

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

**Anonymous Referee #2**

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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.?

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? 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. 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.

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?

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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Still, in general the manuscript is well written and I also appreciate that an error assessment has been done.

### **Specific comments**

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

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

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

### *Results and discussion*

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.

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.

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

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

### *Conclusions*

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

### **Technical corrections**

Page 12481, line 14: receives not receives

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

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

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

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

Fig 4.: Diffusivity not diffusivity

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