and cruise reports for the SANAE cruises are available from the Greenseas database (www.greenport.nersc.no).

**Reviewer comment**: Page 1844, line 21: if there are two, it should be “analyses”. **Response**: This has been changed.

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

**Reviewer comment**: Page 1849, lines 27-28: strange double-negative (“no such ” and “not limit-ing”) make this sentence difficult to parse.

**Response**: Sentence has been modified
**Changes to the manuscript**: "No such difference was observed. This supports the contention that iron supply was replete at this time of year." **Reviewer comment**: Page 1852, line 9: don’t use a non-standard term for atom% enrichment.
**Response**: This has been changed to use 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)
**Response**: The tables have been reordered.

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



OSD
11, C906–C927, 2014

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?

**Response**: This has been changed to provide only the months. "SOSCEX" and "Winter" which are the data from this study have been mentioned as such. Unpublished data has been removed. Furthermore, as suggested by the reviewer, this table is now part of the supplementary material.

**Changes in manuscript**: The table blurb now reads " Overview and comparison of integrated nitrate and ammonium uptake rates, $\rho_{NO_3}$ and $\rho_{NH_4}$ ($mmol\,N\,m^{-2}\,d^{-1}$) respectively. Average rates and the range are presented here"

Table 2: all of the terms need to be defined. Are the concentrations in umol N L-1 or umol NH4+ L-1? This is an important distinction because the rates are defined as N L-1. The second mention of NH4+ lacks [ ]. The depth integrated rates should be m-2. New production and total primary production are mentioned but not shown (nor are they defined).

**Response**:The mentions of new production and total primary production have been removed. Units have been corrected.

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 15N 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 $\mu$M?). Is the depth over which the rates are integrated ever defined?



OSD
11, C906–C927, 2014

**Response**: The integration depth is defined as the MLD in the methods section.

Figure 1 needs an inset map giving a sense of the greater area. STF, SAF, and PF need to be defined. **Response**: An instet showing a bit more of Africa (South Africa and Southern part of Madagascar) as well as part of Antarctica has been hadded. STF,SAF and PF have been defined. They show the Subtropical front, Subantarctic front and the Polar front.

What are the contour lines in Figure 2?
**Response**: The contour lines represent the temperature contours. This is now mentioned in the manuscript.

Figure 3, panels c, d, e, and f: are the numbers in the legend CTD numbers?
**Response**: The numbers are the CTD numbers. This is now be mentioned in the caption.

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?
**Response**: The parameters have been redefined as $\rho$NO3$_1$, $\rho$NO3$_{55}$, $\rho$NH4$_1$ and $\rho$NH4$_{55}$ instead of NO3_1, NO3_50 NH4_1 and NH4_50.

Most of the data (>95%) in figure 6 is less than âĹij7 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?
**Response**: The uptake rates in figures 6 and 7 have been replotted as log (nitrogen uptake) to make the differences more visible. The legend positions have also been adjusted.



OSD
11, C906–C927, 2014

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