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Biogeosciences Discussions

Interactive comment on "Importance of dissolved organic nitrogen in the North Atlantic Oceanin sustaining primary production: a 3-D modelling approach" *by* G. Charria et al.

G. Charria et al.

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Anonymous Referee 1

1. General Comments:

The manuscript studies the impact of transport of semi-labile dissolved organic matter for primary production in the North Atlantic Ocean using a 3D-biogeochemical model of intermediate complexity. From their study the authors conclude that lateral supply of DON might be important in closing the N-budget over the Atlantic Ocean and sustaining primary production in the oligotrophic gyre.

The study builds on earlier, well recognized, work by the Toulouse modelling group and gets off to study in detail the relevance of DON transport for primary production distribution in the North Atlantic, in particular PP in the subtropical gyre, and the transport processes and source regions of DON. The importance of DON transport had been suggested earlier (papers by Ric Williams and Mick Follows and co-workers), but this study provides an important addition to this discussion. The paper convinces with a very detailed (almost complete, but see below) model-data comparison and a sensitivity study of relevant parameters for DON dynamics.
I suggest publication in BG after moderate improvements.

Major critics:

For a paper concerned with the importance of DON transports for production, I missed a more detailed comparison with DON data. As the manuscript stands, the comparison is just some 10 lines (section 4.4) and no figures.

I am aware of the problem that DON data are still not very abundant, however, there are several published data sets, f.e. in Vidal et al., 1999; 2003; Kähler and Koeve, 2001, Roussenov et al. 2005, Knapp et al. 2005; likely others as well, please search the literature). This lack of a, in the context of this paper most needed, quantitative data-model comparison is surprising. The authors conclude that DON transport is a significant nitrogen flux in their model NA, they demonstrate this f.e. by showing various DON transformations and fluxes (Fig. 9a, c, 10, 11), however they don't even show the model distribution of (semilabile) DON, nor data-model comparison. Providing this material is significant to support the manuscript's conclusions on the importance of DON transport, relative to other processes. I strongly suggest to add this material.

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So far, the DON transport hypothesis is mainly supported by evaluating the physical aspect of transport as such, i.e. through analysing the distribution of T, S etc. it is confirmed that water transports are reasonable, to the extend that distributions of T and S can judge this. The transport of a tracer, however, is given by the physical transport combined with the tracer distribution, I regard it fundamental to provide an analysis of the tracer distribution (model data).
In fact there are some aspects of the paper, f.e. the mismatch of observed, satellite and modelled higher latitude (ARCT, SARC, NADR) primary production, which are largely unexplained. This model feature could be due to too vigorous export of semilabile DON from nutrient rich parts of the North Atlantic, an analysis of the regional DON distribution could help to exclude this possibility.

We agree with Referee 1. Indeed, the comparisons with DON data have not been detailed in the manuscript for two main reasons. First, the available DON data are not representing in situ measurements for the year 1998 studied in our paper. For example, Vidal et al. (1999) is describing a cruise in 1995, Kähler and Koeve (2001) a cruise in 1996, and Knapp et al. (2005) a time series at BATS in 2000/2001. Another reason of the lack of quantitative data-model comparison is that we only model the semi-labile DON. DON measurements are generally total dissolved organic nitrogen, including the refractory fraction. The concentrations of refractory dissolved organic nitrogen can be estimated from deeper profiles. The value obtained at depth can be subtracted to deduce a labile and semi-labile DON profile (the labile fraction can be neglected because this DON fraction is quickly consumed).
However, to improve the presentation and the validation of DON fields, we added figures and qualitative comparisons between modelled DON in 1998 and observed DON for other years. The section 4.4 (section 3.4 in this revised version) was rewritten and the publications mentioned by Referee 1 are now discussed.

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2. Specific Comments:

Introduction (section 1):

Sources of primary production in the subtropical gyre are discussed (DIN-, DON transport, N2-fixation). The major source of N (80-90%) of oligotrophic primary production, however, is NH4, being recycled by zooplankton (and bacteria). Either this should be mentioned clearly, or the introduction should be re-written to discuss nutrients sustaining new/export production, I guess this is what the authors like to refer to, hence relevant references are missing and need to be added as well (f.e. work of Jenkins). - Also atmospheric fluxed might be worth mentioning as a potential N source to oligotrophic waters, see Duce et al., 2008, Science, for a recent review.

In the introduction, the sources of nutrients, including NH4, in the subtropical gyre are now discussed. The potential atmospheric N sources to oligotrophic waters are also discussed and the reference of Duce et al. (2008) publication has been added in the manuscript.

Material and Methods (section 2):

Add a table with source-minus-sinks (SMS) equations, please. In particular in view of the open access character of BG and the fact that the paper you refer to as source for SMS equations is not open access. Also the paper by Huret et al. 2005 is in sigma co-ordinates and this one is in Z co-ordinates.

The SMS equations have been added in an appendix (Appendix A).

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p1731, l 12: Regarding the parameters chosen (see also remarks to Table 1) the manuscript refers to Huret (2005) and Oschlies and Garcon, 1999, and concerning this implementation to the thesis of the 1st author. Though details may be in any of the two thesis mentioned, I would like to see some mentioning of the physiological significance of the chosen parameters.

Most of parameters have been deduced from Oschlies and Garçon (1999) and Huret et al. (2005). A few adjustments for hydrolysis and remineralization rate have been performed following model-data comparisons. A preliminary sensitivity study and data comparison have been performed and led to new values for phytoplankton exudation, remineralisation and hydrolysis rates (Charria, 2005). The adjusted values for these three parameters are within the range of possible values estimated from the literature in the subtropical gyre of the North Atlantic Ocean (i.e. Dadou et al., 2004; Salihoglu et al., 2008).
References for the parameter values have been added in Table 1.

Model-data comparison (section 4):

Section 4.2 (Nitrate and chl-a): Given the huge model-data mean difference in nitrate at BATS (3.3 mmol m-3), which is explained by weak representation of the western boundary current, is the use of this station for biogeochemical model-data comparison in the context of this paper meaningful? I expect that in the model the BATS location is not an oligotrophic station at all, right? This leads to the general problem that for explicit stations or sections (see f.e. also text on AMT6, 36.6N) improper representation of the circulation field translate into huge differences for nutrients (and chl-a). So how to deal with this general problem? The authors could perhaps discuss this

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methodological aspect of data-model comparison a bit further.

Indeed, large differences in model-data comparisons are often related to physical dynamics (i.e. boundary currents, water masses). Consequently, for example, the BATS station does not correspond to an oligotrophic station in the modelled fields. The general problem for model validation is that we do not have many observations, even in the well sampled North Atlantic Ocean. Especially in 1998, the choice of cruises or time series is limited. We then need to compare with all available data. This approach is useful for demonstrating the biases in the physical model and for quantifying the impact of such biases on biogeochemical fields.
Following Referee 1 suggestions, we have included a comment on this methodological aspect of model-data comparison at the end of section 3.2 in the manuscript (p. 1737 l. 2 in the submitted version).

Section 4.4 (DON at BATS and EUMELI): I miss some details here. Please give references for the DON data used. In M+M you give Steinberg et al. for BATS, certainly not appropriate for BATS DON. The same for EUMELI, the reference to the Morel et al. paper does not provide a reference to a description of the DON data from EUMELI. More importantly, it needs to be explained how the refractory fractions at both stations (and AMT) are identified. Reading section 4.4 I learn only about differences between model and data and the standard deviations of both. No details concerning the absolute concentrations of semilabile DON, except for the AMT transect for which a range is given, are provided. As mentioned above, a more detailed evaluation and comparison with data is needed for semilablile DON.

We agree with Referee 1 that references were not describing DON data. Indeed, we had chosen to keep the general references written in English for the description of the different biogeochemical variables at BATS and EUMELI stations. More precise

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references concerning DON: Knapp et al. (2005), and Salihoglu et al. (2008) for DON at BATS, and Pujo-Pay and Raimbault (1994); Pujo-Pay (1995) for DON at EUMELI have been added in section 3.4.
As mentioned above, the section 4.4 (3.4 in the present revised version) has been extended to detail and evaluate, by model-data comparisons, the magnitude and spatial distribution of the modelled DON concentrations (assuming a known refractory DON concentration).

p 1737/1738, discussion of Table 2: see my general remark on Table 2 (below); also the text is weak here: 'f.e. BF97 is based on 1971 to 1994 measurements, AM96 on 1978-1986 data', etc.; this is partly wrong partly, difficult to understand.

The Table 2 has been corrected following Referee 1's comments.

Role of DON (section 6)

The model does not differentiate between NO3 and NH4 uptake (Figure 1). In oligotrophic regions, however, tracer uptake experiments have indicated that 90% of inorganic nutrient uptake is sustained by NH4, where the implicit assumption was that this NH4 is locally regenerated by zooplankton. Model results suggest that nitrogen fluxes from semilabile DON to DIN (not NO3, as indicated in figure captions of 9a,b) clearly dominate zooplankton excretion (ZOO to NH4 in reality, ZOO to DIN in the model) by up to one order of magnitude. This questions either the concept of the dominance of locally regenerated nutrients (on time scales of days) for primary production in oligotrophic waters, or the concept of semilabile DON. The authors should clearly state this and discuss it further.

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Indeed, nitrogen fluxes from DON to DIN dominate ZOO to DIN nitrogen fluxes. Our results are in agreement with similar model studies as Huret et al. (2005) where ZOO to DIN flux represent 2645 10^6 molN and DON to DIN dominates with 19400 10^6 molN. In Dadou et al. (2004), ZOO to DIN fluxes are almost equal to DON to DIN fluxes. As mentioned in Salihoglu et al. (2008), large amounts of nutrient can be made available in the upper ocean by rapid cycling of dissolved organic matter released by a variety of processes including phytoplankton exudation, bacterial release, viral lysis, zooplankton excretion and grazing. Then, the role of locally generated nutrients for primary production through semi-labile DON can be important (i.e. Salihoglu et al., 2008).
The figures 9a, b titles have been corrected.

Sensitivity study for DON (section 5):

Two remarks. First, only a comparison with the standard model is given in Fig. 8. Thus we learn about the strength of the varied parameters, but not whether increasing (decreasing) one parameter improves the model with respect to the DON data distribution (or other data distributions). (Again the suggestion for more model-data comparison for DON data.) Second, an additional experiment excluding all DON related processes could be informative: how worse do nitrate and chl-a fields get, compared to observations?

The sensitivity experiments have been performed from a simulation which has been previously optimized to produce results as close as possible to observations using these biogeochemical equations. We did not detail the tuning process in the manuscript but based on sensitivity experiments and model-data comparisons, the initial set of parameters has been adjusted. Then in this section of the manuscript, new sensitivity experiments have been performed to better understand the influence

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of the parameter values associated with the DON processes on the different states variables of the biogeochemical model and not to improve our simulations.

Concerning the second remark, we agree with Referee 1 that an additional experiment excluding all DON related processes could be informative. However, as the project had been finished, we do not have available computing time to run this experiment.

2. Misc:

1730, l 10, give proper scientific reference for the MERCATOR project, web site references are fluent and should only be given in addition to scientific references

The following scientific reference has been added: Etienne, H., and Benkiran, M. : Multivariate assimilation in MERCATOR project : New statistical parameters from forecast error estimation, J. Marine Syst., 65, 430-449, 2007.

1731, l 4-6: please give a reference for the concept on refractory, semi labile, labile DON; f.e. Anderson Williams 1999 (GBC), or references given therein (their introduction).

The references Kirchman et al. (1993) and Carlson and Ducklow (1995) were added in the manuscript.

1731, l 7; I think the reference of Huret et al. 2005 is misleading here, Huret did not work with the MNATL, right?

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M. Huret did not work with the MNATL physical model but with the NPZDDON biogeochemical model. We agree with Referee 1 about the confusing sentence. The sentence was slightly rewritten as follows: "This biogeochemical model (see Huret et al., 2005, for a detailed description of the model) is coupled with the MNATL circulation model previously described."

1731. l 23: 'is nutrient limited', be more specific here, please

The text was slightly modified to be more specific as follows:
"If phytoplankton growth is nutrient limited (low DIN concentrations),..."

1732, l 5: how is the initial DON concentration (3 mmol N m-3) justified

The semi-labile DON concentration at surface is justified by an average value of available observations (BATS, EUMELI, AMT).

1733, l 26: correlation coefficient 'is above 90%'; correlation coefficients are between 0 and 1

We agree with Referee 1. "90%" was replaced by "0.9".

1741, l 8: 'let's examine': slang!

We agree with Referee 1. We replaced "let's examine" by "We examine".

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1741/2: 'This analysis showed that ... (not shown).' a little awkward formulation

We agree with Referee 1. The sentence was rewritten: "This analysis confirmed that DON plays an important role in less productive regions with respect to high productive regions (not shown), in agreement with the study from Gunson et al. (1999)."

Tables/Figure: [Please note that I refer to the printer-friendly version when commenting on quality of figures. There is a tendency with BG manuscripts to present very tiny graphics (graphics with tiny details, captions, lables, etc.), for which details can only be seen on the screen after zooming in. As many people still prefer to read printed papers, I suggest to stick to reasonable quality levels also for these.] In general figure of this manuscript are characterized by often tiny labels and partly (Fig. 8, 9) in-figure captions that duplicate information given in the legend. Work is needed to optimize figures for print and screen reading. Please check carefully.

As recommended by Referee1, the label and symbols for figures 8, 9 as well as for figures 3,6 are now optimized for print and screen reading.

Table 1: Comparing with Huret, I find that most of the parameters relevant to DON SMS are different from the 2005 paper. Please give a short justification and provide references for all chosen parameter values, if possible pointing to experimental work supporting your choices (i.e. physiological meaningfullness).

We added the references for all chosen parameter values.

Table 2: The column 'JGOFS' refers to data published in Ducklow (2003), however,

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checking his Table 1.4 I find that most of the numbers cited in this manuscript are from the 'Original estimates' column, which gives numbers as estimated by Longhurst et al. 1995, and are NOT based on JGOFS data. (Exception is for NASW.) Please correct.

We agree with Referee 1. Indeed, in Ducklow's (2003) publication,we considered the JGOFS estimations when they were available per meter square (for NASW) and the Longhurst et al. (1995) estimations for the other provinces. To address this comment, we slightly modified the text and considered all values from Longhurst et al. (1995).

Fig. 2. Datasets 1-4 share the same symbol and are differentiated by different colours. F.e. red-green bad eyesighted people will not be able to follow. Use clear symbols / or show cruise tracks as lines with cruise identifiers shown in the figure.

Following Referee 1 comments, the figure 2 has been modified.

Figs. (2), 3 6 could benefit from choosing identical symbols (colors), to the extend possible. Please optimize.

The symbols in these figures have been optimized.

Fig.3: 50% of the symbols are basically invisible in the printout due to wrong colour and small symbols. Please improve.

In Figure 3, the size of the symbols has been increased and colours have been modified to improve the reading.

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Fig. 4, nitrate panel: 'Colour' scale can be optimized by inverting the scale and also isolines to highlighting the 0-10 uM range (or so).

Figure 4 has been adjusted.

Fig. 5, nitrate panel: similar to Fig. 4; also indicate month of observation in the legend

Figure 5 and its caption have been modified.

Fig. 6, like Fig. 3, some of the symbols are basically invisible in the printout

Same improvements as in Figure 3 have been applied in Figure 6.

Fig. 8: Some of the text in the figure is basically too tiny to read, f.e. x-, y-axes labels. This is obviously due to the way BG organizes figures (one figure per half page). Having five Taylor plots and an extensive legend makes up a figure which ends up being very much useless. BG needs to have more flexibility here! Unless this is not possible, however, it is in the responsibility of the authors to make sure that the reader can access the full information from the printed figure.

Symbols could be better explained (what is MUD? one has to guess; please use the same terms as in Table 3) . Also the small texts in the figure (f.e. a) Sensitivity Run for DIN surface concentrations; etc.) are repetitions of the information given in the legend, they use lab slang (P, Z, DIN, D, and partly are tiny 'eye powder'. Leave out details, just give a), b) etc.

Plot radials, like in Fig. 3

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Figure 8 has been improved following referee advices.

Fig. 9: again tiny labels (units, captions)

Figure 9 has been improved.

4. Language:

I (not a native English speaker myself) sometime found awkward expressions, some of which sound a little French English. Please check carefully and consult adequate advice. Examples, which I picked, not complete:

Leg. Fig. 3: "proportional to their distance apart"

The text in Figure 3 caption was replaced by: "The green lines measure the distance from the reference point and indicate the RMS error (once any overall bias has been removed; see the mathematical expression in section 2.2)."

Leg. Fig 8: "simulation of reference"

"Simulation of reference" was replaced by the "unperturbed simulation".

1729, l 4/5; "extension of the gyre is important", you mean that N fluxes in the gyre can be quantitatively relevant due to the large extension of the gyre, right? improve, please

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We modified the text to clearly state that the total primary production (gC) in the gyres could be important due to the large extension of the gyre even if the primary production (gCm^{-2}) is low.

1730. l 18, lie? I think 'are lying' is more appropriate

We agree with Referee 1 and we corrected the text.

Interactive comment on Biogeosciences Discuss., 5, 1727, 2008.