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Vrije Universiteit Brussel

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To Biogeosciences Editor Office,

Dear Editor,

Please find below the detailed responses to the two reviewers who took the time to evaluate our manuscript now titled "Production regime and associated N cycling in the vicinity of Kerguelen Island, Southern Ocean".
We sincerely hope that we have formulated convincing responses and a revised version fit for the next step of publication process.

We are looking forward to hearing from you,
Best regards,

Anne-Julie Cavagna, François Fripiat & coauthors

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 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.

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 $^{15}N\text{-}NO_3^-$ 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 ^{14}N-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 NH_4 and NO_2 accumulation in the mixed layer across the study area but with concentrations remaining lower than 0.5µmol/L (but no data is shown to support this). If there is no obvious downstream enhancement or even spatial/vertical variability in NH_4 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 NH_4 (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

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 (ΔNO_3 measured = NO_3 ti – NO_3 tf) with calculated variation of nitrate concentration through incubation experiment (ΔNO_3 calc = NO_3 uptake – NO_3 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 $\delta^{15}N$ and $\delta^{18}O$ 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 (±0.11); 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.