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

G. Charria et al.

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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 regions of the subtropical gyre. The authors stress that their results are in

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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 overinterpreted. 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.

Indeed, zooplankton compartment is playing a central role in the biogeochemical model as a closing compartment. However, references discussed in the manuscript are describing similar organic matter pathways because our primary source of DON is the remineralisation of sinking particles produced by zooplankton and phytoplankton (as in Huret et al., 2005) and exudation by phytoplankton and zooplankton excretion are secondary weaker DON sources.

2. Specific Comments

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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.

We agree with Referee 2 and we have adjusted the main questions in the introduction. We now focused our main questions around: the maintain of Dissolved Organic Matter pool and the role of meridional Dissolved Organic Matter transport to sustain Primary Production in the subtropical gyre of the North Atlantic ocean.

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

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(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.

We agree with Referee 2 and we followed his suggestions.

p.1731 I2: Particulate Organic Nitrogen (PON) is more than just the detritus compartment.

We fully agree with Referee 2 and we corrected it.

p. 1731 I11: Why are the parameter values deduced from OG99? In Huret et al. (2005) the maximum grazing rate was drastically reduced to $g = 0.75 \text{ d}^{-1}$. Why not here? Choosing over the maximum grazing rate has strong implications. Using $g = 2.0 \text{ d}^{-1}$, 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 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.

The parameter values have been adjusted from sensitivity experiments at basin scale with model-data comparisons. After several experiments, we have deduced that OG99 parameters are more adapted to reproduce North Atlantic biogeochemical conditions. The parameters values used in Huret et al. (2005) come from OG99

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but some parameter values (for example maximum grazing rate called g) have been modified from model-data comparisons for the application in the Rio de La Plata estuary. We agree with Referee 1 that the grazing rate implies that the standing stock of zooplankton will be increased. However, a weaker grazing rate as in Huret et al. (2005) will induce an unrealistic large phytoplankton biomass in North Atlantic (we controlled it).

We agree that the impact of detritus, sustained by the mass flux from zooplankton, is a consequence of the standing stock of zooplankton. However, weaker grazing rate will not change the importance of zooplankton to detritus flux compared to zooplankton excretion.

μ_D has been included and μ_z has been corrected.

[p. 1731 I15: nitrogen units](#)

We made this correction.

[p.1731 I23: How can the modelled phytoplankton growth be adjusted under nutrient limitation to become equally limited by light and nutrients? I thought that Chla is a diagnostic variable here, which is calculated from phytoplankton nitrogen in the model.](#)

The modelled phytoplankton growth is under nutrient or light limitation (see the SMS terms of the biogeochemical model in a new appendix A, as suggested by the Referee 1). Indeed, Chla is a diagnostic variable. P.1731 I23, we are describing the χ coefficient which is function of limitation terms (light and nutrients). This coefficient is used to convert nitrogen phytoplankton in chlorophyll phytoplankton. Then, the phytoplankton growth is not equally limited but the value of this coefficient χ is calculated as a

function of light and nutrient limitation terms coming from the biogeochemical model. We reworded this sentence to clarify this point.

2.3 Data used

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

We agree with Referee 2 that particulate organic nitrogen data would have been of help as a complementary model-data comparison. However the only available data are from the BATS station for the year 1998.

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°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.

Indeed, due to the northern position of the Gulf Stream, we would expect saltier and warmer conditions. In the manuscript, we have made confusion between two aspects of the simulation: the northern position of the Gulf Stream and the fresher and colder trends in the model simulations. The text, p. 1735, l.12 has been modified as follows:

"The model is able to reproduce the large-scale features of the temperature, salinity and density fields; however the modelled fields are generally fresher and colder than the observations."

The northern position of the Gulf Stream, a well-known bias in this kind of models is coming from several sources (i.e. western boundary condition, interaction with bathymetry).

At the opposite, the trends in temperature (colder) and salinity (fresher) are more related to small errors in atmospheric forcings, slight biases in water masses formation and properties (i.e. Subtropical Mode Waters, Mediterranean waters).

[p.1737 I2: "The model slightly overestimates chlorophyll..."; this is slightly understated.](#)

We agree with Referee 2 and removed "slightly".

[p.1737 Figure7: Why are the modelled surface chlorophyll concentrations south of 36°N still greater than 1 \$\mu\text{g/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 \(e.g. high grazing rates that in turn fuel primary production\).](#)

The large concentrations south of 36°N have been diagnosed as coming from the dissolved inorganic nitrogen concentrations which are not limiting enough the phytoplankton growth. These higher concentrations are related to a deeper vertical mixing in winter and a bad representation of the low nutrient content of the subtropical mode water in the simulation compared to observations.

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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?

To perform these sensitivity analyses, we have changed the parameter values assuming different percentages of error (+100%, -100%, -50%) on the initial value of the parameters in the biogeochemical model. The costs in computing and memory have imposed the limited number of experiments. So we did not choose the different perturbed values of the parameters using a complex mathematical or statistical method.

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

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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.

We agree with Referee 2 that additional experiments, where different biological pathways would be investigated and where the results would be directly related to the assumptions made in Roussenov et al. (2006) and in Salihoglu et al. (2008), will be of great interest. However, as the project had been finished, we do not have available computing time to drive these experiments.

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.

We agree with Referee 2 that the issue of how representative certain transects are for constraining the entire PON and DON overturning is of great interest. However, this point is beyond the scope of this study. Further investigations are needed to answer this question.

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.

A conclusion paragraph, already included in the discussion has been written as a conclusion paragraph, recalling the main questions from the introduction.

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3 Technical corrections

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

We made the corrections.

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

We made the corrections.

p.1731 I2: Particulate Organic Nitrogen (PON) in this model is detritus + phytoplankton + zooplankton.

We agree with Referee 2 and we replaced PON by detritus.

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