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Interactive Comment

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.

Anonymous Referee #3

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

(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^{-2} d^{-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 mmol N 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.

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 $P\partial \bar{J}(z,t)/\partial t$ 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).

(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 longitude-time ranges should have been selected on the basis of SLA amplitudes alone.

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.

(4) In Fig. 6, north of 17°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 large-scale 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°N CA+ (CA-) are associated with increases (decreases).

(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 magnitide 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.

(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.)

Technical Corrections

p 934 ligne 3: “have a clear signature" $\rightarrow$ “appear to have a signature", given Specific Comment #1 above.

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.

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

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.

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.

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

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.

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

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)?

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

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.

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

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

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

p 948 l 17: "confirms" $\rightarrow$ "suggests" would be better, as "confirms" seems too strong.

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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.

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.

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

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

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^{-2} d^{-1}) with the other fluxes.

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

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

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.

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

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