

Interactive comment on “Organic nutrients as sources of N and P to the upper layers of the North Atlantic subtropical gyre along 24.5 N” by A. Landolfi et al.

Anonymous Referee #3

Received and published: 21 July 2010

The manuscript deals with organic nutrients as sources of N and P to the upper layer of the North Atlantic. To this end, authors introduce the two main topics to tackle with: (1) the missing nutrients for export production in the oligotrophic subtropical Atlantic and (2), the significance of the dissolved forms of nutrients in balancing the mismatch between import-export nutrients. To be able to sustain primary production and contribute to the export production, these organic nutrients have to be labile but not much, as to be transported from the nutrient rich / production sites to the oligotrophic sites. This is the core issue, and although the idea might be appealing, the manuscript does not contribute to its resolution. One main problem is the definition of the sites of net dissolved organic nutrient production. The other is the computation of the transit time of

C1894

those nutrients. With respect to the former, authors offer no convincing evidence to justify their choosing of the 0.1 mg chl a / m³ as a threshold for net production of organic nutrients. Respect to the later, I have the impression that analytical problems and the various assumptions involved in the computation of turnover times of both DON and DOP may cast doubt on data. I refer to the different efficiencies in DOM oxidation used for DON (HTCO) and DOP (photooxidation) determinations, yielding different percentages of the total pools accounted for N and P; and to the authors assumption that those pools are accessible to the enzymes, excluding the possibility of different degrees of lability for DON and DOP molecules. Both facts have an effect on the computation of in situ enzyme activities in different ways for N and P so resulting in uncertainties in the calculated turnover times.

The analysis of the isotopic signal of particulate organic nitrogen (PON) constitutes the other important part of the manuscript. This is surprising because this issue is barely mentioned in the introduction. Otherwise, the interpretation of results of the isotopic PON signal is plagued of inconsistencies. For instance, equivalent isotopic values in western (to about 70° W) and eastern (46-30° W) parts are interpreted as indicating the presence of inorganic nutrients in the former case, and the lack of measurable nitrate in the later (page 4013, line 21 and onwards). This is at odds with the nitrate distribution of figure 3, page 4034, which shows lower nitrate concentrations in western than eastern parts.

Other objection with this manuscript refers to the interpretation of DON and DOP distributions in relation to the hydrographical setting (section 3.1, page 4011). I would like to see some comment on the relationship between the mixed layer depth, the depth of the nutricline, and chlorophyll depth distribution. The fact is that the higher DON in the west than in the east, as stated by the authors (Page 4011, line 22), coincides with a greater separation between the MLD and the nutricline depth and, as a consequence, low chlorophyll a concentrations in the mixed layer. In contrast, in the eastern parts the nutricline overlaps the MLD, so contributing more nutrients to the mixed layer and

C1895

more chlorophyll a. This contradicts the author's appreciation concerning the regions of net organic nutrient production. DOP concentrations show the reversed pattern, with high concentrations in the east and very low values in the west. I wonder if these different patterns are a consequence of the methodological constraints before stated, i.e. the inefficiency of the DOP oxidation procedure. Authors, have also to explain the subsurface DOP increase observed in the central parts.

The ensemble of problems stated above is not helpful for the interpretation of data and do not contribute to provide reliable conclusions. This in turn affect to the definition of the four regions characterized by different nutrient supply regimes (page 4002, line 8), which authors typify on the basis of the isotopic PON signature, dissolved organic nutrient distributions, mixed layer depth, etc. (page 4022), and raise as one of the main conclusion of the manuscript.

In summary, I consider that this manuscript do not provide with reliable data to answer the three main questions rose in the introduction (page 4005, line 5), in part because data interpretation and most assumptions are not justified. With reference to more formal aspects, the manuscript is poorly structured and written, and plagued of mistakes and inconsistencies. I consider that the manuscript is not acceptable to be published in Biogeosciences journal.

Interactive comment on Biogeosciences Discuss., 7, 4001, 2010.