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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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Anonymous Referee 3

General Comments

Overall this paper makes an excellent point: that although Rossby wave crests may enhance primary production, Rossby wave troughs (according to their model) reduce primary production, such that the net effect of Rossby waves on primary production appears to be small. Although this paper has several weaknesses (listed below), I expect this conclusion will stand. So this paper is a significant and thought-provoking contribution to the current discussion in the literature on the effect of Rossby waves on ocean biogeochemistry. I support its publication after some revision.

Specific Comments

(1) My first concern is the overstated confidence (in the Abstract and Introduction) that the effect of Rossby waves on surface chlorophyll has been observed. Figures in Killworth et al. (2004), Sakamoto et al. (2004), Machu et al. (1999) and Cipollini et al. (1997) suggest smaller zonal wavelengths ca. 400 km, which could equally appropriately be called mesoscale eddies. Chelton et al. (2007) say that what they thought were Rossby waves actually are mesoscale eddies. This is because altimetric data doesn8217;t really have enough longitudinal resolution to accurately assess wavelengths smaller than 400 km, and because of the similar westward propogation speeds and spatiotemporal scales of baroclinic Rossby waves and eddies. For instance, a 1000 km x 1000 km domain generally contains only a few strong eddies, such that the mean SLA in that domain is more likely dominated by the residual of the eddy SLA than a long Rossby wave SLA. While long (1000-km wavelength) Rossby waves presumably can be generated by atmospheric forcing, they are unstable where they significantly exceed the internal Rossby radius of deformation i.e. at higher latitudes (Isachsen et al., 2007, JPO p 1177, and references therein). Rather than saying that Rossby waves have been observed (which have the wrong propogation speed), it is perhaps more appropriate to say what has been observed are not exactly linear Rossby waves (Zang and Wunsch, 1999, JPO p 2183).
What I do like about this modeling study is that the 1/3-degree resolution suppresses the intensity of eddies, while adequately resolving 300+ km Rossby waves, and so may be in a better position to assess the impact of long Rossby waves than (potentially eddy-aliased) satellite data. That is, model grid resolution can be used to filter out certain phenomena. So even though their model results do not include the

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effect of shorter-scale Rossby waves or wave-eddy interactions, their simulation and conclusions do apply to waves with 300+ km wavelengths.

Concerning the first part of this comment, we agree with Referee 3 and we made changes in the abstract and in the introduction of the revised version of the paper, saying what was observed in the satellite data are baroclinic Rossby waves as well as non-linear eddies with the same spatiotemporal scales. We also added a comment in the revised version of the paper (Page 942 Line 8) about the model 1/3-degree spatial resolution which suppresses or filters the intensity of eddies as well as shorter-scale Rossby waves, and wave - eddy interactions as compared to satellite data.

(2) The most significant problem is that many of the statements in Section 6.1 cannot be supported by Figs. 7-9. Figs. 7-9 compare terms in units of percentage; this is misleading. For example in Fig. 7b both "zad phy" and "yad phy" increase by 100%, but the reader does not know which one is actually larger in units of mmol N m-2d-1. It is possible for a flux to have a large percentage increase but still be negligible compared to the other terms. Figs. 7-9 should have been presented in units mmolN m-2 d-1 i.e. as (CA+ - CA0) instead of (CA+ - CA0)/CA0. Thus I suspect the unusual conclusion on p 947 ligne 5-6 of a production increase due to vertical diffusion of phytoplankton is a mistaken interpretation based on a large percentage increase in a minor flux.

We agree with Referee 3 and we improved the figures 8, 10 and 13 (old figures 7 to 9). The absolute values (CA+, CA0, CA-) are now represented together with the associated percentages to better understand the relative amplitude of the different processes. This representation was not previously used because the range of values can be very different according to each process. This is why in some cases we are not able to visualize the absolute values. To limit this effect, a linear Y scale, divided in 6

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intervals, is preferred.

The differences (CA+ - CA0), suggested by Referee 3, are not represented in the new figures 8, 10 and 13 because they are not straightforward to interpret following the sign and amplitude of CA+ and CA0 processes. Indeed, the sign of the difference will depend of the positive or negative sign of CA+ and CA0, and their value could also result in a different sign. Then, the interpretation of the difference will be impossible without the original value. For these reasons, we keep on figures 8, 10 and 13 (old figures 7 to 9) the absolute values (CA+, CA0, CA-) and the percentages (100[(CA+) 8211; CA0]/|CA0|).

After analysing these new figures, the conclusion on p947 line 5-6 is confirmed and we can clearly observe the important role of the vertical diffusion.

In addition, in Figs. 7-9 the NO_3 physical fluxes cannot be directly compared with the phytoplankton physical fluxes to explain IPP. For example, an increase in NO_3 input will increase IPP if the phytoplankton are nutrient-limited but not if they are light-limited. Changes in primary production $\partial J(z,t,N)P/\partial t$ should be compared against the contributing factors of changes in phytoplankton concentration $J(z,t,N)\partial P/\partial t$, changes in light limitation $P\bar{J}(z,t)/\partial t$ and changes in nutrient limitation $PJ_{max}\partial L_{NO3}/\partial t$. Consequently the conclusion on p 950 line 9-11 ("By contrast...") has not actually been demonstrated.

Also of interest are the mechanisms (advection versus growth) that cause increases in surface (i.e. satellite-observable) Chl concentrations caused by Rossby waves. This was not evaluated. To do this, the phytoplankton physical fluxes should be compared against IPP and the phytoplankton loss terms (pathways 1, 2, 6 and 8 in Fig. 1) to investigate the causes of $\partial P/\partial t$. Similarly, the NO_3 physical fluxes can only be directly compared against IPP the NO_3 source terms (pathways 1, 3 and 9 in Fig. 1).

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Indeed, as highlighted by Referee 3, the primary production will react to increase in NO_3 if the phytoplankton is nutrient-limited. In our study, the different cases are located in the North Atlantic Ocean between 8^{o}N and 40^{o}N, which is an oligotrophic region where the phytoplankton growth is usually nutrient-limited and not light-limited. The NO_3 and phytoplankton physical fluxes can then be directly compared to explain IPP function of phytoplankton and NO_3 concentrations (cf. equations 2 and 7).
To clarify our approach, the text was modified as follows (p945 line 10):
"... the underlying physical processes investigated (Fig. 8-10). Indeed, the primary production, which is nutrient-limited between 8^{o}N and 40^{o}N, is function of dissolved inorganic nitrogen and phytoplankton concentrations (Eq. 2). Results are very sensitive ..."
The phytoplankton and NO_3 concentrations are driving the primary production in the model for the studied region. In this study, we focused on the perturbation of concentrations due to advection and diffusion terms, and we agree that these concentrations are also depending on the biological fluxes. Our approach, analysing only the physical fluxes, is justified by the scale and the nature of the processes that we are analysing. Indeed, we intend to highlight the effect of Rossby waves, a physical process, on one of the biological fluxes in the model, the primary production. The way of integrating processes is related to this physical process signature and the variation of physical inputs of nitrogen are analysed.

(3) Longitude- and time-ranges in Fig. 5 were selected where the Chl-SLA cross spectrum amplitudes were above a certain value (p 944 line 8). While this is acceptable in order to find out the mechanisms (advection versus growth) behind high Chl-SLA correlations, it does not include times or locations where Rossby waves are observed but no Chl response is seen. Consequently Fig. 6 only shows the extreme cases; do the remaining longitude-time windows show negligible change in primary production in response to Rossby waves? To assess the net impact of Rossby waves, the longitudetime ranges should have been selected on the basis of SLA amplitudes alone.

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Indeed, our selected approach does not allow analyzing cases where Rossby waves are propagating but no surface chlorophyll response is detected; consequently only extreme cases are analyzed, as noted by Referee 3. Furthermore, using this approach based on surface chlorophyll signature of Rossby waves, we are not able to detect changes in primary production in response to Rossby waves when no surface chlorophyll signature is detected. This study gives only an estimation of the primary production increase when Rossby wave signature can be detected in surface chlorophyll concentrations.
As suggested by Referee 3, the longitude/time ranges should have been selected on the basis of SLA amplitudes alone to assess the net impact of Rossby waves, However, two reasons explain why we did not base our study on SLA amplitudes alone. First, a signal detected in SLA amplitudes allows locating the Rossby wave but the effect on biology can be delayed in space and time as compared to this location. It is then extremely delicate to determine the limit of the wave effect to integrate primary production and then deduce the wave influence. The second reason is that in this work, we investigate the processes yielding a surface chlorophyll signature and their effect on primary production.

The fact that high Chl-SLA cross spectrum amplitudes were selected means that cases where Chl lagged (or led) SLA by $\pi/2$ were not considered. Fig. 3a indicates significant lags do exist, as is expected from Fig. 9 in Killworth et al. (2004). This suggests that an additional analysis should be done, investigating the mechanisms (advection versus growth) behind cases where Chl lags SLA by $\pi/2$ i.e. high Chl-SLA cross spectrum amplitudes that include this lag. In the interest of time, the authors may not need to do this analysis in this paper, as long as they acknowledge that this investigation is missing from their assessment.

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The chlorophyll and SLA Rossby wave signatures are generally out of phase. In the 2-D cross-spectral analysis that we performed, peaks are selected on the basis of their amplitude, regardless of the phase at the peak. Indeed, each cross spectrum amplitude is associated with a phase, which is generally different from zero and in some cases might well be close to $\pi/2$ as suggested by Referee 3. In other words, in this paper, we decided not to display phases associated to the cross spectrum amplitudes but we can ensure that cases where Chl and SLA are strongly correlated but lagged (or led) by $\pi/2$ are taken into account in our region selection.

To fully clarify this point, the following sentence was added page 944, line 9: "... above 4.35 cm log10 $(mgChlm^{-3})$. These peaks in the cross-spectrum correspond to different phase relationships between the surface chlorophyll anomalies and SLA."

(4) In Fig. 6, north of 17^{o}N there seems to be little correlation between CA+ (or CA-) and increase in primary production. That is, the mean is near zero, there is no largescale trend, as the signs differ in 5 of 7 pairs at the same latitude. This suggests that the estimates are not robust i.e. that they are sensitive to the time-longitude window limits used in Fig. 5. What does seem to be robust however is (i) that the CA- appear to approximately counterbalance the CA+ (can they be shown to be statistically anticorrelated?) and (ii) south of 17^{o}N CA+ (CA-) are associated with increases (decreases).

Indeed, we agree that there is no large-scale trend in primary production increases. This result was an unexpected conclusion showing the strong spatio-temporal variability in the coupled processes involved during the Rossby wave passage. We agree with Referee 3 and confirm that estimates are sensitive to the time-longitude window limits used in Fig. 5. As we specify in the conclusion, this study highlights specific cases and a different approach is necessary to estimate the net effect of Rossby waves on

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primary production at the North Atlantic basin scale.

Referee 3 suggests that two regions can be extracted from Fig. 7 (old Figure 6): (i) north of 17^{o}N, the CA- appear to approximately counterbalance the CA + (anticorrelation) and (ii) south of 17^{o}N CA+ (CA-) are associated with increases (decreases). We computed the correlation between CA+ and CA- north and south of 17^{o}N. The correlation coefficients obtained are -0.65 south of 17^{o}N and -0.49 north of 17^{o}N. These values suggest that CA+ and CA- are anticorrelated with a stronger value south of 17^{o}N but not totally counterbalanced (correlation coefficient higher than -1).

(5) What is the model's 1998 estimate of annual primary production in the oligotrophic gyre? (It was not in Charria et al., 2006b.) If it is significantly lower than observed, this questions the relative magnitude of the model's biological response to long Rossby waves. For example, if the model underestimates primary production by a factor of 2 (due to an underestimate of recycled production), a 20$\%$ increase in model primary production due to Rossby waves perhaps should be interpreted as a 10-20$\%$ increase expected for the true ocean.

The model's 1998 estimates of annual primary production in the oligotrophic gyre are equal to 448 and 392 $mgCm^{-2}d^{-1}$ in the North Atlantic Subtropical Gyre Province-West (NASW) and in the North Atlantic Subtropical Gyre Province-East (NASE), respectively, as defined by Longhurst (1998). These estimates are in good agreement with the range of estimations by different studies (see Table below). This comparison between our model estimate in 1998 and estimations by different studies for primary production is described in a paper in the Biogeosciences Discussion at the moment (Charria, G., I. Dadou, J. Llido, M. Drévillon, and V. Garçon, Importance of Dissolved Organic Nitrogen in the North Atlantic Ocean to sustain primary production: a 3D modeling view, *Biogeosciences Discuss.*, 5, 1727-1764, 2008)

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Biogeochemical provinces	JGOFS	ME	AM96	BF97	This study
ARCT	1330	507	430	687	173
SARC	830	472	483	887	249
NADR	660	555	484	852	230
GFST	490	522	488	677	363
NASW	459	304	355	360	448
NASE	330	410	413	526	392
NATR	290	313	513	363	219

Table: Primary production $(mgCm^{-2}d^{-1})$ estimated by different studies (this study, JGOFS (Ducklow, 2003), ME (Mélin, 2003), AM96 (Antoine and Morel, 1996) and BF97 (Behrenfeld and Falkowski, 1997) for different biogeochemical provinces as defined by Longhurst (1998): ARCT (Atlantic Arctic Province), SARC (Atlantic Subarctic Province), NADR (North Atlantic Subtropical Drift Province), GFST (Gulf Stream province), NASW (North Atlantic Subtropical Gyral Province - West), NASE (North Atlantic Subtropical Gyral Province - East) , NATR (North Atlantic Tropical Gyre Province).

(6) The physical model was initialized from climatology at the start of 1995, and the model results from 1998 examined (p 939). A concern is that this might not be enough time for the kinetic energy in the model to equilibrate. What is of relevance here is Rossby wave activity as evident in SLA variance. Has SLA variance approximately equilibrated by 1998, or is it showing a significant trend? (Given the interannual variability in the model forcing, exact equilibrium is not expected.)

Due to recent technical problems, we are not able to show you the SLA variance during the spin-up years. However, the physics in the model was carefully validated

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in 1997 and 1998 simulated years as detailed in Charria et al., 2008 (Importance of Dissolved Organic Nitrogen in the North Atlantic Ocean to sustain primary production: a 3D modelling view, Biogeosciences Discuss., 5, 1727-1764, 2008) and the surface physics was equilibrated in 1998. To illustrate the balanced state in 1998, the temperature and salinity standard deviation and mean calculated from all model grid points from 1995 to 1999 was analysed. It appears a decreasing trend in standard deviation (std) of salinity from 1995 to 1997. At the opposite, from 1998, the std is almost constant. Concerning the temperature, the seasonal cycle is very similar in 1998 and 1999 compared to previous years. Based on this analysis, we can confirm that by the end of year three (for the physics) the surface physics is well established for the study of Rossby wave activity.

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