Biogeosciences Discuss., 5, S712–S717, 2008
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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.

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Received and published: 2 June 2008

1 General Comments

The manuscript deals with the production and fate of the semi-labile pool of dissolved organic nitrogen (DON) in the North Atlantic Ocean. In their study, the authors investigate different mechanisms that contribute to the meridional transport of DON, while focusing on primary production within the subtropical gyre. As they apply a coupled physical-, biogeochemical model, it is shown that production, transport, and hydrolysis of DON can promote primary production substantially, in particular in oligotrophic

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Interactive Discussion

Discussion Paper

regions of the subtropical gyre. The authors stress that their results are in support of conclusions inferred from Roussenov et al. (2006).

This study is of general interest and is well documented, because it not only provides details on the model configuration but also includes a sensitivity analysis and an extensive data-model comparison. The inference made according to their model simulations, however, relies on a single biological flux approximation, where zooplankton seems to be the dominant and main regulating compartment. Therefore, interested readers may suspect that the role of zooplankton "excretion" (as termed in the manuscript) is over-interpreted. It might well be that micro-zooplankton and the microbial loop are indeed important here, but the authors should then discuss to which extent their specific solution can be justified. Such discussion could be of great value. Surprisingly, the authors refer in their discussion to studies that are actually based on alternative organic matter pathways, where primary sources of DON are rather cell lysis, exudation by phytoplankton, and remineralisation of sinking particles (as cited in the paper: Huret et al., 2005; Salihoglu et al., 2008; Roussenov et al., 2006) than zooplankton excretion. Being more critical on this issue will help to improve the manuscript, as is required that the authors come up with a conclusion.

2 Specific Comments

The written text is comprehensible and readers will get the general impression that the numerical simulations were performed with great care. Few paragraphs and sentences need to be rewritten, in order to either improve the grammar or to clarify statements made by the authors. In the following I will only focus on prevailing issues that need to be addressed by the authors.

2.1 Introduction

Four mechanisms are listed that can sustain primary production in the subtropical gyre. Yet, the relative contributions of each mechanism remain uncertain. The authors would do better if they could bring forward their idea and how it relates to those mechanisms listed. The central questions addressed in the paper are vague: To which extent model parameter values affect modelled surface concentrations of DON is not a general scientific question but a substantial part of a good model analysis. On the other hand, to specify the sources of the available DON to sustain primary production is substantial and of overall interest, but this modelling study does not provide a unique answer to this question.

2.2 Methodology

The model of Huret et al. (2005) is an extension of Oschlies and Garon (1999) (OG99). Whether it captures "all essential biogeochemical features in the North Atlantic Ocean (e.g. the spring bloom,... , exported production)" is not shown and therefore no good justification. It is sufficient to state that the chosen model is an extended OG99 version that explicitly resolves DON, as successfully applied in Huret et al. (2005) for simulations in an estuary.

p.1731 l2: Particulate Organic Nitrogen (PON) is more than just the detritus compartment.

p. 1731 l11: Why are the parameter values deduced from OG99? In Huret et al. (2005) the maximum grazing rate was drastically reduced to g = 0.75 d^{-1}. Why not here? Choosing over the maximum grazing rate has strong implications. Using g = 2.0 d^{-1}, as in OG99, most likely induces a large standing stock of zooplankton, which in turn is responsible for the large flux of nitrogen via *excretion*;. A large standing stock

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in zooplankton also enhances the flux to detritus in the model. As a consequence, the impact of detritus, sustained by the mass flux from zooplankton, on the model's DON concentration is not a finding, as highlighted in the paper, but a prior assumption that comes with the parameter choice. Table 1 does not include hydrolysis (μ_d) and zooplankton mortality should be μ_z as in Figure 1.

p. 1731 l15: nitrogen units

p.1731 l23: How can the modelled phytoplankton growth be adjusted under nutrient limitation to become equally limited by light and nutrients? I thought that Chl*a* is a diagnostic variable here, which is calculated from phytoplankton nitrogen in the model.

2.3 Data used

Measurements of particulate organic nitrogen (PON) would have been of help (e.g. PON:DON ratios along the transects).

2.4 Model-data comparisons

I appreciate the information of the Taylor diagrams, but I would prefer to see more transects, as in Figure 4. However, model-data comparison of Figure 4 is impressive.

p.1735 l12: How can the modelled fields be fresher and colder because of the northern position of the Gulf Stream current. I would expect the opposite, saltier and warmer conditions north of 36^{o}N. In case of the Mode water formation, then I would expect that the bias (fresher and colder) is restricted to the subtropical gyre only, but Figure 4 shows higher surface temperature in model results compared to observations.

p.1737 l2: "The model slightly overestimates chlorophyll..."; this is slightly understated.

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p.1737 Figure7: Why are the modelled surface chlorophyll concentrations south of 36^{o}N still greater than 1 μg l^{-1}? The Gulf Stream is shifted towards north whereas primary production rates and thus higher chlorophyll concentrations are significantly shifted towards the south (if compared with satellite images). I have the impression that this is related to presumptions made in the biological model (e.g. high grazing rates that in turn fuel primary production).

2.5 Sensitivity studies for dissolved organic nitrogen

This section needs to become more precise on how many simulations were done and how parameters were varied. p.1739 l13: "We arbitrary change the parameter values..." How? What is the underlying error distribution?

The authors have chosen those parameters for variation that are directly linked to the DON compartment. This is insufficient, since the DON net gain or loss does depend on the standing stocks in phytoplankton, zooplankton, and detritus as well. As stated before, I presume that the model results come with high zooplankton concentrations, or at least with a high throughflow through zooplankton to detritus. Then, modelled DON concentrations are hardly sensitive to variation of f_2 (organic fraction of zooplankton excretion), but would be highly sensitive to variations of f_1 (the assimilation efficiency of zooplankton) or g (the maximum grazing rate). There is only very little to learn from the sensitivity analysis here, since it is performed in a parameter space where the dominant link between zooplankton, detritus biomass and DON is a strong prior assumption. I am not saying that this prior is wrong, but to my knowledge there is no consensus on this issue. Thus, it would be more appealing to see results of a sensitivity analysis where different biological pathways are investigated and where the results are directly related to the assumptions made in Roussenov et al. (2006) and in Salihoglu et al. (2008). The overall picture would be clearer if PON data were included to the analysis, for instance by looking at the PON:DON ratios in the model. It could well be that the model results shown come along with high values in PON (phytoplankton+zooplankton+detritus, not shown), which needs to be discussed by the authors.

p.1743 l14 through p.1744 l17: The comparison with AMT10 (agreement with Mahaffey et al., 2004) and stressing that the heterogeneous zonal distribution is important is a highlight of the manuscript. The issue of how representative certain transects are for constraining the entire PON and DON overturning can be worked out in greater detail, since I believe that it is of great interest.

In the end I would suggest to the authors to recall those questions posed in the introduction section and then write a conclusion paragraph that includes the answers.

3 Technical corrections

Most units in the manuscript are written with dots in between (mmol.m^{-1}.s^{-1}). Please correct units.

Table 1 does not include hydrolysis (μ_d) and zooplankton mortality should be μ_z as in Figure 1.

p.1731 l2: Particulate Organic Nitrogen (PON) in this model is detritus + phytoplankton + zooplankton.

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

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