

## ***Interactive comment on “Regional differences in modelled net production and shallow remineralization in the North Atlantic subtropical Gyre” by B. Fernández-Castro et al.***

**B. Fernández-Castro et al.**

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General comments The manuscript by Fernandez-Castro compares surface net production and shallow remineralization rates between BATS and ESTOC, representing subtropical ocean time series stations in the western and eastern North Atlantic, respectively. Based on a dataset collected between 1996-2001 the authors assess similarities and differences between the two stations by comparing the output of a 1D model that targets a mass balance. For the BATS site a similar approach was presented by Ono et al. 2011, however, the current authors are the first to compare time series stations from a similar trophic region in the North Atlantic.

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I found the paper interesting, however, to my mind it suffers from a problem that some ‘modeling’ papers share: The information provided is too dense and some of the questions addressed are not clear to non-modeling data providing scientists. To open up the paper for an audience that is not so deep into the subject it would be nice to briefly explain some variables, why they have been introduced and the relevance of them. E.g. What does changes in K imply etc.?

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We agree with the reviewer that some parts of the manuscript may be hard to understand for a no modeller audience. However, the main goal of this study was to compute biogeochemical rates of net photosynthesis and respiration at two subtropical North Atlantic sites by using a mass balance modelling approach. This requires, in a first step, the description of the calculation of all the terms included in the model. Similar approaches have been applied to climatological data in previous studies (Ono et al., 2001; Gruber et al., 1998). We consider that a detailed explanation of the meaning and relevance of terms that are frequently used in modelling studies would involve a substantial increase in the extension of the paper, and would be out of the scope of this study.

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I miss a clearer mission statement in the introduction. Why has the study been done apart from elucidating differences or similarities in production and remineralization?

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The main goal of this study was to compute biogeochemical estimates of net production and shallow remineralization in order to verify if differences in the synthesis and consumption of organic matter could explain the lower export rates of particulate organic carbon observed at ESTOC. This is stated in the final paragraph of the introduction:

Page 12482, lines 9-11: "For the first time, concomitant data of tracers distribution from the BATS and ESTOC time-series sites were analysed and used to build up a 1-D tracer conservation diagnostic model. The model was used to compute net production and shallow remineralization rates at both sites. The main goal of this study was to

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verify if differences in the synthesis and consumption of organic matter are consistent with the lower export rates of particulate organic carbon observed at ESTOC."

In order to clarify our goal from the beginning, the abstract has been modified to:

"We used 5-year concomitant data of tracers distribution from the BATS (Bermuda Time-series Study) and ESTOC (European Station for Time-Series in the Ocean, Canary Islands) sites to build a 1-D tracer model conservation including horizontal advection and compute net production and shallow remineralization rates at both sites. Our main goal was to verify if differences in these rates could explain the lower export rates of particulate organic carbon observed at ESTOC."

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On page 12493, line 18 the authors mention that the model suggests exceeding production over respiration on a seasonal scale. This could serve as hook around which to discuss part of the papers content, as the problem of net heterotrophy in the subtropical regions still is not solved to its end.

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As it was mentioned above the main goal of this study was to verify if differences in the synthesis and consumption of organic matter could explain the lower export rates of particulate organic carbon observed at ESTOC. Note that similar biogeochemical approaches used to determine net production in these regions lead to the same result of net autotrophy (see table 2). We consider that the comparison with in vitro experiments involving a completely different approach and under the influence of experimental artifacts (Marañón et al., 2007; Calvo-Díaz et al., 2011) is also out of the scope of this study.

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Also I find the discussion too long. I think it should be streamlined and focus on the question addressed. One candidate for me to shorten would be part of the discussion on the Redfield ratios.

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In order to accomplish our main goal we have organized the results and discussion section as follows: 1) we first describe the vertical and temporal variability in the biological sources and sinks computed from the model, and 2) we describe and discussed computed net production and shallow remineralization rates. We consider important to keep the discussion about the Redfield ratios as they not only provide information about the stoichiometry of the synthesis and remineralization of organic matter, but rather they inform about the model functioning.

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The model for ESTOC suggest lower export production and the authors conclude that lateral advection of partly remineralized material could be the reason for this – although this has not been modeled as far as I understood. Is it possible to model this lateral transport as well and include it into the budget?

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The reviewer is right that the organic matter compartment is not included in the model. Lateral advection through the manuscript refers to lateral advection of the tracers (oxygen in this case). We have modified the abstract and several parts in the text in order to clarify this point, also considering the comments by the reviewer 1 (see the answer letter to the first reviewer).

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It is not clear to me what dataset(s) from BATS and ESTOC really have been used although the data is presented in Fig 1. I suggest including a link to the databases or references where the raw data can be found. Also I would be interested if data quality checks have been made.

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Page 12482 lines 13-18:

"Monthly climatologies were calculated for temperature, salinity, density anomaly ( $\sigma_T$ ), oxygen, dissolved inorganic carbon (DIC), nitrate (actually nitrate+nitrite) and chlorophyll *a*, using the BATS and ESTOC data collected for the period 1996-2001 (Figure

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??). BATS and ESTOC measurements were made monthly except during the spring bloom period at BATS (February-April) when biweekly samplings were conducted."

modified to:

"Monthly climatologies were calculated for temperature, salinity, density anomaly ( $\sigma_T$ ), oxygen, dissolved inorganic carbon (DIC), nitrate (actually nitrate+nitrite) and chlorophyll *a*, using the BATS and ESTOC data collected for the period 1996-2001 (Figure ??). The BATS data were obtained from the BATS Web site (<http://bats.bios.edu>). BATS and ESTOC measurements were made monthly except during the spring bloom period at BATS (February-April) when biweekly samplings were conducted. Methodologies and results have been reported earlier for BATS (Michaels and Knap, 1996; Steinberg et al., 2001) and ESTOC (Neuer et al., 2007)."

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Where does the methods section start? Please put a header at the appropriate place.

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Similar to other papers describing modelling work (Ono et al., 2001; Gruber et al., 1998; Brix et al., 2006) we decided not to follow the traditional article structure. The main goal of the paper was to build the 1D model and for this reason we describe the model implementation details in a separate and specific section.

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With what software was the model constructed and run? Are there scripts or the like available?

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The model was developed with Matlab. At the moment the scripts are not available on line but we would be happy to provide the code in case somebody requests.

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Page 12486, line 7-9: Are these reports from BATS or ESTOC or generally from the subtropical Atlantic; please specify.

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These lines:

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"The seasonal variability of the tracers considered in this study, in agreement with previous reports (Menzel and Ryther, 1959; Jenkins and Goldman, 1985; Marchal et al., 1996; González-Dávila et al., 2003; Neuer et al., 2007; Santana-Casiano et al., 2007), is consistent with the fact that synthesis of organic matter occurs in the euphotic layer and that this matter is remineralized, at least partially, in the shallow aphotic zone between 100-250 m. In order to quantify the contribution of the biological processes and the physical forcing to the observed seasonal variability of the chemical tracers, a 1D diagnostic model was implemented."

were modified to:

"The seasonal variability of the tracers considered in this study, in agreement with previous reports for BATS (Menzel and Ryther, 1959; Jenkins and Goldman, 1985; Marchal et al., 1996) and ESTOC (González-Dávila et al., 2003; Neuer et al., 2007; Santana-Casiano et al., 2007), is consistent with the fact that synthesis of organic matter occurs in the euphotic layer and that this matter is remineralized, at least partially, in the shallow aphotic zone between 100-250 m. In order to quantify the contribution of the biological processes and the physical forcing to the observed seasonal variability of the chemical tracers, a 1-D diagnostic model was implemented."

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Page 12493, line 5-19: Personally I am happier with data in tables and the description in the text. Thus I suggest to remove the data from the text and add a reference to the table. The paper is already quite long, this would shorten it at least a little.

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Following the suggestion by the reviewer 1 we have deleted tables 1 and 3 as this information is already included in figures 8 and 9.

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Page 12495, line 27: I don't think that 'fact' here is the correct word as it is based on a good part of uncertainty. I suggest removing it.

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Done

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Page 12493, line 15 and page 12496, line 14: What statistical test have been done with which software?

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In this study statistical significance was determined by comparing the overlap of mean values  $\pm$  standard deviations.

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Page 12496, line 2-3 – I did not understand the rationale why only April to December has been integrated and not the whole dataset (see also Table 1) despite the comment that this way intense winter mixing was avoided.

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Our seasonal modelling approach (and climatological data) is not able to correctly simulate intense and intermittent winter mixing events and their impact on tracer concentrations. In addition mixed-layer is deeper than 110 m between January and March, such that the MLD-110 m integrals have questionable significance. In any case, we have verified that remineralization rates do not significantly change if the whole period is used for the integration.

"Both rates were integrated between April and December in order to avoid the period of intense winter mixing"

modified to:

"Both rates were integrated between April and December in order to avoid the period of intense and intermittent winter mixing, which is not accurately simulated by the model"

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Page 12498, line 25-28: This is pure copy and paste and should be avoided.

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These lines were modified from:

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"Due to the relevance of the allochthonous sources of organic matter at ESTOC, we cannot assert that the differences in shallow remineralization computed for both stations could explain a significant fraction of the observed discrepancy in carbon export rates. Alonso-González et al. (2009) indicated that this difference could be, at least partially, explained by low sedimentation rates of the particulate organic matter in the eastern part, as slow-sinking or suspended POC does not accumulate in the sediment traps and can be laterally advected (Aristegui et al., 2009)."

to:

"Alonso-González et al. (2009) indicated that the reported differences in the carbon export rates between both stations could be, at least partially, explained by lower sedimentation rates of the particulate organic matter in the eastern part of the gyre, as slow-sinking or suspended POC does not accumulate in the sediment traps and can be laterally advected. Although organic matter was not explicitly taken into account in our model, our results support this hypothesis as they show stronger lateral transport and higher remineralization rates for oxygen at ESTOC, that can not be sustained by the sinking flux of organic matter."

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Page 12500, line 25 – I did not understand this phrase: OK, subtropical gyres might get bigger with increasing global warming?

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Spelling mistake. Sentence changed from:

"Subtropical gyres play a crucial role in the global carbon cycle due to the large extension they occupy which is expected to get large as a consequence of global warming (Polovina et al., 2008)."

to:

"Subtropical gyres play a crucial role in the global carbon cycle due to the large extension they occupy which is expected to get bigger as a consequence of global warming"

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(Polovina et al., 2008)."

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Technical corrections Page 12481, line 14: receives not receives

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Done

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Page 12484, line 25: in concert with; delete the to

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Done

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Page 12488, line 9: winter mixed layer not winter mixed later

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Done

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Page 12500, line 17: In the light not at the light

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Done

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Page 12494, line 18: . . . at least on a seasonal scale not for a seasonal scale

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This sentence: "In agreement with previous geochemical estimates (Riser and Johnson, 2008) our results indicate that in these regions, at least for a seasonal scale, photosynthesis exceeds remineralization of organic matter."

was modified to:

"In agreement with previous geochemical estimates (Riser and Johnson, 2008) our results indicate that in these regions, at least for the depths (mixed layer base to 110 m) and the time period (April - December) we considered, photosynthesis exceeds remineralization of organic matter."

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Fig 4.: Diffusivity not diffisivity

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Done

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SEE MODIFIED MANUSCRIPT IN THE SUPPLEMENTARY MATERIAL

## References

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Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/8/C6407/2012/bgd-8-C6407-2012-supplement.pdf>

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Interactive comment on Biogeosciences Discuss., 8, 12477, 2011.

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