

## ***Interactive comment on*** **“Temperature-dependence of planktonic metabolism in the Subtropical North Atlantic Ocean” by L. S. García-Corral et al.**

### **Anonymous Referee #1**

Received and published: 20 March 2014

#### General comments:

The discussion paper by Garcia-Corral et al. on the “Temperature-dependence of planktonic metabolism in the Subtropical North Atlantic Ocean” evaluates euphotic zone gross primary production, community respiration and net community production and the dependence of these parameters on the predicted temperature increase due to climate change.

The study is based on 3 legs, each crossing several biogeographic provinces of the subtropical North Atlantic in a band between 10°N and 40°N, thus the data density is relatively high. For me not very surprising the authors find an increase in commu-

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nity respiration rates and gross primary production with temperature. Part of the news seems to be that the investigated subtropical gyres were mainly autotrophic not heterotrophic as has been shown by some of the authors on the paper in the past. In conclusion they argue that increased respiration might lead to a positive feedback concerning the air-sea carbon dioxide exchange and might also lead to more heterotrophy in the surface subtropical ocean.

While the above arguments might be true, I am not convinced that the data presented is able to suggest such conclusions. The main reason being the missing explanation on how the data were treated for statistical analysis and the data presentation itself. Much of the results and discussion seems to deal with chlorophyll a concentrations, which to my mind is not substantially adding to verify the conclusions reached. On the other hand the important data GPP, CR, NCP etc. are not sufficiently discussed in the context of the temperature dependence.

#### Specific comments:

##### Introduction:

Page 3243, line 21-22: What do you mean by stating “the metabolic balance of plankton communities is ...”?

##### Methods:

I feel that the methods section needs to be rearranged. I would place the paragraph on measuring chl a concentration after the section “study area”, which I would call sampling sites and study area.

Somewhere in the paragraph on the community metabolism I would expect a better description of the Winkler approach. Particularly I miss the mention of the chemicals used. I could not see what the use of the satellite derived data for the study is, so I would delete this paragraph plus the figure.

You need to add a short paragraph on data treatment and statistics (what test has

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been used under which conditions etc). Where the data generally normally distributed? Using t-tests e.g. is valid if the data are normally distributed. I also miss a note on how the integrated rates were calculated.

Page 3246, line 4: Is there a citation for the claim that the incubation bottles themselves reduce incident radiation by 8-12% or did you measure this for a batch of bottles?

Page 3247, line 22: Did you calibrate the sensor yourself (with which solution) or did the company do it?

Figure 1: Indicate the biogeographical provinces you mention in the text. A shape file can be found at <http://www.marineregions.org/sources.php> Longhurst biogeographical provinces.

Results:

Generally the results are confusing and need extensive editing. The long sentences including many brackets and detailed numbers should be shortened. See as a prime example page 3250 line 21-24. Particularly the different sampling depth should either be simplified (it does not matter whether you sampled at 23.49 m or at 23.5 m depth) or referred to in a table.

It is not adequate to use a simple t-test when comparing the means of 3 or more samples, particularly without any mention of the data distribution. I suggest you to use ANOVA analysis (if the data conforms to the assumptions) plus the appropriate post hoc tests, including a correction for multiple significance testing, to detect differences of means between Leg 1, 2 and 3.

Instead of using Leg 1, 2 and 3 it would be more intuitive to substitute these for the season names winter, spring and summer.

Figure 2 is too small, particularly the labels can hardly be read. In the text you explain temperature first, thus put temperature here as first row too. In the row showing chl a concentrations I suggest you to indicate the DCM. The legend is incomplete: what are

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the black dots and lines?

I would prefer to see GPP, CR and NCP as well as the GPP/CR ratio in a graphical form while table 1 could be showing temperature and chlorophyll a.

Page 3252, line 14: I did not understand what the use of Chl a standardized CR or GPP rates should be. A one liner in the text explaining this would be nice.

Page 3251 paragraph 2: I am sure that your data on GPP is not accurate to the second digit after the decimalpoint (including all the errors of calculation and integration). Thus I guess integers will do.

Discussion:

Page 3253, line 13-17: The authors state at length that the eastern and western basin are different to highlight the uniqueness of their western basin data. If this is so, it would be consequent to desist from comparing the overall average of chl a concentrations with and average of the eastern basin from a former study by Teira et al.

Page 3253, line 18-19: Isn't it trivial to report that chl a concentrations sampled in the oligotrophic gyre are representative of oligotrophic waters?

Page 3253, line 25: Do you mean phytoplankton community structure here? In any case, I suggest to discuss more in depth how the various factors you mention influence the variability of the measured rates.

Page 3254, line 8-24: The issue on heterotrophic versus autotrophic subtropical regions made quite some buzz in the past. Here the authors find that the subtropical Atlantic seems to be primarily autotrophic. Thus I suggest to discuss this topic in more detail, and at least speculate on the carbon sources in the western basin.

Somewhere in the methods I read about Q10 values but these are not discussed. Why?

Page 3255, line 23-29: The content of what the authors want to state is somehow clear, but this paragraph need extensive editing.

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Technical corrections:

Page 3244, line 1: Recent analysis suggested seawater temperature as a driver ...  
Page 3245, line 1: ... regional variability of plankton metabolic .... Page 3247, line 4:  
Incubation tubes were ... Page 3247, line 6: ... holding the temperature within  $\pm 0.5^{\circ}\text{C}$   
of the in situ temperature ... Page 3249, line 22: ... showed a similar range for ....  
Page 3254, line 28: ... show a clear temperature-dependence ... Page 3255, line 24:  
Something went wrong here by copy pasting.

The figures need letters to distinguish the different panels that are described in the  
legend (e.g. A, B, C)

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