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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.

Anonymous Referee #2

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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.

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.

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 a clarity.

p.2, l.36: I suggest "naturally fertilized"

p.2, l.37: this is all referring to the fertilized site, right? Please clarify.

p.2, l.38: see my comments above about the f-ratio.

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.

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?

p.5, l.130: How much did the temperature vary?

p.7, l.180-182: Please clarify the meaning of this sentence.

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.

p.7, l.190: define 2sd upon first use.

p.7, l.186-193: Given the methodological constraints described here, how confident are the authors in their nitrification rate data?

p.7, l.198: How did the authors determine the depth of the mixed layer?

p.7, l.206: "across all the study area" – what does this mean? Please clarify.

p.7, l.196-208: What about the reference (HNLC) station?

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.

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.

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.

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

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.

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

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.

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).

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.

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

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

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.

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

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 favourable environment for nitrifiers.

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

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.

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.

Fig. 5: The different stations cannot be distinguished.

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