

Interactive comment on “Spatio-temporal patterns of C : N : P ratios in the northern Benguela upwelling regime” by A. Flohr et al.

Anonymous Referee #3

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GENERAL COMMENTS:

The authors of the above manuscript present a study of the spatio-temporal variability of elemental ratios in the north Benguela Upwelling (NBUS). They used three in-situ datasets collected in different years (but similar season) to analyse the general patterns of C/N/P/O ratios and their variability.

The topic is highly relevant and the paper is clear in its objectives and its presentation. The authors hypothesised a very interesting mechanism that could control the behaviour of the NBUS (source or sink of N/P) at a basin-scale. However, their analyses are sometimes lacking details and precisions, while an extensive discussion of their results (and associated caveats) is required. I have made a number of suggestions below which, I hope, will help the authors to improve the overall quality of the

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article.

In particular, I suggest to further examine the datasets to properly characterize the biogeochemistry of the water masses of interests and to describe the vertical structures of the elemental ratios, allowing a full consideration of all local and remote factors affecting C/N/P/O ratios in the region. The article would also benefit (i) from a clear identification of the caveats associated with the present approach and (ii) from a developed discussion of the influence of the southern BUS and the possible factors driving the N* variability in SACW. Some technical comments were also made to improve the figures, polish the text and correct the remaining typos.

Overall, I recommend the publication of this manuscript in BG providing the authors revise their manuscript to address the below points.

SPECIFIC COMMENTS:

- The abstract should be revised according to the below comments (and those from the other reviewers).
- In the introduction, it may be worth mentioning the stoichiometric ratio of O₂ / AOU versus other dissolved elements and indicate briefly their interpretation.
- One may re-organise the “Materials & Methods” section as followed: 2.1 Study Area – 2.2 Water sampling, laboratory work and data analysis – 2.2.1 C_T and A_T – 2.2.2 Dissolved nutrients – 2.2.3 Elemental stoichiometry – 2.3 Characterisation of central water masses.
- p. 10463: make sure you are consistent with the water masses characterisation. Line 6: I believe ESACW stands for Eastern South Atlantic Central Water. Line 16: are you dealing with central or intermediate water masses? The description of the large scale pathways are not very clear to me.
- In 2.2: please indicate what is the mean vertical resolution of your sampling, and if some depths were particularly targeted.

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- In 2.2.3 (previously 2.4): a few details are missing and might be reported using short sentences such as: (i) "NOx is used hereafter for nitrate (NO₃-) and nitrite (NO₂-)"; (ii) "AOU stands for ... and was calculated according to..."; (iii) when describing P*, the readers would benefit from brief explanations indicating how to interpret positive/negative deviations and how its analysis is complementary with the one of N*.

- I am also in favour of the new T/S plot (new Fig. 2) which allow further analysis of the water masses of interest. Please indicate in sect. 2.3 the O₂ content of SACW (ESACW, respectively) if it is already well known (+ appropriate ref.). If it is a new result, the authors could analyse the T/S plot (Fig. 2) to derive typical O₂ concentrations of these water masses and then flag it in the results section. I also suggest the authors to zoom over the T/S plot to focus on the difference between SACW and ESACW. One could be colouring the data points (in the T/S space) by their AOU/N*/P*/C_T to see if these water masses have robust biogeochemical signatures. I understand that this will decrease largely the number of data points (since only water samples can be used) but it may still provide a more precise characterisation of these water masses.

- p. 10468, analysis of fig. 3: one could transfer lines 9 to 16 after the sentence ended line 3. This allow to analyse first the data with low O₂ content, and then the one characterised by higher O₂. Lines 1-3 could be rephrased, in accord with Fig. 3: "...C_T inputs from anaerobic respiration (denitrification) and oxidation (anammox)..."

- Previous Fig. 3 and 4 (merged into new Fig. 4 in a response letter by the authors): why do you use only data from the latest cruise to analyse general patterns? I suggest to include all the data (3 cruises) to build Fig. 4 (consistently with your old Fig. 2) and to analyse the general patterns without the influence of a specific oceanographic context occurring during a single cruise. Then, the paper focuses on spatio-temporal variability in the next section and so it is natural to describe/analyse your 3 datasets one by one.

- Fig. 5: I suggest the author to explore also the vertical sections of the biogeochemical proxies N* and P*. It might support clearly their analysis (p 10469, lines 4-16) that I

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did not find very convincing. Related to lines 13-16, there is indeed an important latitudinal large-scale gradient of N offshore the Namibian/South-African coastline, which is clearly visible in climatological datasets such as WOA09. It suggests that the overall latitudinal NOx decrease is more likely due to large-scale effects and water masses variability (as mentioned by the authors) than to local N loss.

- p. 10470, lines 3-6: the authors discussed the horizontal distribution of water masses (SACW/ESACW) depending on the upwelling intensity, which is then impacting the water found on the shelf. Another effect of upwelling intensity on the shelf waters is the vertical origin of the upwelled waters. Stronger upwelling-favourable winds are generally associated with an upwelling from deeper sources. This will in turn affect the nutrients concentrations of the upwelled waters (usually higher), even if they originated from the same water mass. The authors might want to consider, or at least to discuss, this "vertical" effect.

- Section 3.2 and 3.3 (p. 10469-10472): the authors compared the distribution and characteristics of the SACW, O₂ and N* and their spatio-temporal variability in relation with upwelling intensity. Their main conclusions, if I understood well, are: (i) the distribution of SACW explain well the spatial variability of O₂ (except over the shelf off Walvis Bay, where low levels are always observed, due to water/sediment interactions along the mud belt) (ii) the N* signature of SACW are very variable over space and time and (iii) the variable N* in SACW, itself driven by the intensity of N₂-fixation further north, is controlling the amount of excess N brought into the NBUS and thus the consequent amount of P exported offshore. The interpretation is sound and the hypothesis is interesting. However, some caveats remain in their analysis and other possible mechanisms were omitted or poorly discussed. To strengthen their conclusions, the authors should develop the discussion of the present results and provide more robust evidences of such mechanism. In particular, the mismatch of the spatial distribution of N* (Fig. 8a) and SACW (Fig. 7d) at 200 m seems to suggest that the positive N* observed in March 2008 is not only linked to the presence of SACW. In par-

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ticular, the shelf region south of Rocky Point ($\sim 19\text{--}21^\circ\text{S}$) is indeed characterised by 50 to more than 90% of SACW, whereas it has negative N^* . Maybe some local factors are involved here? Did you also look at spatial maps of P^* ? Did you consider the possible influence of water masses (with specific biogeochemical signatures) advected from the southern boundaries of your domain (i.e. southern BUS)?

- p. 10471, lines 2-10: I found this argumentation a bit wobbly. The authors proposed that the high N^* found in the NBUS originates from the high level of N_2 -fixation in the sub/tropical North Atlantic, already documented by numerous studies. But the subsurface pathways that would connect the sub/tropical North Atlantic (roughly between 0 to 20°N) to the NBUS ($18\text{--}23^\circ\text{S}$) are not reported, nor obvious. Maybe it is a typo and they were indeed mentioning the South Atlantic? If not, this argumentation required additional clarifications and referencing.

- As mentioned above, I suggest to develop the discussion about the possible influence of the Southern BUS and the different biogeochemical functioning expected there. The SBUS has seasonal but intense upwelling and a lot of mesoscale mixing which influences biogeochemical dynamics (see also Gruber et al. 2011 and a paper in press by Hernandez-Carrasco et al., DSR, 2013). Indeed Weber and Deutsch, 2010 also found that physical mixing between water masses affects largely the variability of these biogeochemical proxies, so a short discussion of these effects would be interesting.

- As suggested by another reviewer, I am in favour of a few lines of discussion of the factors driving N_2 -fixation in the tropical ocean (see for instance Subramaniam et al. GRL 2013 and a recent paper by Straub et al. Nature 2013 for longer time-scales).

TECHNICAL COMMENTS:

- p. 10462, line 3: here and elsewhere in the manuscript, make sure that the variables are named consistently. Here it should be written: (NO_3^- , NO_2^- , PO_4^{3-}). Maybe, one could define clearly the abbreviations, such as NOx for nitrate/nitrite, P for phosphate and so on..., and then used them consistently throughout the whole manuscript.

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The current presentation is sometimes confusing and it is not easy to understand to which chemical species the authors refer.

- p. 10462, line 4: changed "raised" to "measured".

- p. 10463, line 13: originate from. Line 14: "along the continental margin" and "along the Namibian shelf break" means the same thing to me. Please delete one of them or rewrite.

- p. 10465, line 4: colorimetric. Line 17: a minus sign is missing.

- p. 10468, line 1: anaerobic. Please also rephrase the last sentence of the page, starting line 26.

- p. 10470, line 15: "...caused by the local decomposition of..."

- Please revise the figures captions, they contain many typos (e.g. captions of Fig. 1, 2, 4, 5...).

- Previous Fig. 3 and 4 (merged into new Fig. 4 in a response letter by the authors): in panel 4d, I can see a subset of a few samples (~ 6) characterised by low O_2 (red cores) and $\text{C}_\text{T} \sim 2200 \mu\text{mol/kg}$. Then, in panel b and c, there are only 3 similar data points, and none in panel a. Why is that? And more importantly, could you provide an hypothesis to explain why they are not together with the "red group"?

- Fig. 6: to improve visual interpretation, the authors could add the corresponding colorscale and remove all the labels (or just keep labels + black contours for a few key temperature values).

- Fig. 8: make sure to centre the colorscale (white) on 0.

- Vertical variability of these biogeochemical proxies is important but not explicitly analysed here. I understand it is not the core of the paper but a few brief explanations may help the readers to follow your analysis based on horizontal slices at different depths (e.g. Fig. 8). One option would be to plot vertical sections of N^* and P^* in Fig. 5.

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Another option is to improve Fig. 9 by: (i) zooming-in with a y-axis from 0 to 1000 m or less (as deep waters are not analysed), (ii) delete horizontal dotted lines but instead add a line for the vertical profile of % SACW (iii) while it presently displays only a station sampled off Kunene, I suggest to plot also a vertical profile off Walvis bay. The vertical pattern may be different due to the presence of the mud-belt at the coast.

Interactive comment on Biogeosciences Discuss., 10, 10459, 2013.

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