

***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.

Received and published: 7 July 2008

Anonymous Referee 1

Detailed comments

Abstract: This reads as though part of it has been lifted from a proposal. It starts with a question and then says 'we aim to investigate ... '. Then if an abstract wants to tell me that vertical and horizontal processes are involved, I could wonder whether there is really anything new in the paper.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



The abstract was slightly rewritten to better highlight the new results from the paper. Indeed, we intend to show in this study what is the origin of the surface chlorophyll signature of Rossby waves and their impact on primary production. Results are showing that the processes involved are strongly variable in space and time and, based on 20 regions, the chlorophyll signature may be attributed to a combination of horizontal and vertical processes. Furthermore, this study pointed out the local impact of Rossby waves on primary production.

[Page 935, line 25: This section confuses the long baroclinic Rossby waves discussed later with the meso-scale eddies discussed by Oschlies et al.](#)

We agree with the referee and the text was improved following the comment from Referee 3 (Page 935, line 27):

"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."

[P 936, Section 2: The detailed equations for the biological model add very little to the paper and could either be left out, as has been done with the physical model, or placed in an Appendix.](#)

Both other referees asked for more details on the biological model equations. To properly answer their concerns, we had to add explanations, so we think that these biological equations should be in the paper.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

P 936, Section 2: The results of the paper are only of interest if we have some confidence in the underlying physical and biological models. At present the only real justification is the newsletter article of Charria et al (2006b).

Either some good references to refereed papers should be added or the paper should include examples of the fields, showing both spatial and temporal behaviour, which can be compared to observations and other models.

Indeed, the ability of the physical and biological models to reproduce realistic fields is necessary to support this study. When this paper was submitted, we only referred to the newsletter published in 2006 but we can today refer to a paper submitted in Biogeosciences presenting a detailed validation of the simulation used in our study. The reference to this paper was added in the manuscript:

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.

P 939, L 18. What is the scale length of the exponent?

The scale length of the exponent is 1/100. We chose 100 m as the vertical scale for the euphotic zone. This value is usually chosen in similar studies: Sarmiento et al., 1993; Oschlies and Garçon, 1999.

P 939, L 19. I am surprised that the Rossby wave and other transients had all died out by the end of year two. The statement could do with more justification.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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 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.

[P 942, L 3. This section implies that model also has other \(eastward propagating?\) waves present. Is this so?](#)

Westward propagations are clearly dominant at these scales in the region (cf. Charria et al., 2006, JMR). The preprocessing is necessary to clearly extract the Rossby wave signature, mainly in surface chlorophyll concentrations where a steady signal is observed as, for example, the marked seasonal cycle. There is no evidence of basin-scale eastward propagations.

[This raises a more important point. The paper seems to assume everywhere that the features being observed are long Rossby waves orientated predominantly north-south. The paper provides no evidence of this.](#)

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Indeed, in this paper, the spatial and temporal scales analyzed are corresponding to the first mode of baroclinic Rossby waves propagating zonally. Before exploring these propagations, their features were identified as described in section 4. In this section, the identification is based on wavelengths, periods and phase speeds. When we compare these properties with the dispersion relationship from linear and extended theories, our simulated fields are fitting very well with the theoretical curves. However, we agree that according to recent studies (Chelton et al., 2007), as mentioned in Discussion (section 7), non-linear propagating eddies can have similar signature. We believe that for wavelengths larger than 400 km (equivalent to an eddy with a 200 km diameter) analyzed after filtering, baroclinic Rossby waves are the dominant westward propagating signals. Further investigations are in progress to distinguish baroclinic Rossby waves and non-linear eddies in sea level anomaly fields.

If they are long Rossby waves with a predominantly north-south orientation, then one would expect the same Rossby waves would show up on the neighbouring lines of figure 5 and have very similar effects on the biology. However the discussion (for example of the two northern lines) implies (because different times had to be used) that the Rossby wave regime is very different on neighbouring lines.

Indeed, the referee pointed out that two close regions at close latitudes should correspond to the same wave. We fully agree with this statement, but for the same wave, the biogeochemical background can change dramatically with latitude and time, and then we can expect large variations for neighbouring lines.

P 942, L 7. The model grid size here (~ 40 km) is an order of magnitude less than the wavelengths being studied (>400 km). For most ocean models, numerical errors are

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

proportions to the square of this ratio. Thus unless the present model has some other serious problem, grid size effects are unlikely to produce an error in SSH of more than a few percent (say <4%). The grid size certainly could not explain the factor of two seen in figure 2. Thus the differences are more likely to be due to errors in the forcing or in some other aspect of the model physics.

These lower amplitudes in the simulations compared to the satellite data are partly due to the model spatial resolution. With the 1/3-degree eddy-permitting spatial resolution, our model results do not include the effects of shorter-scale Rossby waves (less than ~ 300 km), waves-eddy interactions as well as eddies with the same spatiotemporal scales (Chelton et al., 2007). These processes are included in the satellite data and might explain the higher SLA amplitude in the satellite data. Another explanation is due to the atmospheric forcing (model input in our study 8211; ECMWF products), which might not have the right frequency (here daily) to force the entire Rossby wave spectrum as in the satellite data. We added this comment in the revised version of the paper (Page 942 Line 6).

942, L 19: How can the surface chlorophyll and SSH fields be related if their wavelengths differ? It would be helpful to add a model chlorophyll plot to figure 2 and to give examples from more than one latitude.

Indeed, the surface chlorophyll and SSH fields cannot be related if their wavelengths differ. In this part of the paper, we describe the predominant wavelengths, which might be different in both datasets. When we performed the cross-spectrum analysis, same wavelengths were considered for chlorophyll and SSH fields.

Following the referee comments, we prefer to remove the sentence p. 942, l. 19.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

As required by Referee 1, and in order to improve the view of the relationship between chlorophyll anomalies and SSH anomalies in our simulations, we modified Figure 2 including now several latitudes in SSH and chlorophyll.

P 944, L 11. Although the study of extreme cases with a good chlorophyll signal is a useful start, you also need to explain why the neighbouring regions did not have such a good signal.

Rossby waves are propagating at all latitudes with a clear signal in sea level anomalies but their signature in surface chlorophyll concentrations is not observed everywhere. This wide range of signal intensities is strongly related to the biogeochemical processes and biogeochemical background in the region. Indeed, for example, small differences in the vertical phytoplankton gradients (vertical position and intensity) can explain that along the trajectory of the same wave, the signal in chlorophyll concentrations is not observed all the time.

In the manuscript, we modified the text as follows: "...Based on these latitudes, the extent of the time/space region is refined over the time period where positive/negative chlorophyll anomalies can be clearly identified and followed in time. Indeed, the variations of the biogeochemical background conditions along the wave trajectory, tracked using the SLA signature, are inducing fluctuations in the chlorophyll signature intensity."

P 944, L 15. Region CA0 is defined as "the lack of chlorophyll anomaly", but given that the wave crest and wave trough have been defined, a description in terms of the wave properties may be better. Later CAO is described as the "background condition" but given that conditions during wave growth and decay are different is it a mistake to average over both?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



To estimate the "background condition" in the case of a "lack of chlorophyll anomaly", we decided to average over three points before, between and after the chlorophyll wave crest and trough. We agree that the conditions during wave growth and decay can be different and this is precisely the reason why we are considering an average situation. Indeed, in this study, our purpose is to compare the positive and negative chlorophyll anomalies with the same mean state, then even if this "background condition" can vary over one wavelength, we are concentrating over the perturbations compared to this mean background condition.

P 943, L 20. "The value for the lack of chlorophyll" A better descriptive phrase is needed. In calculating the values of fluxes during the CAO period I am not clear why the "western boundary of the trough" is not the same as "the eastern boundary of the crest" or the the bit presumably "between the crest and the trough". It may be helpful to show some examples of the waves and the regions chosen for analysis.

To support these sentences describing the meaning of CA+, CA- and CA0 integrations, we added a diagram to illustrate the method (new Fig. 6).

Figures 6 to 9. It would be better to plot absolute values of the fluxes (per cubic m of the surface layer). The values currently plotted do not show which changes are most important. As a result much of the discussion in the rest of the paper could be in error. One of the relative fluxes may have increased significantly, but its absolute effect may have been negligible.

Old figures 6 to 9 were replaced to include absolute values and the text in the paper was accordingly adjusted as a function of these new detailed figures.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

After considering the absolute values with the percentages, the conclusions remain similar. The text was slightly improved to take into account this new information. Indeed, the description based on absolute values emphasized the importance of vertical diffusion, which upwells more nitrogen than advection does. This last point shows that in our model the diffusion is playing a central role in reproducing the vertical tracer distributions.

Section 6. The section needs to be reworked with more information on the vertical profiles and horizontal gradients of chlorophyll and nutrients, and the vertical and horizontal volume transports. This would help the reader understand better the differences between the different sections - i.e. are the differences in the vertical flux of chlorophyll due to different vertical volume fluxes or to differences in the vertical gradient of chlorophyll?

The section 6 was reworked with more information on vertical profiles (new figures 9, 11, and 12) to improve the understanding of our results.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)