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Comment

Interactive comment on “Spatial and temporal variations in dissolved and particulate organic nitrogen in the equatorial Pacific: biological regulations and physical influences” by X. J. Wang et al.

Anonymous Referee #2

Received and published: 18 September 2008

General comment:

This work is an attempt to describe the distribution of dissolved organic nitrogen and particulate organic nitrogen (PON), in the equatorial Pacific Ocean, and especially in the High Nutrients Low Chlorophyll (HNLC) area.

DON may become important for inclosing N budget over basins, as first proposed for the subtropics of the North Atlantic (Rintoul and Walsh, 1999). But it is true that identifying the role of DON has proved difficult through the paucity of direct observations,

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as well as the uncertainty regarding the reactivity and composition of dissolved organic matter. Simulating spatial and temporal variability of organic nitrogen distribution would help to better understand nitrogen cycling. Then, I am sympathetic to the point being addressed, and I feel this work would be done, i.e. to improve the information on nitrogen cycle in oceanic waters. The manuscript is clear and well written, but I think results are not deeply discussed and more complete interpretation can be done. For example, one of the most important challenge is the vertical export to deep waters, and a central question is how much export production through inputs of DON. More Spatial and temporal distributions of DON and PON reflect a competition between their biological sources and sinks, and their basin scale transport and vertical mixing or diffusion. But, results from the model, proposed here, bring no new information on these different processes. No information or hypothesis is given to explain high DON concentrations ($>7\mu\text{moles.l}^{-1}$): High DON release due to grazing or low bacterial degradation rates? And what about the chemical composition of the DON pool? What is the part of the labile fraction? Is the model can help us to better understand DON dynamics and to better define sources and sinks ?

Some specific points

1) Results from the model are validated with two sets of data obtained from literature and one new set for which no methodological information is available. Authors present good comparisons between observations and modelled data but some features are surprising and not really discussed: Figure 1: the model greatly underestimates (30%) the surface DON in the north of the equator. It would be interesting to test different parameters to explain this discrepancy.

Figure 2: I disagree that modeled DON is consistent with observation as argued by the authors. Raimbault et al. (1999), in spite of very low spatial resolution, have found a clear DON accumulation in surface water between 7° and 11° S, which is not visible on figure 2C. More, high DON concentration ($>6\mu\text{moles.l}^{-1}$) near the equator are observed below the photic zone, not in surface s expected by the model. These dis-

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crepancies are not trivial, because new results from model could dramatically modify previous interpretation on nitrogen dynamics. PON shows similar spatial and temporal variations than iron (although it is not easily readable on the figures 10). I think this relationship would be the same with nitrate. But what can we conclude?

I suggest to the authors to more deeply discuss differences between model and observations and to examine some interpretations.

2) In this equatorial region, silicate can be a limited factor, especially for large diatoms cells, and can control PON and DON formation. Why this parameter was not included in the model?

3) Biological nitrogen fixation seems to be no negligible in the north part of the studied area (between 8 and 20° N and 110-150° W, see Deutsch et al., 2007) and can be an important source of new PON and DON. Can you include this process in the model and then reevaluate PON production, especially in N-deficient waters along the HNLC front.

4) The authors present the DON pool as a homogenous pool whatever the geographical localisation and the concentration. They conclude that spatial and temporal variations were weak (less than a factor 2). But, the conclusion could be totally different when considering the labile or semi-labile fractions, which are important links between autotrophs and heterotrophs. Then, it would be more useful to separate DON pool into semilabile and refractory pools (See (Roussenov et al., 2006) to bring new insights. Because the primordial questions are: the fate of excess DON, its resident time and its quantitative role in export production?

5) Page 3272: what is the method used to collect the recent observations (2005-2006; figures 3 and 11). Are they comparable with those of Raimbault et al. (1999) and Libby and Wheeler (1997).

6) Observed PON are generally lower than modeled PON (see figures 1 and 11). Au-

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thors suspect the role of zooplankton, but I don't understand how the model is corrected to obtain a better agreement. On the other and, authors don't take into account the fact that observed PON measured on GF/F filters were certainly underestimated (Libby and Wheeler, 1997; Raimbault et al., 1999). A great part of PON (up to 60%) has been found to pass through GF/F filters and collected on 0.2 μ m membranes. This aspect also needs to be included in the discussion.

7) Finally, I am not sure that the relationships found between DON and zooplankton or phytoplankton, are sufficient to include "biological regulation" in the title.

In conclusion this paper addresses relevant scientific questions within the scope of BG. The overall presentation is well structured and clear. The scientific methods and assumptions are valid and clearly outlined but could be improved to support substantial interpretations and conclusions. Finally, this paper does not present novel concepts as its stands.

Interactive comment on Biogeosciences Discuss., 5, 3267, 2008.

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