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Biogeosciences Discussions

Interactive comment on "A comparative study of biological production in eastern boundary upwelling systems using an artificial neural network" *by* Z. Lachkar and N. Gruber

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We would like to thank the Reviewer 2 for his/her comments, suggestions and remarks that have helped to improve the quality of our manuscript.

Response to Reviewer's Comments:

Authors use a complex but powerful tool to analyse and compare biological production in the four major eastern boundary upwelling systems. Given the complexity of nonlinear interactions between the variables considered as drivers responsible of the Net Primary Production (NPP), the method, Self-Organizing Map and Hierarchical Agglomerative Clustering, seems to be adequate. Neverthe-

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less, results found are descriptive, i.e. cannot be used to argue that one variable "inhibits" another one. Furthermore, EKE is estimated in the region from 150km to 500km from the shore, missing the coastal strip where most of the upwelling takes places and where the highest EKE values are found. On one hand coastal altimetry data can be trusted much closer than 150km. On the other hand, methods have been proposed to merge different altimetry datasets and recover more realistic SSH data up to the coast. Therefore, I would like to recommend the article for its publication after the two issues commented above are considered.

We address both issues raised by the reviewer in detail in our responses to comments #7, #9 and #15 below.

A list of minor comments follows:

1) P. 9902 L 9: direction of alongshore winds is crucial to "enhancing NPP effect".

We added the word "equatorward" to make the direction of the wind explicit.

2) P.9903 L. 24: there are more works than those cited that compared the 4 EBUS (for example Chaingeau et al, JPO 2009 used satellite altimetry data up to the coast and compared eddy activity in the four EBUS).

We agree with the Referee #2 that there are more studies that have compared the 4 EBUS than those cited in our study. Yet, since the focus of this paper is on NPP, we decided to cite only those comparative studies that have investigated the production and chlorophyll levels in the different EBUS and their potential drivers.

3) P. 9904 L16-19: Gruber et al 2011 is now published. I suggest highlighting here that your major finding in Gruber et al 2011 is that mesoscale eddy activity may reduce NPP rather than what is expressed.

Following Referee's suggestion, we added a sentence summarizing the key result of this study (see page 4, line15). The eddy-induced NPP reduction mechanism that Gruber et al (2011) identified on the basis of numerical model simulations is discussed

BGD

8, C5038–C5045, 2011

later when we examine the potential mechanisms of the effect of eddies on NPP (see page 15, line 29 to page 16, line 4).

4) P. 9905 L 25-26: there are two Lachkar and Gruber 2011 in the references, please specify which one is cited here.

Done.

5) P. 9906 L24-26: Please precise how and with which data do you estimate the upwelling index (UWI). Upwelling might be critically underestimated if you used the monthly time series cited on L 6-7.

Since we analyze monthly data of NPP, we also use monthly averaged QuikSCAT data from JPL/PO.DAAC cited in L25-26 (page5) to estimate the upwelling index. Since our analysis is focused on relationships rather than absolute magnitude of upwelling intensity, we believe that the potential biases in QuikSCAT mentioned by Referee 2 are not an issue.

6) P. 9907 L. 10-12: The wording here is somehow confusing. Upwelling in the EBUS regions is mainly due to persistent upwelling-favorable winds, not to the current systems.

To avoid confusion and for more clarity, we substituted the term "current systems" by "upwelling systems" in this sentence.

7) L. 13-14: A large amount of EKE will not be considered by not taking into account the nearshore 150km strip. The comparison with other datasets which do include the nearshore strip does not seems too fair to me.

In the revised manuscript, we are now using the offshore region extending from 50km to 500km offshore for the EKE calculation instead of the originally considered 150km-500km strip. We still exclude the first 50km from this analysis because as already mentioned in the original version of the manuscript, the errors of altimetry data can particularly be large there. These errors are essentially associated with the tidal sig-

BGD

8, C5038–C5045, 2011

nal correction (Matsumoto et al., 2000; Volkov et al., 2007), but can also result from the land contamination in the atmospheric corrections of unresolved high-frequency signals (Volkov et al., 2007). This point is now explicitly discussed in the revised manuscript (page 7, lines 5-9).

It is worth noting that this change in the EKE calculation region from 150-500km (original manuscript) to 50-500km (revised manuscript) did not have any noticeable effect on the patterns identified with our SOM analysis. The only small change that we could notice after implementing this modification is a slight change in the distance between a couple of neurons. This has led to a very small change in the distribution of the 4 retained classes on the SOM map (see figure 8 in original vs revised manuscript). This suggests that our results regarding the potential effect of eddies on NPP are little sensitive to the definition of the offshore region boundaries.

8) L 15-17: Bathymetry does not vary with time. Please indicate how you arranged datasets to enter to the SOM algorithm.

For each of the 90 meridional bins of averaged shelf width, data were repeated over time (41 times for 41 months). This is now explicitly mentioned in the revised manuscript (see page 7, lines 10-13). See also our earlier response #1 to Referee #1.

9) Fig. 6: Distribution patterns in sub-figures b), c) and d) do not suggest being limited by the dotted line representing inhibition. At least c) and d) suggest two different linear patterns for different values of the x axis. I wonder how sensible is the SHW parameter to the 150km EKE blanked region. Is the austral-summer/boreal-winter effect taken into account? NPP has a clear seasonality which is almost out of phase following seasons according to which hemisphere is considered. Does this matter when entering the time series to the SOM algorithm?

Given the complexity of the question raised by the reviewer and the variety of aspects it touches upon, we structure our response around the 3 main points invoked by the

BGD
8, C5038–C5045, 2011

Referee:

1) On the nature of the relationship between NPP and its drivers (linear correlation vs inhibition):

While the relationship between the strength of upwelling (UWI) and NPP on the SOM can well be represented by a simple linear model, the relationship of the latter with the other drivers is non-linear. While narrow continental shelves, elevated EKE or deep mixed layers are all clearly associated with low NPP, the relationship is very weak for wide shelves, low EKE and shallow mixed layer. The relationship between these three drivers and NPP is therefore to be asymmetric. Because of this asymmetry, we believe it is more appropriate to describe these variables as limiting factors (or inhibiting factors) whose effects on NPP start gradually to be important as certain thresholds are crossed. We have adjusted our arguments slightly, emphasizing that these relationships have the nature of limiting factors, but that our analysis per se cannot positively confirm this nature.

2) How sensitive are the results to the 150km EKE blanked region?

As already discussed in our earlier response #7 above, the relationship between the different drivers and NPP illustrated in Fig 6 is largely insensitive to the extent of this "EKE blanked region". When we excluded the 50km only in the EKE calculation, our results remained practically unchanged.

3) Is the austral summer/boreal winter taken into account in SOM analysis?

Since time is not used as an explicit variable in the SOM analysis, dealing with observations coming from EBUS located in different hemispheres and characterized with a different seasonality is not an issue.

10) P9912, L 9: please check sub-title (based ON).

The classification of EBUS that we propose here is based on their production regimes. Therefore, we believe the title "a production regime based classification of EBUS" is

BGD

8, C5038–C5045, 2011

correct.

11) L20-24: the largest the branch in the dendrogram plot, the more likely are the two clusters merged? If so, why all branches are cut at the "same height"?

Our argument in this paragraph is that the longer the branches in the dendrogram, the more different (or dissimilar) the clusters they merge. Since a "good" classification in the statistical sense is one which produces the most distinct clusters, it makes therefore full sense to cut the HAC tree at the level where the gap between two successive mergers is the largest (in our case it results in 4 classes).

12) P9914 L1: "...the weakest wind forcing contain primarily winter observations..." Here is the first time a reference to the time of the year is taken into account in the discussion of the results. Again: is time considered as a variable?

As discussed in our earlier response #9 above, time is not used as a variable for the SOM training. The time information is however implicitly contained in the results as the data used for training the SOM algorithm spans the four seasons in each EBUS.

13) Figure 12: arrows are different in length. Does it means something?

The different arrows are now made with equal size in the revised manuscript as only their direction is important. Furthermore, for more clarity, the pie diagram has been redesigned to better show how NPP and each of its drivers vary across the different EBUS.

14) Figure 13: would you add information about r2? intermediate and weak conditions looks far from being close to a linear fit.

As discussed in the paper and in our previous response #9, NPP is controlled by several variables whose effects on productivity is strongly non-linear. Therefore, we do agree with the Referee 2 that a linear fit of the upwelling strength is far from being able to explain the total variability observed in NPP. Yet, the goal in this paragraph is to show

BGD

8, C5038–C5045, 2011

that there is a statistically significant difference in the slope of the linear correlation between UWI and NPP, and that for a given strength of upwelling, NPP has a probability of more than 95% to be larger under weakly inhibited conditions in comparison to intermediate or strongly inhibited conditions. We added in the revised manuscript the 95% confidence intervals for the different slopes to Figures 13 and 14.

15) Discussion section: "inhibition": this is a statistical analysis. Fig. 6 suggests that when EKE or MLD has large values, NPP is low. But I don't see any causality evidence to proclaim those factors as inhibitors of NPP. It would be the same as arguing that correlation between two variables implies causation (which is not necessarily true).

As Referee #2 put it: this study is based on statistical analysis, which by definition cannot prove or identify the processes or mechanisms responsible for the reported relationships. Yet, we do believe that the discussion section is the right place to formulate hypotheses based on previous studies in the literature as well as from our recent model simulation studies in an attempt to explain our statistically based findings. To avoid any confusion and for a better consistency, we made it clearer in the revised manuscript that the discussed mechanisms are hypotheses based on previous studies and supported by our recent model-based works. In line with these arguments, we did the following modifications in the text:

- In the abstract we replaced "We show that in addition to the expected NPP enhancing effect of stronger equatorward alongshore wind..." by "Our results suggest that in addition to the expected NPP enhancing effect of stronger equatorward alongshore wind..."

- In the results section, . we replaced the statement "The relationship between these three drivers and NPP is therefore asymmetric and amounts to a limitation relationship" by "The relationship between these three drivers and NPP is therefore asymmetric and can be viewed as a limitation relationship"

BGD

8, C5038–C5045, 2011

- In the discussion section, . we replaced "A key finding of our analysis is that three factors tend to inhibit..." by "A key finding of our analysis is that three factors may inhibit..." . we substituted "Our finding that a narrow continental shelf tends to inhibit NPP can be interpreted" by "Our finding suggesting that a narrow continental shelf may inhibit NPP can be interpreted" . we replaced "The role of deep mixed layers in inhibiting NPP" by "The potential role of deep mixed layers in inhibiting NPP".

Interactive comment on Biogeosciences Discuss., 8, 9901, 2011.