Biogeosciences Discuss., 11, C7916–C7921, 2015
www.biogeosciences-discuss.net/11/C7916/2015/

Biogeosciences
Discussions
Open Access

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 #1

Received and published: 7 January 2015

Review of Biological productivity regime and associated N cycling in the vicinity of Kerguelen Island area, Southern ocean by Cavagna et al.

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 inconsistencies and critical omissions 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.

Specific comments Nitrification is a two step process involving the conversion of NH4 to NO2 and NO2 to NO3. It is undertaken by archea 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 archea and bacteria may differ. This is not really explored in this ms and in many ways recognition that nitrification is a two step 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.

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.

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 % 15NO3ti) 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 analysed for atom % 15NO3. If this was the case how were 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.

P18079 L6/8: The reference to equation 1 and equation 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 recognised that the uptake rates are underestimates. What impact will this have on the f-ratio, for example?

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) 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. 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 umol/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).

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.

P18082 L12: 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.

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 depth. 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).

Why is there no presentation or discussion of integrated N uptake rates? This seems to be a 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)

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.

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).

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 (archea 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.

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 stations 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.

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 KEOPS-2 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, A3, E1, E2, E3, E4E, E4W, E5. However in the methods section (P18078, L5) it is stated that only stations F-L, A3_2, E4W, E1, E3, E4E, E5 were sampled. Clearly there is a mismatch in labeling and identification. Please correct.

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

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

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.

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. . ." P18075 L15: Nitrification rates are wrongly reported with units of mmol C m-2 d-1 in the abstract. P18076 L7: Replace sentence with "Concern regarding ongoing climate change has triggered great interest in this part of the global ocean" P18080: Equations 3, 4 and 5 are not numbered P18082 L23: Station "PF" appears wrongly identified. I assume the correct station is F-L (as noted on Line 14) P18082 L25: Station "PF" appears wrongly identified. I assume the correct station is F-L (as noted on Line 14) P18083 L9: Please add correct chemical species to the nitrification rates for clarity i.e. umol N L-1 d-1 P18083 L15: Replace a with at ". . .but at much lower rates" P18083 L17: Marra et al. (2014) reference is missing from reference list P18084 L3: Remove the word such "For the Atlantic sector [such] low primary production rates. . ." P18085 L7: Please use full units i.e. umol C L-1 and umol N L-1 P18086 L10-15: It is not possible to see the spring-summer difference indicated on Figure 5 (see also specific comments above) P18086 L23: Remove the word 'still' P18086 L24: Concentration should be plural P18088 L15: remove the word 'fits'

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