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Biogeosciences Discussions

Interactive comment on "Biological productivity regime and associated N cycling in the vicinity of Kerguelen Island area, Southern Ocean" *by* A. J. Cavagna et al.

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Please find attached in pdf a friendly to read version of the responses to reviewers Thanks

Reviewer 2

The authors present a very nice data set that describes out two major findings: 1) natural iron fertilization in the Southern Ocean enhances primary productivity, C and N assimilation rates, and phytoplankton growth rates to an even greater degree than iron fertilization experiments, and yet organic C export is hardly enhanced, and 2) nitrate assimilation is the dominant N cycle process occurring in the sunlit upper layer (the

euphotic zone), with nitrification dominating the waters just below the euphotic zone but still within the mixed layer at most sites. This study includes a large amount of robust data from an important region of the ocean, with the profiles of nitrate assimilation and nitrification standing out as particularly impressive. I feel, however, that the manuscript requires some major revisions, or at least significant clarifications, before it is suitable for publication.

Major concerns: As the authors point out, there are important implications for the carbon cycle of nitrification overlapping with nitrate assimilation in the surface ocean, particularly with respect to inferring rates of new and export production, and understanding the ocean's biological pump. However, the distinction between the occurrence of these processes in the euphotic zone (i.e., where there is light for photosynthesis) versus in the mixed layer is very important in this regard – if the mixed layer is deeper than the euphotic zone, but export production is taken as the flux of organic matter out of the euphotic zone (or flux of new nutrients into the euphotic zone), as is typically the case, then the occurrence of nitrification within the mixed layer but below the euphotic zone is not necessarily problematic for estimates of export. Indeed, as the authors themselves point out, the highest rates of nitrification in most regions of the ocean are typically found at the base (or just below the base) of the euphotic zone, so their findings are not really surprising. I feel that the authors need to revisit the discussion of their nitrification versus nitrate assimilation rates in the different regions of the surface ocean, and clarify the implications for export production from the euphotic zone versus from the mixed layer – the two regions are not interchangeable with respect to export production. Given the dataset that the authors have, they can probably start to quantify the potential impact of nitrification on nitrate assimilation. Also, by definition, the waters of the mixed layer are easily mixed, which perhaps implies that nitrate produced by nitrification (i.e., regenerated nitrate) in the mixed layer below the euphotic zone is easily supplied to euphotic zone waters above, complicating estimates of new production. However, if this possibility part of the authors' argument, I could not find any discussion of it in the manuscript. It is misleading to suggest that the mere occurrence of nitrification in the mixed layer brings estimates of export production into question – if the authors want to make such a claim, it needs to be more robustly and clearly laid out in the manuscript.

REPLY: We would first like to indicate that the estimation of export has been done at 100 and 150m (below the MLD and not Zeu as assumed by the reviewer). We agree with the reviewer that in most oceans Zeu is deeper than MLD (e.g., oligotrophic oceans). In this case, nitrification below Zeu will have no impact on the assessment of new primary production (nitrate is distantly produced). The particularity of our study is that Zeu is shallower than MLD. Therefore, nitrification produces nitrate which is directly available for primary production (as water is well mixed in the mixed layer). We tried to clarify this point in the updated section 4.2, highlighting also the difference with the ocean in general.

Related to my concern above is the treatment in the manuscript of the f-ratio. The authors calculate the f-ratio very simply as nitrate uptake/(ammonium+nitrate uptake) according to Dugdale and Goering, 1967. However, this very clearly ignores the role of regenerated nitrate (produced by nitrification), which would serve to overestimate the f-ratio, and which the authors themselves claim is an important source of nitrate to surface waters. I cannot understand, therefore, why they use this simple definition of the f-ratio, and ascribe meaning to (and indeed interpret) the values that they calculate. The contribution of regenerated nitrate is going to be different at different depths, at different stations, and at different times of year, so I don't think that the f-ratio in this case is even useful as a relative measure. I suggest the authors either remove this entirely, or find a way to use their nitrification rate data to correct for regenerated nitrate production. Moreover, the caveats associated with the f-ratio calculation need to be clearly laid out and discussed.

REPLY: We now discuss about the uncorrected and corrected f-ratio presented also in Table 1. However, we would like to point out that the f-ratio is not biased by nitrification. It still indicates how much of the primary production is sustained by nitrate. It is only

when we discuss about new production (i.e. primary production x f-ratio) that any ML nitrification introduces a bias (since part of the nitrate is regenerated instead of being a new nutrient advected from the deep). This is now discussed in the section 4.2 (line 440 to 445).

Minor concerns: I suggest that the manuscript be edited for English - there are a number of grammatical errors and redundancies that can lead to a lack of clarity. => the revised version has been carefully checked for grammar and language quality p.2, l.36: I suggest "naturally fertilized" => done p.2, l.37: this is all referring to the fertilized site, right? Please clarify. => done p.2, l.38: see my comments above about the f-ratio => see previous comments p.2, l.40: see my comments above, but I am not convinced that these high rates are unexpected. Moreover it should be stated here that the high rates are typically below the euphotic zone. => done p.5, l.127-129: How do the authors know the original nitrate and ammonium concentrations in order to add the appropriate spike? Was it done as stated in line 138-139? If so, there should be some reference to this earlier. In addition, what is the sensitivity of the continuous flow approach? => done p.5, l.130: How much did the temperature vary?

REPLY: Sea surface temperature (upper 200m) was in the range of 1.5 to 3.5°C (see also potential temperature / salinity diagram available in Jacquet et al. 2015).

p.7, l.180-182 (actually l.186-188): Please clarify the meaning of this sentence: "The modelled calculated nitrification rates were screened for consistency with observed evolutions of nitrate concentrations over the duration of the incubation experiment and with measured nitrate uptake rates."

REPLY: here we explain that we compare measured variations of nitrate concentration through incubation experiment (ΔNO3 measured = NO3 ti – NO3 tf) with calculated variation of nitrate concentration through incubation experiment (ΔNO3 calc = NO3 uptake – NO3 nitrification) to test the quality of our nitrification rates dataset.

p.7, l.185-186: It is unclear why the authors used the model at all if they simply threw out any modeled rates that were incompatible with their data – I think perhaps I'm misunderstanding this sentence, but it needs to clarified: "When the rates given by the model were incompatible with concentration evolution, it was considered as an outlier and discarded from the dataset."

REPLY: what we express here is a method to avoid working with data outliers. Since we were aware that our dataset was sensitive in terms of robustness, we made the decision to submit it to a comparison with modeled data. And then, when the mismatch was too important (beyond analytical precision) the concerned analytical data was eliminated from our dataset and considered an outlier (see previous comment)

p.7, l.190: define 2sd upon first use. => done p.7, l.186-193: Given the methodological constraints described here, how confident are the authors in their nitrification rate data? REPLY: We are confident because most of the variations are higher than 2 SD. p.7, l.198: How did the authors determine the depth of the mixed layer?

REPLY: Density criterion – MLD calculated with the criteria: depth of MLD = depth where the potential density = potential density at 10m + 0.02 kg m-3 (Boyer Montegut et al., 2004 JGR). We now cite the reference in the revised version (line 218-219).

p.7, l.206: "across all the study area" – what does this mean? Please clarify. => This means over the entire study site. This has been changed in the ms. p.7, l.196-208: What about the reference (HNLC) station?

REPLY: This station is south of the Polar Front and, therefore, included in the station south of the Polar Front.

p.8, l.226-230: It's difficult for the reader to remember what depths these PAR levels refer to. I realize they are different for the different stations, but perhaps the authors can find a way to clarify, for example including PAR indicators on Fig. 3.

REPLY: In figure 3, the euphotic layer is highlighted by a dashed line, it is the 1% PAR level as classically defined (it has been specified in the legend of figure 3)

p.8, l.232: This seems to be true for nitrate, but I'm not sure it's always true for ammonium. I suggest separating discussion of nitrate and ammonium here.

REPLY: done

p.8, l.235-239: Please see my comments above regarding the f-ratio. I feel that using the standard Dugdale and Goering definition of the f-ratio here severely undermines the authors' argument about the potential importance of nitrification in surface waters.

REPLY: The f-ratio is giving the relative contribution nitrate to total N uptake, independently of whether there is nitrification or not. It is only when we are trying to assess new primary production (something that we didn't do in the submitted ms) that a bias from nitrification occurs (sensu Eppley and Petterson, 1979). We show both uncorrected and corrected f-ratios in the revised version (Table 1, discussed in art 4.2.)

p.9, l.240: Please see my comments above regarding the "unexpectedness" of nitrification below the euphotic zone in the mixed layer.

REPLY: the sentence has been modified in the revised version (lines 264 to 266) highlighting now the significance of nitrification rather than its unexpectedness

p.9, l.250-265: It's not clear what the reader is supposed to take away from this paragraph. It is largely a review of previous findings. While that is not necessarily inappropriate here, I encourage the authors to include a concluding sentence or two that communicates the point of this paragraph to the reader. The same goes, albeit to a lesser extent, to the following paragraph as well (l.266-289). Here, I feel that the main point is that the distinctions are driven by iron, and I suggest that the authors state this more clearly.

REPLY: The discussion in the revised version has been reworked to avoid redundancy

p.10, l.298-301: I don't understand the argument here, please clarify.

REPLY: This has been clarified in the revised ms (line 314 - 318). We intend to say that the observed large variations in growth rates were not due to variations in T°C. p.10, l.302-303: The authors know that light limitation must be occurring in some cases, they invoke light limitation as a way to explain the vertical distribution of nitrate assimilation versus nitrification, so I find this sentence too non-committal.

REPLY: We agree and remove this sentence from the manuscript. We decide not to talk about light-limitation given the small latitudinal range of the studied area, implying a relatively uniform input in term of solar radiation.

p.12, l.342-351: This is a great summary paragraph. I feel that authors could take even further the finding that C export is not enhanced by natural iron fertilization, which is very interesting (and important for our understanding of the Southern Ocean, and thus the global ocean, biological pump).

Reply: We agree with the reviewer but such finding is already discussed in Blain et al. (2007) and other manuscripts in the KEOPS2 special issues (e.g., Jacquet et al. and Planchon et al.). That's the reason why we keep this paragraph short.

p.13, l.367: What about the role of nitrification in overestimating the f-ratio? This seems far more pressing than the potential role of organic N assimilation by phytoplankton. Please see my comments above.

Reply: We do report both corrected and uncorrected f-ratios (see previous comment).

p.13, l.381: Please clarify the meaning of this sentence.

Reply: This sentence has been removed.

p.13, l.391: the release of DOM that stimulates nitrification will affect estimates of the f-ratio.

Reply: We discarded this hypothesis and make it simplier 'higher productivity = likely higher nitrification rates' (e.g., Furhman and Capone, 1991). See also previous comments about both 'why nitrification is significant in this area' and about the f ratio.

p.14, l.414: I feel the authors cannot make statements like this unless the distinction between euphotic zone and deep mixed layer processes (and their respective implications) are very clearly laid out. Moreover, the reference to unpublished nitrate δ15N and δ18O data (Fripiat et al., in prep) is problematic in that these tracers integrate over multi-seasonal timescales, such that nitrification in the winter mixed layer may remain evident in the nitrate isotopes in the Tmin layer in summer. Without being able to read the Fripiat et al. study, I find it problematic as a line of supporting evidence for the findings of this study. I suggest the authors remove reference to it.

Reply: First, we make the study of Fripiat et al. available to the reviewer by providing a copy if asked. In the nitrate δ15N and δ18O dataset, there is no indication of winter mixed layer nitrification at the onset of the bloom (uniform nitrate δ15N and δ18O in the upper 500m). There is a clear seasonal trend which appears to be erased in winter. In addition, mixed layer nitrification is required to explain the observation (subsurface nitrification is not sufficient).

p. 14-15, l.400-442: Some of the discussion in this paragraph (which, incidentally, should be divided into multiple paragraphs) is very interesting, and would be more compelling if the authors clearly distinguished earlier in the paper between euphotic zone and mixed layer nitrification. Reply: We have tried to take into account the comment of the reviewer by discussing more about the decoupling between mixed layer and Zeu, and by highlighting the differences with the rest of the ocean (see Discussion part 4.2.)

p.15, l.453: "mirrors nitrate uptake": what does this mean? Please clarify.

Reply: Done (inverse of the nitrate uptake vertical profile)

p.15, l.455-461: It seems to me that the nitrification rates can be explained by some combination of all of these things; it doesn't have to be a single explanation. I think that's what the authors are getting at too, although I would suggest a sentence clarifying that all these conditions likely contribute to creating a favorable environment for nitrifiers.

Reply: done (last sentence in the conclusion).

Fig. 2: why is the PON doubling rate so much higher than the POC doubling rate?

Reply: Actually, except for station R, there is a good agreement between PON and POC doubling rates (POC doubling rates = PON doubling rates x 1.18 (ïĆś0.11); p-value < 0.001). The mismatch for station R is likely due to the higher error sensitivity in case the doubling rate and the specific growth rate are very low (from the equation ln(2)/V).

Fig. 3: I suggest noting in the figure caption that there is a scale change between the reference station and the other stations for N uptake and primary production. => This has been added in the figure 3 caption.

Fig. 4: Please see my concerns above about the treatment of the f-ratio. It would be informative if the authors could find a way to combine panels a) and b) to account for the effect of regenerated nitrate production on the f-ratio.

Reply: We are not convinced that adding an extra panel is useful. The f-ratio is just reporting the relative contribution of nitrate vs. ammonium to total N uptake (no effect of nitrification, see previous comment). It is only when we talk about the concept of new primary production (Eppley and Petterson, 1979) that a bias from nitrification occurs (nitrate is not completely new but still assimilated). We now present both corrected and uncorrected f-ratio in the light of our discussion about the assessment of new primary production (see updated section 4.2).

Fig. 5: The different stations cannot be distinguished.

Reply: We added the label on the x axis.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C9472/2015/bgd-11-C9472-2015-

supplement.pdf

Interactive comment on Biogeosciences Discuss., 11, 18073, 2014.