

***Interactive comment on “<sup>15</sup>N enrichment in the surface Particulate Organic Nitrogen of the north-eastern Arabian Sea from the middle to the waning phase of the winter monsoon: possible causes” by S. Kumar and R. Ramesh***

**Anonymous Referee #2**

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The authors observed an increase in  $d^{15}N$ -PON of 5 pro mille in the surface waters of the north-eastern Arabian Sea between the middle (January) and waning phase (February-March) of the monsoon. They explain the observed enrichment by exploring four hypotheses, being (1) enrichment of surface nitrate in  $^{15}N$  due to an intensification of denitrification in the deeper layers, (2) mixing of  $^{15}N$  rich nitrate from the deeper layers with surface nitrate, (3) change in the extent of fractionation of the phytoplankton community and (4) inefficient utilization of nitrate.

The introduction is too concise. It contains simplified statements that do not capture

the complexity of the N cycle and the use of stable isotopes to study N cycling. The material and methods sections is incomplete, lacking a description of nitrate sampling. Overall, it is difficult to follow the rationale of the authors, because many statements stand alone, without reference to the appropriate results. It makes the reader have to go back to the appropriate results or discussion sections many times. The conclusions are not always supported by the results and are based on many assumptions that are not always supported by literature (no references). Many statements lack back-up from data or literature. The data reported in this study are not sufficient to distinguish between the possible processes that could cause the increase in d15N-PON. Therefore, the manuscript is highly speculative.

### Introduction

In Line 14-18 of the introduction (p 246) contains contrasting statements: First it is stated that d15N values of marine organisms vary significantly, then the authors state that phytoplankton, zooplankton and fish have fixed values of 7, 10 and 15 pro mille, respectively. Are these values characteristic for that region? It should be mentioned if this is the case.

P247: Line 1: this statement needs a reference. Line 2: It would be better to formulate the sentence as follows: 'the areas with d15N-PON values close to 0 pro mille may thus be...' Line 3: 'However, lower d15N could also be caused...' Lower relative to what? Line 9-10: 'Isotopic fractionation during the formation of PON...' and 'd15N of PON records the availability...': This sentence should be reworded and needs more clarification. Did you mean to say that the extent of isotope fractionation during the formation of PON reflects the N source taken up?

### Material and methods

It lacks a description of how the nitrate samples were taken. Were the analyses done on filtered seawater? At what depth were the samples taken?

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## Results

Line 16-17, p 250: Do you mean to say that st. 13 had highest PON concentrations during both the January and February-March cruises? Line 21, p 250: Is there any reason to exclude these stations? Line 23, p 250: 'The phytoplankton community...' This remark should be moved to the discussion section, if this has not been investigated in the current study. Line 6, p 251: 'The phytoplankton community...' Again, this section should be moved to the discussion. Please give full names of species, when mentioned for the first time. It would also be helpful to specify to which phytoplankton groups Noctiluca and Trichodesmium belong. Line 8, p 251: On from this paragraph, results from both periods are combined. This paragraph should have a new subheading. Line 10-12, p 251: This statement needs to be clarified and needs a reference. Why would one expect a negative correlation between NO<sub>3</sub><sup>-</sup> and d15N-PON in the current data only because the nitrate concentration in a region is high? I cite: '...as in regions where the surface nitrate concentration is high'. Please explain. Line 19, p 251: Has the study of Montoya and Voss (2006) been carried out in the same region?

## Discussion

Line 7-9, p 252: Based on which result did you draw the conclusions that (1) there is entrainment of nitrate in the surface layer - if it is based on the temperature profiles, it would be good to mention so - and (2) this nitrate is enriched - did you mean enriched in 15N? It would be helpful to see the spatial/temporal distribution of NO<sub>3</sub><sup>-</sup> plotted in a graph. Line 10-11, p 252: It is not clear on what this conclusion is based. Line 17-18, p 252: Why would very high production in a region imply that the source N used is NO<sub>3</sub><sup>-</sup>? Line 27, p 252: Did you measure an increase in d15N-NO<sub>3</sub><sup>-</sup>? Line 1, p 254: There are a few steps missing in the rationale. '...a decrease in d15N of nitrate due to the dilution by lighter isotopes added due to nitrogen fixation in the surface layer'. Nitrogen fixation itself will not add 15N depleted nitrate to the water. Line 2-4, p 254: On what is the assumption of 'similar level of dilution of nitrate' based? Line 24, p 254: 'natural' should be replaced by 'surface' nitrate. Line 13-16, p 255: The explanation given

by the authors, namely a 'flexibility of phytoplankton to fractionate differently during January and February' is very unlikely to explain the observed change in  $\delta^{15}\text{N}$ -PON. Fractionation by the phytoplankton community may only change when there is a major shift in the phytoplankton community composition and if this shift involves species with different fractionation values. The term 'flexibility' is not appropriate here, because it insinuates that phytoplankton can change its fractionation value. Although a change in fractionation value due to a change in nutrient concentration has been described (Pennock et al. 1996, *Limnology and Oceanography*), I understand that the authors actually mean to say that the apparent fractionation value has changed. Line 20, p 255: 'euphotic zone integrated nitrate concentration' - does that mean that nitrate concentrations were measured at several depths? The material and methods section does not mention how/where nitrate was sampled. Line 2, p256: Earlier a value of 5 ‰ was used as the initial  $\delta^{15}\text{N}$  value of nitrate. Why now change to 7 ‰? Line 6, p 256: you can omit the equation  $\epsilon = (\alpha - 1) \times 1000$ . It only makes sense to mention this when you use isotope ratio notations. No further reference to  $\alpha$  is made and all values for fractionation factors reported in the text are in the  $\epsilon$ -form.

### Conclusion

Why did you omit the second hypothesis (dilution of surface nitrate with deeper nitrate) from the conclusions?

Figures: Explain what the squares and circles in figure 3 stand for. Figure 4 is redundant. It would be interesting to add a plot of the spatial/temporal variability of nitrate.

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