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Biogeosciences Discussions



# *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.

**X. J. Wang et al.**

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Response to referee #2

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 iden-

tifying the role of DON has proved difficult through the paucity of direct observations, 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 (>7μmoles.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?

>>We agree that these issues, e.g., export of DON and basin scale transport, are important for understanding N dynamics. This manuscript focuses on the distributions of DON and PON, addressing the issue raised previously, viz., "we have only a cursory knowledge of the distributions of dissolved and particulate organic nitrogen" in the equatorial Pacific (Libby and Wheeler, 1997). Because there are large spatial and temporal variations in many physical and biogeochemical fields in the equatorial Pacific, we first need to know if DON and PON have similar variations. There is little information in the literature in this regard. It seems that some sentences were not clear enough to describe the purpose and objective of this study. We have reworded the text to remove any potential ambiguities.

Some specific points



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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.

>>There were two small sections (i.e., 2.2 In situ PON data and 2.3 Other in situ data) that were lost during technical formatting. We have added them back.

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.

>>The modeled DON is approximate 2 mmol m-3 lower than the observed in the north of the equator for the period of Sep, 1992. While it may be possible to further tune the model to match the DON observations for this period, we anticipate that potential errors and uncertainties associated with other factors (e.g., forcing fields and physical processes) may also contribute to the mis-matches. Moreover, field measurements often represent some events occurring at much shorter time scales which can range from less than one hour to several days whereas our model simulations represent the mean conditions for a single month. Nevertheless, we have indeed tested different parameters with many sensitivity studies. Our choice of parameters is based on integrated and overall model validations, including not only many components and processes (e.g., chl., nitrate, ammonium, DON, PON, primary production, nitrate and ammonium uptake and regeneration), but also spatial (vertical, meridional and zonal) and temporal variability (seasonal to interannual time scales).

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 μmoles.l-1) near the equator are observed below the photic zone, not in surface s expected by the model. These discrepancies are not trivial, because new results from model could dramatically modify



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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.

>>We have reworded as "Modeled DON and PON show relatively uniform distributions in the upper 100 m, which is similar to the observations". Models may not be coherent with observations at small scales (see responses above).

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?

>>We have considered including silicate in the model. Since there are many processes and parameters that should be included, we can only do one or two at a time. Simulating Si cycle would be our next step for model development. Moreover, while adding new variables to the model is easily accomplished, constraining them based on observations is a much more onerous task and we are interested in the latter as much as the former.

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 revaluate PON production, especially in N-deficient waters along the HNLC front.

>>Indeed, nitrogen fixation can be important in the N-depleted waters. But this model is mainly for the tropical regions, particularly the HNLC region. So we have to put our efforts on the HNLC dynamics first.

4) The authors present the DON pool as a homogenous pool whatever the geographical



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localisation and the concentration. They conclude that spatial and temporal variations were weak (less tan 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 semilable 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?

>>We agree that it would be useful to separate DON pool into semi-labile and refractory pools. We believe that further model development and studies of the fate of excess DON, its resident time and its quantitative role in export production will bring new insights. We appreciate the referee's constructive comments and suggestion, and have a plan to address these issues in future work.

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).

>>We have added the method with a reference.

6) Observed PON are generally lower than modeled PON (see figures 1 and 11). Authors 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 $\mu$m membranes. This aspect also needs to be included in the discussion.

>>We do not correct any parameter in the model simulation. While we suspect the role of zooplankton, we do not believe that zooplankton alone can explain everything. The difference in 0.7 $\mu$m PON and 0.2 $\mu$m PON is pointed out in the section 2.3. We have included this aspect (i.e., underestimation of in situ PON due to GF/F filters) in



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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.

>>We have changed the last part of the title as "biological and physical influences".

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.

>>We appreciate the referee's constructive comments. We have revised our manuscript to meet the criticism and to improve the presentation.

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