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Ocean Science Discussions

Interactive comment on "Influence of Rossby waves on primary production from a coupled physical-biogeochemical model in the North Atlantic Ocean" *by* G. Charria et al.

G. Charria et al.

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Technical Corrections

p 934 ligne 3: "have a clear signature" → "appear to have a signature", given Specific Comment 1 above.

We made the correction.

p 934 l 11-12: This sentence does not appear to be well-supported by Figs. 7-10, of

which only Fig. 9a shows a significant vertical input of DIN.

We agree with Referee 3 and the analysis of the absolute value shows that the vertical input of DIN is not systematically correlated with a stronger increase in primary production. This sentence was then removed and the abstract was rewritten after the referee8217;s comments.

p 934 l 16-17: the "(" and ")" brackets around "$\sim \pm 20\%$ of the estimated primary production" can be removed.

We made the correction.

p 935 l 27, "Vertical velocities associated with Rossby waves can induce a similar effect": This statement is misleading, because the magnitude of the vertical velocity associated with Rossby waves is ten times smaller than for eddies. For example, if a typical eddy is a 20-cm SLA displacement associated with a 100-m thermocline displacement over 200 km, then a Rossby wave which is a 2-cm SLA displacement must be associated with a 10-m thermocline displacment over 200 km (or a 40-m thermocline displacment over 400 km, etc.). This point should be mentioned.

We agree with Referee 3 and the text was modified to mention this feature:
"Vertical velocities associated with Rossby waves can also induce a similar effect, named as the "Rossby rototiller" (Siegel et al., 2001), with two crucial differences: 1) the vertical velocities induced by Rossby waves are generally smaller than those in an eddy that is forming or evolving; 2) however, while eddies upwell water only while forming or intensifying, Rossby waves would upwell nutrients all along their propagation path through an ocean basin."

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p 938: It is unclear whether Eqs. 8 and 9 or Eq. 10 is used for $\bar{J}(z,t)$ in Eq. 6. If the former, then p 938 line 18 to p 939 line 3 ("Evans...(11)") should be removed. If the latter, then p 938 line 11-18 ("In fact...depth).") should be removed. If Eq. 10 is kept, then β needs to be defined.

Equation (10) is the analytical solution of Equation (8). Then, even if in the model we are using equation (10), we had to write equation (8) in the manuscript to explain the form of the equation. However, we agree with Referee 3 and added the missing definition of ß (Page 939 Line 1).

p 939 l 1-2: 8220;τ the time at noon" $\rightarrow$ "τ the time from sunrise to noon"

We made the correction.

p 939: It should be mentioned somewhere that the model DON is only the "highly labile" component of total DON; this makes the quick equilibration time understandable.

The sentence, p. 937 l. 22, was modified to introduce the semi-labile form of DON: "... D particulate organic matter and DON the semi-labile dissolved organic nitrogen".

p 940 l 18, "a monthly zonal average...is removed": Were regions near the coast excluded from this zonal average?

Yes, regions near the coast were excluded ($\sim 12^o$ far from the coast), represented as white areas in Fig. 4, mainly due to high chlorophyll concentrations close to the coast.

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The text was modified to add this information: "As our study focuses on the anomalies of surface chlorophyll-a concentrations, a monthly zonal average from raw data at each latitude and for each month is removed (coastal regions are excluded), so also removing part of the seasonal cycle."

p 941 l 2-6: What objective analysis mapping scales were used for SLA? 150 km and 20 days, as in Le Traon et al. (1998)?

The method was improved since Le Traon et al., 1998 and the mapping scales were changed. The products distributed today are using a 15 days temporal correlation scale at midlatitudes, and a space correlation scale depending on the latitude, equal to $\sim$200 km at 25^{o}N (Ducet et al., 2000).

p 942 l 19: "slightly higher north" $\rightarrow$ "slightly larger north" would read better, to avoid confusion with "higher north" i.e. northward.

We made the correction.

p 9 944 l 4-5, "Larger values...": It seems clear that the larger values in the north-west are due to rings and eddies generated by the Gulf Stream extension. (The Gulf Stream can generate Rossby waves too, but Rossby wave SLA anomalies are only a tenth that of eddies.) Note the zonal band centered at 35 N 40 W in Fig. 4 is clearly associated with the Azores front, and not a Rossby wave propogating from the coast.

We agree with Referee 3 that in the Gulf Stream region and the Azores front, eddies are intense and have a strong signature on SLA anomalies. However, the contribution

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of the Rossby waves and eddies is tricky to quantify. We believe that at spatial scales (400km) that we are considering, larger amplitudes are mainly Rossby wave amplitudes, but interaction between wave and eddies in these regions will certainly contribute to change Rossby wave amplitudes.

p 944 l 20-21: These are Chl "crests" and "troughs", not SLA "crests" and "troughs", right? This should be clarified.

Referee 3 is right, the text was thus modified. 8220;chlorophyll crests8221; and 8220;chlorophyll troughs8221; are mentioned.

p 945 footnote 2: "between CA+ (or CA0) divided" → "between CA+ (or CA-) and CA0 divided"

We made the correction.

p 948 l 7-8, "In fact...": This sentence is unclear and does not contribute; it probably can be removed.

The sentence was removed.

p 948 l 17: "confirms" → "suggests" would be better, as 8220;confirms" seems too strong.

The verb was replaced by "suggests" as advised by Referee 3.

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p 950 l 13-15, "The different...": This sentence is unclear. It is not clear what "strong stratification in summer which will induce a de-correlation...in the mixed layer and below" has to do with changes in primary production.

We decided to remove this sentence since it did not improve the understanding.

p 951 l 16-17 "which is a region where non linear eddies are not predominant": a reference to support this point would be helpful here.

We added the reference to the paper Chelton et al. (2007) to support this point.

p 951 l 17: "allow to certify" → "certify"

We made the correction.

p 951 l 18, "However, Sweeney...": I do not believe that they showed that.

Following Referee 3 's remark, we removed the Sweeney et al., 2003 reference. This reference was chosen because they describe the absence of biogeochemical perturbations during the passage of anticyclonic eddies.

p 952 l 2 "vertical inorganic dissolved nitrogen advection": Only in Fig. 9a was "zad no3" positive, and it is not possible to compare its magnitude (in units mmol N m-2d-1) with the other fluxes.

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The contribution is now more detailed in the text with the absolute values and appears in new figures 8, 10 and 13. Indeed, after revision, it appears that the vertical inorganic dissolved nitrogen diffusion is more intense than the advection.

p 952 l 12 "different particular cases" → "particular time-longitude cases"

We made the correction.

Fig. 2: It would be an improvement to remove the blank areas west of 70^{o}W and east of 23^{o}W i.e. have the x-axis be -70 to -23.

We changed significantly Figure 2 to add longitude-time diagrams at several latitudes.

Fig. 3 recalls the assessment of Isachsen et al. (2007, JPO p 1177), and references therein, that baroclinic Rossby waves are unstable at higher latitudes. This probably should be mentioned, either on p 943 or in the Discussion.

We now mention this point and refer to this paper (Page 951 Line 17).

Fig. 6: It would be helpful to add vertical dashed or dotted lines at 39, 28, 23 and 17^{o}N to separate the different regions discussed in the text.

We made the correction on new Figure 7 (old Figure 6).

Interactive comment on Ocean Sci. Discuss., 4, 933, 2007.