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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 2

Detailed comments

1. Page 936 line 21 and Page 942 line 7. There might be some initial comment about whether 1/3^o is suitable spatial resolution. If the Rossby wavelengths are 400 km or more, this is more than 10 grid points per wavelength, OK I would think. Are the "lower amplitudes in the simulations" really "due to the model spatial resolution"?

We agree with Referee 2. We added a comment in the paper about the model grid size, which has an adequate spatial resolution to resolve Rossby wave with wavelengths longer than 300 km (Page 942 Line 8). For instance, with a $1/3^o$ resolution, there are 10 grid points or more for a 300 km wavelength wave.

Concerning the lower amplitudes in the simulations, they are partly due to the model spatial resolution. With the 1/3-degree spatial resolution, our model results do not include, or with some smoothing, the effect of shorter-scale Rossby waves (less than ~300 km) as well as wave-eddy interactions. 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 output in our study) 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 8).

2. Page 938. There is a problem with the notation for time. In (8) there is integration over t = time of day to produce J (overbar) which is still a function of time (of year?). This definitely needs a different variable from the one integrated over. Presumably time in (6) is the same as on the left side of (8). In (10), beta needs to be defined and also tau: "the time at noon" is not a helpful definition.

Yes, in the $\bar{J}$ term, t is measured in days. So this term is the daily integrated light-limited growth rate. The text has been changed in the revised version of the paper (Page 938 Line 4). We also changed the notation of time (t' now) in the right side of equation (8) and in the equations (9), (10) and (11) which represents the time over a daily duration d (t'=0 at sunrise, t'=τ=d/2 at noon, t'=d at sunset). We also added the definition of β (Page 939 Line 1) and τ (Page 939 Line 1).

OSD

4, S495–S501, 2008

Interactive Comment

3. Pages 945-948. I found this quite hard to follow, and I am not convinced by the use of percentages. If the CA0 value of a term is small, then a big percentage change may not represent much effect in the P equation. If absolute values (not percentages) were used, then the some of the terms (the "phy" terms?) should add to equate to the rate of primary production; their anomalies would add to the change in rate of primary production. I think this could be shown diagrammatically better than the present figures 7 to 9.

We agree with Referee 2 and we improved the figures 8, 10 and 13 (old figures 7 to 9). The absolute values 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 segments, is preferred.

I think the advection and vertical diffusion of DIN are different; they do not contribute in the same way to the phytoplankton equation (advective-diffusive plus SMS). In (2) DIN only appears through J; there is no advection or diffusion of N here. So how do advection and vertical diffusion of DIN have equal status with advection and vertical diffusion of chlorophyll/phytoplankton in figures 7 to 10? I think clarification is needed.

The advection and vertical diffusion of DIN or phytoplankton can be very different (cf. new figures 7 to 9). The equations 1 to 5 are only representing the source-minus-sink terms. In the model simulation an advection/diffusion equation is solved at each time step for each tracer. This equation can be written as follow:

OSD

4, S495–S501, 2008

$$\frac{\partial C}{\partial t} = SMS(C) - \left[\frac{1}{e_1 e_2}\left(\frac{\partial(e_2 C u)}{\partial i} + \frac{\partial(e_1 C v)}{\partial j}\right) + \frac{1}{e_3}\frac{\partial(C w)}{\partial k}\right] + D^{vC} \quad (1)$$

where C is the concentration of the considered tracer (DIN,P,Z,D,DON), SMS(C) is corresponding to the equations 1 to 5. D^{vc} is the vertical diffusion and the local deformation of the coordinates system is given by the scale factors: e1, e2, and e3 (Madec et al., 1999).
Thus, each tracer is advected and diffused using the same advection/diffusion scheme. The DIN and phytoplankton advections and vertical diffusions are comparable.

4. Page 935 lines 11-12. Better ".. assumptions were made (for example .. ratio). We .."

We made the correction.

5. Page 936 line 2. Better ".. processes such as induction in .."

We made the correction.

6. Page 939, line 7. Strictly the "G(P) slope" for small P is 2pP, not p.

We agree with Referee 2 that the "G(P) slope" is 2pP here. To describe more precisely the terms in Equation 12, we replaced "p the G(P) slope for the weak P values" by "p the prey capture rate".

7. Page 942 lines 4, 5. In the figure it looks as though the wave height from crest to

OSD
4, S495–S501, 2008

trough (˜2 x wave amplitude) reaches 5 cm (simulated), 7-10 cm (observed).

We agree with Referee 2 and replaced 'amplitudes slightly weaker' by 'amplitudes weaker' in Page 942, line 4, 5.

8. Page 942 line 19. Better ".. slightly longer north of this .."

This sentence was removed in the new manuscript as suggested by other Referees.

9. Page 944 line and figure 4. Should the units be "cm log10(mgChl m-3)"? The Rossby wave amplitudes are only a few cm; I assume the chlorophyll concentration is of order 10 mgChl m-3, not 10**100 mgChl m-3 .

Indeed, Referee 2 is right, the units of the cross-spectrum amplitudes are in cm $log10(mgChlm^{-3})$. The units are corrected in the manuscript.

10. Page 945 footnote 2. Better to write this as a formula, 100[(CA+) - CA0]/ |CA0| ?

The formula was added in the footnote 2, page 945.

11. Page 957, Table 2 title line 3. ".. North- .."

We made the correction.

12. Page 958 Figure 1. The caption should explain that the numbers relate to the text

OSD

4, S495–S501, 2008

Interactive Comment

in section 2 before equations (1) to (5).

We added more details in Figure 1's caption to relate numbers to the corresponding fluxes and to define the different boxes.

13. Page 964 Figure 7. The caption in lines 3-4 should explain the abbreviated notation for location of the sections, as in section 6 of the text.

The abbreviations are now explained in Figure 7's caption.

14. Page 967 Figure 10. Caption line 8, omit "respectively". Can there be consistency please, either always "chlorophyll" or always "phytoplankton" (as in figures 7-9)?

"Respectively" was removed in Figure 10's caption.

Along the manuscript, "chlorophyll" and "phytoplankton" terms are used following the quantity that we are analysing. The satellite data are chlorophyll concentrations and the biogeochemical model is a nitrogen-based model, then the concentrations calculated are corresponding to a phytoplankton biomass, expressed in mmolN m-3. In Figure 10, we are comparing chlorophyll concentrations from the satellite data and model fields. A variable chlorophyll-to-nitrogen ratio was used following Hurtt and Armstrong (1996) to convert modelled phytoplankton in nitrogen units into chlorophyll concentrations. This conversion is used in this study when we compared remotely sensed data and simulated fields (statement added in the manuscript p. 942, l. 15-16 and new figure 14 (old figure 10) caption adjusted).

OSD

4, S495–S501, 2008

Interactive Comment

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OSD

4, S495–S501, 2008

Interactive Comment