

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.

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We thank the reviewer for this thoughtful review.

Specific comments 1) NH_4^+ regeneration: There might be NH_4^+ production by photochemical processes as well, see e.g., Rain-Franco et al. (2014). So, I am wondering whether NH_4^+ regeneration by photoproduction in the upwelling off Mauritania/NW Africa may play a role as well. >This is a good point and would be likely to contribute to the process in situ. As noted by Rain-Franco et al (2014), the processes of photoproduction and NH_4^+ regeneration cannot be separated. We will acknowledge that a contribution from photoproduction is a possibility in our data set although we are un-

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able to estimate its contribution. It is also worth noting that by using polycarbonate incubation bottles we exclude any contribution to photoproduction of NH_4^+ from UV.

2) N deposition by aerosols may play a role for new production too; especially in view of the fact that filaments off NW Africa can receive a lot of Saharan dust input. Please discuss. >Reviewer 1 requested that we expand our consideration of N-sources that contribute to f-ratio determinations. Atmospheric dust inputs of nitrogen falls within this scope and will be included in the revised version of the manuscript.

3) Nowald et al (2015) present particle flux (OM flux) data from a sediment trap deployed at the same time (and very close to the filament track) of the study described in the ms under review. I am wondering whether the OM flux data by Nowald et al may match those presented in Section 3.5. >While the Nowald et al (2015) paper is interesting, it is difficult to directly link our studies. Nowald et al compare seasonal patterns of particle fluxes using sediment traps deployed at 1100 meters. The authors note that we do not fully understand the processes which influence particle formation, transport and destruction and links between surface ocean chlorophyll and sediment trap records are poorly understood. However they also note that once particles escape the zone of biological activity little subsequent transformation takes place. Consequently, there are indirect links between our studies in that new production (C-export; our study) estimates set an upper limit for vertical C-flux (i.e. the Nowald study). We will cite the Nowald et al paper and highlight this indirect link although we do not propose to make direct comparisons between our data sets.

4) There are rather old (but nevertheless important) studies on nutrient distribution and primary production off Mauritania/NW Africa by Minas et al. (1982a, b; 1986) which are ignored. Minas et al. calculated f ratio (0.9), N:Si ratios and measured PP rates. I suggest that these data are included in the discussion. >Unfortunately we do not have access to the Minas et al 1982a publication. However, Minas et al 1986 refer to a separate study (Minas et al 1982) in which an f-ratio was estimated in the absence of NH_4^+ regeneration data; a value of 0.9 was provided and deemed to be an over-

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estimate. Their revised value of 0.64 in Minas et al 1986 is almost identical to that measured on the first day of this study (0.61-0.63, depending upon method used to account for N regeneration). We will include this comparison in our discussion.

>Minas et al (1986) provide an estimation of primary production of 2.312 g Cm⁻² d⁻¹ for the NW African upwelling. This value falls within the range we report from our study and will be included in our discussion.

5) In Zindler et al. (2010) N:P ratios and phytoplankton composition from the upwelling off Mauritania are presented. This ref. should be cited as well (see e.g., Sections 3.1 and 3.2). >Zindler et al (2010) presented N:P values approaching 20 for freshly upwelled water, dropping to 0.1 as upwelled water advected offshore. This supports our suggestion that we caught an early stage of the upwelling process, but possibly not the earliest stage (values in the upper MLD were 13:1). We will include this point and cite the Zindler et al paper.

6) p. 17800: I am not fully convinced by the discussion about particle associated nitrification. In a recent study by Ganesh et al (2014) it was shown that indeed denitrification is particle associated but not nitrification. So, I suggest that denitrification in sinking particles could take place in oxic subsurface water masses off NW Africa. >The literature on microbial activity in association with particles is complex. Ganesh et al (2015; p2687) state that while sequences matching ammonia and nitrite oxidising microbes were more abundant in free living rather than particle bound (1.6 – 30 μm) fractions, they were nevertheless identified in both fractions suggesting that nitrifying organisms were particle bound in this OMZ (noting that the study was of an OMZ rather than oxygenated water column, which modifies the activity of N-cycle microbes). We acknowledge that this suggestion is speculative (we are unable to present our direct evidence relating to the European Shelf Sea) and perhaps this point needs to be emphasized. However, the mechanism we propose is consistent with our observations. It offers an explanation as to how the decoupling between NH₄⁺ and NO₂⁻ oxidation (observed in many studies as cited) can be sustained. > We make the point that denitri-

fication is unlikely to be taking place in our incubations of an aerobic water column (we report O₂ concentrations); we will add that this is because denitrification is a strictly anaerobic process. >In our revised manuscript we will emphasise that this proposed mechanism is speculative. We can offer no more than the reasoned arguments already presented.

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