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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 1 This study by Cavagna et al. presents new springtime observations of net primary production, nitrate uptake, ammonium uptake and nitrification rates from the euphotic zone of the Southern Ocean around Kerguelen Island. It adds to, and complements, the pre-existing summertime observations collected during KEOPS-1 yet also reveals some interesting differences. The most surprising result is the extremely high rate of nitrification. There are however a number of inconstancies and critical omis-



sions in this paper and I also find it strange that the related KEOPS-2 study by Dehairs et al (2014, Biogeosciences Discussion) is not referred to in the current paper particularly given the very strong cross over between the two studies including nitrate and ammonium uptake rates and nitrification rates obtained using different methods. These two studies do not stand entirely alone but complement each other and I have found myself in the unusual position of referring between them to better understand the data presented here. I would encourage the authors to better discuss the links between these two studies as they appear to reinforce the surprising conclusions reached here.

REPLY: the authors agree with this comment and the revised version has been upgraded in this way. See especially lines 452 to 454 in the Discussion part 4.2.

Specific comments: Nitrification is a two steps process involving the conversion of NH4 to NO2 and NO2 to NO3. It is undertaken by archaea and/or bacteria and no single organism is known to facilitate both conversions. As such care needs to be taken in the interpretation of the results presented in this study as the underlying environmental controls on archaea and bacteria may differ. This is not really explored in this ms and in many ways recognition that nitrification is a two steps process is not evident due to the way in which nitrification was measured (isotopic dilution of 15NO3 pools). I would encourage a more careful interpretation/discussion of the data given the (many) unknowns.

REPLY: We acknowledge that we only measured the second step of nitrification and that a decoupling can exist between the first and the second step but this one is likely to be low since nitrite concentration stay low and relatively constant, implying a balance between production and consumption processes. We write a short note about this in the Discussion part 4.2 (See Lines 403 to 409).

In particular, an argument made here is that iron fertilization enhances nitrification rates by promoting higher primary production and dissolved organic matter production both above the Kerguelen plateau and downstream of the plateau. This is a speculative argument unsupported by data demonstrating either that organic matter production is enhanced or that ambient NH4 concentrations are higher downstream of the plateau. All this study shows is that NPP is higher downstream of the plateau.

REPLY: the authors acknowledge this misunderstanding. The Discussion part 4.2 has been significantly modified in the light of the following rationale. (i) The fact that the euphotic layer depth is shallower than the mixed layer depth allows nitrifiers to compete with phytoplankton for the ammonium consumption within the mixed layer, meaning that a greater proportion of the organic nitrogen in primary production is returning back into nitrate. (ii) The absolute rate of nitrification is likely depending on the magnitude of primary production (which is particularly high in the Kerguelen area), stimulating the nitrogen cycle (uptake and regeneration; e.g., Fuhrman and Capone, 1991, L&O 36(8)). If primary production is enhanced, more ammonium is potentially produced. However, we disagree with the reviewer about the relationship between concentration (i.e., ammonium and organic matter) and the rates of processes (e.g., nitrification) (also for the following comments). In productive systems, most of the time, there is a balance between production and consumption processes for the intermediate products (e.g., nitrite and ammonium). It is not because ammonium concentration is higher that nitrification will be larger. If there is a balance between ammonium production (i.e., ammonification) and consumption (i.e., assimilation and nitrification) processes, ammonium concentration can remain low despite high ammonium production and consumption.

Methods: it is not clear from the description of the nitrification method (P18079 L15) whether the Atom% 15N required for the initial conditions in equation 5 (atom% 15N03 ti) was made on an aliquot of sample collected after the addition of the 15NO3 tracer or before. This may have an important bearing on the magnitude of the nitrification rates. Can the authors please clarify this as P18079 L25 implies a single post incubation sample was analyzed for atom% 15NO3. If this was the case how were the initial conditions obtained? More detail is needed as P18080 L8 suggests that initial



abundances were actually measured for NO3, but estimated for DIC and NH4. Please clarify.

REPLY: The initial atom % for 15N-NO3- is measured just after spike addition and therefore it represents the true initial condition for the incubation. All the parameters in equation 5 have been measured. This has been clarified in the revised version (lines 160 to 163 and 171 to 175)

P18079 L6/8: the reference to equation 1 and 2 is awkward. Please consider rewriting this sentence to clarify the impact that the long incubation times will have had on the uptake rates (i.e. more detail is needed). It does not appear that corrections for isotopic dilution were applied to the NH4 uptake rates, though it is recognized that the uptake rates are underestimates. What impact will this have on the f-ratio, for exemple?

REPLY: This has been clarified in the ms (Lines 142 to 157). The underestimation for ammonium uptake rates due to isotopic dilution (from 14N-ammonium regeneration) were estimated in the revised manuscript by applying a steady state model (Glibert et al., 1982) which assumes equal rate of uptake and regeneration for each nitrogen pool. The outcome of this calculation is that any underestimation of the ammonium uptake is likely to be low (on average 1.12 times lower than the uncorrected uptake rate). Taking into account that nitrate uptake rates are most of the time much higher than ammonium uptake rate (3.3 times on average), the difference induced by this correction is lower than 0.1 on the f-ratio.

The results section is very short (2 pages) compared to the longer discussion (7 pages). There is no presentation of nutrient data in support of the observations, which would be beneficial, instead there are vague statements on high and relatively uniform concentrations (P18081 L21) (P18081 L21) south of the polar front and a mixed layer average NO3 concentration is given providing no information on the variability between stations in the vertical, yet for the single station north of the polar front a range of NO3 concentrations is provided.

REPLY: We agree with the reviewer and present now a figure with nitrate, nitrite and ammonium concentrations (Figure 2 in the revised manuscript and associated Annex document detailing profiles for each station).

Later, (P18081/2) there is a vague statement on a slight NH4 and NO2 accumulation in the mixed layer across the study area but with concentrations remaining lower than 0.5$\mu$mol/L (but no data is shown to support this). If there is no obvious downstream enhancement or even spatial/vertical variability in NH4 concentrations then the suggestion that iron fertilization enhances nitrification rates cannot be supported. More detail on the distribution of nutrients is needed particularly the vertical distribution of NH4 (see also P18089 L3 where higher rates of ammonium release are inferred but not shown, to support the observations reported here).

REPLY: We now present the ammonium and nitrite vertical profiles in the Figure 2. We disagree with the reviewer concerning the relationship between ammonium concentration and magnitude of nitrification (see our reply to one of the comments above). If there is a balance between ammonium production and consumption, then, ammonium concentration remains low even at high remineralization and nitrification rates. This rational also holds for nitrite.

There is too much repetition in the discussion due to overly lengthy discussion of the data and parts of the discussion (section 4.1., 4.2.) read like a literature review but without the critical link to the new observations reported here. The discussion could be both shorter and more focused. In particular the strongly linked assessment of integrated nitrification rates reported by Dehairs et al needs to be referred to in the discussion.

REPLY: the revised version has been updated in this way.

P18082L12: it is stated that a positive relationship exists between POC/PON biomass and doubling times, yet both figures 2e and 2f suggest that the relationship is not positive as the doubling time decreases as biomass increases.



REPLY: The authors apologize for this mistake; there is indeed a negative relationship for doubling times. This has been corrected in the revised version (line 241-243).

P18083 L18: the rationale for using the deeper mixed layer depth rather than the shallower euphotic depth for integrations is that primary production continues beneath the 1% irradiance depth. However from figure 3 it is apparent that at stations E3 and E5 the mixed layer depth is shallower than the euphotic zone. This is not addressed in the ms and suggests that the results from these two stations are biased low. Also there is no presentation of integrated nitrate or ammonium uptake data, or of nitrification rates which makes mention of the integration procedure superfluous (also I would encourage the authors to clarify the differences in the stated integration depths between Dehairs et al (to 0,01% PAR) and this study (to the mixed layer). Clearly these are not the same.

REPLY: The choice of integrating over the mixed layer was motivated by the fact that (i) this layer is well mixed and shows a steep density gradient at the bottom, implying uniform biogeochemical properties and limited exchanges with the underlying ocean, and (ii) the euphotic layer in which most of the primary production is taking place (>85% of the total) is more shallow. However we agree with the reviewer that for two stations (E3 and E5), the mixed layer depth was actually more shallow than that of euphotic layer. For these two stations, the integrated primary production over the mixed layer was 40 and 20% lower than the one integrated over the euphotic layer. This is now indicated in the manuscript (line 305-309). Please note that we now also discuss about integrated N data (showed in Table 1, discussed in Discussion part 4.2.). About the differences in integration depths between our study and Dehairs et al. (2015), the difference between integration over MLD and 0.01% PAR level is less than 10%, except once again for station E3 and E5 because of the shallower mixed layer.

Why is there no presentation or discussion of integrated N uptake rates? This seems to be easy and useful addition and would allow comparison to other similar studies (e.g. Lucas et al. 2007; DSR II – crozex study; or Cochlan 2008 – Southern Ocean).

REPLY: We now discuss about integrated N data, as well as referring to Lucas et al., 2007 and Cochlan, 2008 (see table 1 and lines 373 to 383)

P18083 L3: figure 4b is not described in the results section, but is referred to later in the discussion section. Reference to this figure needs to be made earlier

REPLY: Figure 4 is now Figure 5 and Figure 5b is now referenced in the results section (line 264).

P18087 section 4.2. Much of this section is repetitive from earlier sections of the manuscript and can be shortened. For example, there is no need to re-describe the variation in the f-ratio from productive to less productive waters (this is done on P18083).

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

P18089 L6: although substrate availability is likely important for nitrification rates it is speculative to argue that substrate concentration is also linked to nitrifier community efficiency. The nitrifier community is unknown (archeae and/or bacteria dominated?) and the two step process of nitrification from NH4 to NO2 and from NO2 to NO3 is undertaken by different organisms. No mention is made of NO2 concentrations despite its importance though one assumes it is a minor term. It is more critical to present the NH4 concentration data.

REPLY: We reformulated our hypothesis explaining why nitrification is important in this area (Discussion part 4.2.; see previous comment): (i) decoupling between MLD and Zeu, and (ii) high primary production which likely stimulate the N cycling (uptake and regeneration; e.g., Fuhrman and Capone, 1991). In the revised manuscript, we now discuss about the two nitrification steps and the expected balance between these two steps given the relatively low and constant ammonium and nitrite concentration in the mixed layer. Ammonium and nitrite concentrations are now shown in the figure 2 of the revised manuscript.



P18090 L22: It is stated that "ammonium assimilation rates are much lower than nitrate and nitrification efficiently competes with phytoplankton for ammonium". This statement is both incorrect and garbled. From figure 3 it is clear that ammonium assimilation rates at station R2 exceed those of nitrate assimilation (though this is correctly stated on P18087 L16), and nitrification (a process) does not compete with phytoplankton, rather the nitrifiers compete.

REPLY: this sentence has been removed and we now discuss about the integrated N data (Table 1)

Figures: generally clear and readable however figure 1: it is rather difficult to see the position of the 7 stations sampled in this study (excluding reference station R2) given the inclusion of all KEOPS2 stations in the figure. Please consider making the station labels and/or station markers larger. Also according to the white labels used in the figure to denote sampled stations I see stations F-L, 3, E1, E2, E3,E4E, E5 were sampled. Clearly there is a mismatch in labeling and identification. Please correct.

REPLY: Figure 1 has been modified to fit better with clear visibility of stations locations.

Figures 3 shows at least 6 data points for station F-L, yet figure 4 shows only 5. Where is the missing data point?

REPLY: there were missing points for all those stations with a MLD deeper than Zeu because figure 4a showed profiles through the euphotic layer only (down to 1% PAR). Zeu was chosen here because we wanted to show the relationship between the f-ratio and nitrogen uptake in the productive part of the water column (where most the N-uptake is ongoing). However, MLD integration is also accurate: this figure has been replaced by MLD integrated N data (fig. 5a in the revised manuscript).

Figure 5: it is not possible to identify the stations producing the data points shown in this figure. As such the caption is meaningless. I suggest adding labels to the data points or x-axis to better clarify which data point comes from which station.

REPLY: This figure has been removed and replaced by the Table 1 (also presenting integrated N data).

Figure 6: there are more symbols used in the figure than portrayed in the legend and cross-referencing to figure 1 is difficult due to the quality of the figure 1 in my pdf of this article. I would add more information to the legend to remove all doubt.

REPLY: We now add in the figure caption a description of the symbols used for the KEOPS2 stations.

Minor comments: there are numerous grammatical issues throughout the manuscript. I have listed those I spotted below but the ms would benefit from a careful reread. Page 18075 Line 3: Insert the word of "…downstream of the.." => done P18075 nitrification rates are wrongly reported with units of mmol C m-2 d-1 in the abstract. => done P18076 L7: Replace sentence with "Concern regarding ongoing climate change has triggered great interest in this part of the global ocean" => done P18080: Equations 3, 4 and 5 are not numbered => done P18082 L23: Station "PF" appears wrongly identified. I assume the correct station is F-L (as noted on Line 14) => done P18082 L25: Station "PF" appears wrongly identified. I assume the correct station is F-L (as noted on Line 14) => done P18083 L9: Please add correct chemical species to the nitrification rates for clarity i.e. umol N L-1 d-1 => done P18083 L15: Replace a with at ": : :but at much lower rates" => done P18083 L17: Marra et al. (2014) reference is missing from reference list => done P18084 L3: Remove the word such "For the Atlantic sector [such] low primary production rates:" => done P18085 L7: Please use full units i.e. umol C L-1 and umol N L-1 => done P18086 L10-15: It is not possible to see the spring summer difference indicated on Figure 5 (see also specific comments above) => Figure 5 only shows only data from KEOPS2 expedition (original data presented in this study) but in the ms, we cite Mosseri et al. 2008 to highlight the higher primary productions the authors observed during KEOPS1 expedition (summer). Our aim is to highlight the fact that NetPP is higher in summer than in spring in agreement with satellite chl-a concentrations. P18086 L23: Remove the



word 'still' => done P18086 L24: Concentration should be plural => done P18088 L15: remove the word 'fits' => done

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C9462/2015/bgd-11-C9462-2015-supplement.pdf

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