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Interactive comment on "The significance of nitrogen regeneration for new production within a filament of the Mauritanian upwelling system" *by* D. R. Clark et al.

Anonymous Referee #1

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GENERAL COMMENTS

Clark et al. describe microbial nitrogen cycling in an upwelling system off the coast of Mauritania, highlighting the importance of nitrogen regeneration in sustaining primary production in upwelling systems. There have been several studies obtaining similar results in the past, so the conclusions are not novel, but still the measurement of other fluxes not so commonly included in previous works (i.e. NO2 oxidation) is important and the study worth publishing if several details can be clarified and the text improved. The authors use a nifty Lagrangian approach to follow upwelled water masses (sulphur hexafluoride and 3He), which is appreciated as maintaining a real Lagrangian sampling

in upwelling systems is well known to be challenging.

Despite the methods section is considerably long, the methods are unclear and at times confusing (see specific comments below). It is not clear how many replicates were used per type of sample, why some bottles were incubated in the light and others in the dark, etc. Then other methods such as indophenol and sudan-1 synthesis are described in great detail, which is not needed if the authors have previously described them in other publications. I suggest reorganizing and summarizing the methods section. Finally, I strongly recommend separating the results and the discussion as two separate sections. The amount of data is considerable and having a separate results section should help expert readers identify patterns and compare rates to previous works.

The reference to previous studies and interpretation of the data in the light of the planktonic community responsible for the fluxes measured is insufficient. Despite the authors only have larger phytoplankton counts and no flow cytometry data, their fluxes could be better discuss in the light of previous works in the NW African upwelling.

Given the existing literature on this subject published in the last $\sim$20 years, and the different approaches taken to measure new and regenerated production, it is perhaps the time to produce a review paper comparing the data obtained in different systems, homogenize results, and even provide the community with a consensus formulation for new and regenerated production (concepts which need to be revisited, as the authors discuss “it is likely that NO3 based exportable production from such systems has been over-estimated historically”).

SPECIFIC COMMENTS

ABSTRACT

The abstract contains many details on the data, but none on their interpretation.

INTRODUCTION

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The introduction could highlight better how considering regenerated production may change our view of upwelling ecosystems and global carbon and nitrogen cycling in marine systems.

Page 17785, line 5: please also mention all other nitrogen fluxes that may influence f-ratio calculations (DON release, N2 fixation. . .), including appropriate references.

MATERIALS AND METHODS

Page 17785, line 20: please provide details on the remote sense data (satellite used etc), how much time passed between detecting the filament and starting sampling it

Page 17785, line 25: "GC" is not described elsewhere in the text. More details on the SF6/3He detection method should be provided (i.e. are these compounds detected in a continuous way, like with seawater pump continuously through the GC? Or sampled discretely?

Page 17786, line 1: when is that? Provide detection ranges, limits.

Page 17786, lines 5-6: it would be clearer to list which stations were sampled at which depth levels and which were not.

Page 17786, line 12: do the authors know what's the temperature range within their flushing boxes along incubations, and/or how does it compare to in situ temperature?

Page 17786, line 25: the authors mention chemicals and where they were acquired from before actually saying what they used them for. I would recommend describing how 15N additions were made and then stating in parentheses where the chemicals were obtained from.

Page 17786, line 27: why was the sample blacked-out? Was it only maintained in the dark until being dispensed in different incubation bottles and amended with 15N? Why regeneration fluxes were measured in the dark and assimilation ones in the light? Do the authors have any evidence that nitrogen regeneration is not performed by photo-

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heterotrophic bacteria for example?

Page 17787, line 9: so one bottle or set of bottles was used to measure NH4 regeneration in the light and another one in the dark? It is unclear. Bottles were incubated in monoplicates but then split in triplicate subsamples for analysis? Are the error estimates given in Figure 5 analytical standard deviation? It is not clear from the methods.

Page 17787, line 18 and elsewhere: the range of %15N enrichment caused by 15N-labeled substrate addition should be mentioned somewhere in the methods.

Pages 17788-17789: consider reducing methods and referring to previous publications where possible (see general comments above).

Page 17790, lines 4 and following: 660 mL for 15NO3 and 15NH4 incubations seems a really small volume. What was the range of PON concentrations measured? what is linearity limit of the IRMS used? Can δ15N values be given with enough accuracy which such low PON concentrations (if they were low)?

Page 17790, line 9: assimilation incubations were performed for 6 h, while regeneration ones were 8 h. The use of short incubations for the measurement of regeneration fluxes is common, but the assimilation incubation times seem short to me. The authors should state why these timings were chosen, as well as discuss how this might have affected their rates and compare to other studies using different incubation times.

Page 17790, lines 10 and following: it is not clear how many replicates were done.

Page 17791, line 9: this notation (T-1-T7) has not been explained before in the methods section.

Page 17791, lines 15-20: a table including the variability of RNO3 values along the study would be helpful. So, in summary, the proportion of RNO3 with respect to total NO3 increases with distance to the recently upwelled water near the coast? Please add a conclusive sentence at the end of this paragraph.

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Page 17792, line 10: It is a pity that flow cytometry data are not available.

RESULTS AND DISCUSSION

At the time of their sampling the authors found that the filament contained mainly NACW, which has a lower nutrient content than SACW. Can the authors discuss, in the light of water mass depiction in this upwelling system (previous publications) how different water mass proportions and seasonality may affect microbial nitrogen fluxes on a yearly basis?

Despite the authors do not have prokaryote abundance data, it is worth discussing their role in regeneration processes referring to previous works. Also, the use of GF/F filters and the related potential underestimation of nitrogen fluxes should be discussed (Morán et al., 1999).

Page 17793, lines 26-27: the description of T-1 etc should be included in the methods section.

Page 17793, lines 18-20: it is unclear how the mapping was done. Regular (multiple) CTD casts? Moving vessel profilers, SeaSoar? It should be better explained in the methods.

Page 17793, lines 25-27: so the Lagrangian study was successful from days 0 to 7, but not beyond (lines 5-7), or is it not fully reliable from days 0 to 7?

Page 17794, line 17: Even if N2 fixation was not targeted in this study (and is likely minimal in this upwelling system) it should be mentioned and discussed. The low N:P ratios found could promote diazotrophic activity. It is worth discussing works like Raimbault and Garcia (2008), Sohm et al. (2011) and others. The subject is briefly mentioned in Page 17795, line 6, but could be further developed.

Page 17794, line 27: you mean denitrification?

Page 17795, line 2: the authors mention here P* without having explained what it is,

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how it is calculated etc.

Page 17795, line 2: It is probably out of the scope of this paper, but can the authors use offshore transversal velocities to estimate how much phosphate (and other nutrients) are exported offshore (export rates) by the filament and compare with other upwelling systems? It would be interesting to compare with data from other filaments like those in California, off the Iberian Peninsula and Cape Ghir, for example. Presumably physical data can be easily obtained from Meunier et al. 2012 (?). This could be included at the end of the discussion (Page 17802).

Page 17795, line 8: very vague, please be more precise.

Page 17795, line 12: add references to figures where needed.

Page 17795, lines 13-21: and why is this? Different nutrient regime? Try to compare with other upwelling systems (reasons behind different primary production rates).

Page 17795, lines 22-23: state ranges, refer to figure.

Page 17796, lines 9-10: state ranges of phytoplankton abundance (here and in the following lines).

Page 17796, line 11: at least state % of carbon provided by diatoms.

Page 17796, line 15: the high proportion of flagellates is important and explains the predominance of regeneration fluxes. Discuss further, cite other works where the protagonism of flagellates in upwelling systems has been highlighted (e.g. Anabalón et al., 2014; Böttjer and Morales, 2007).

Page 17796, line 17: POC would be better if available.

Page 17796, lines 20-24: See Benavides et al. (2013).

Page 17798, line 17: or active release.

Page 17798, line 20: and probably negligible in your samples due to the small volume

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used.

Page 17800, lines 4-24: before speculating on particle-attached nitrifying organisms, I suggest discussing the much more oxygenated character of this upwelling system in comparison with Peru for example and how this affects nitrification/denitrification processes.

Page 17800, lines 25 and following: here the authors could refer to the RNO3 table proposed above.

Page 17802: Have the authors tried converting new production to carbon using Redfield (or C:N ratios from their own samples) and comparing those rates to 14C-based primary production data?

Page 17802, conclusions: Can the authors estimate how much new production rates in upwelling systems are overestimated by not discerning between 'new' and 'regenerated' NO3?

Figure 1: This figure needs to be improved. The longitude is not aligned between panels. The path of the labeled water mass (SF6 distribution) should be superimposed on the map. Satellite images of Chl and/or temperature would be helpful for the reader to see the structure of the upwelling filament.

Figure 3: This figure is barely discussed in the text. The differences in N:P ratios between MLD and below MLD are interesting and merit discussion. Also, MLD is not written in full at first use in the text (Page 17798, line 4).

TECHNICAL CORRECTIONS

Lagrangian and Eulerian should be capitalized throughout the text.

Page 17786, line 17: CTD is fully written here, but in fact was mentioned before in the text (line 3 of the same page).

Page 17794, line 28: here the authors use 'N' instead of 'nitrogen', please be consistent

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throughout the text. Same in Page 17796, line 1 and elsewhere.

Page 17795, line 24: 'northwest African', also written as 'North West', and 'NW' elsewhere in the text. Please be consistent.

Page 17800, line 3: typo "μmnol"

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