

We wish to thank referee #2 for his/her detailed analysis and his/her thoughtful comments, which will improve the quality of this manuscript. Here, you will find a detailed reply to each comments :

Response to Referee#2's Comments

Major and specific comments

1) Figure 1: Why not add a figure comparing model and measured data for nitrate in, for example, surface waters? Besides chlorophyll and surface currents, there is not much comparison with observations, which would add robustness to the model results.

In response to referee #1 and #2, we propose to add, in Figure 1, white contours of nitrate concentrations at 100m depth (the depth of the boxes defined for our analysis) both in the model and the CARS 2009 global atlas product. This will permit us to show the sharp change in nutrient concentrations which occurs off Cape Blanc between nutrient-poor North-Atlantic Central Water north of Cape Blanc, and nutrient-rich South-Atlantic Central Water south of Cape Blanc. This will be mentioned in the text p.5/l.12 :

« Noticeably, the flow of the undercurrent over the slope is always poleward (not shown) in agreement with observations (Mittelstaedt, 1983). Besides the model accurately represents, at latitudes around Cape Blanc, the sharp gradient of nutrient concentrations in upwelling source waters between nutrient-poor North Atlantic Central Water (NACW) and nutrient-rich South-Atlantic Central Water (SACW), respectively north and south of Cape Blanc (see contours in Fig. 1). This actually results from the deepening of the poleward undercurrent transporting SACW and its intensive mixing with NACW north of Cape Blanc (Mittelstaedt, 1983). »

2) Section 3.1.1, lines 15-16: The sentences "Offshore transport of nitrate... cross-shore velocities" do not really fit this paragraph because they describe the nutrient behavior while the paragraph is about ocean circulation. They should be moved to line 23, before "Vertical (upwelling-induced) nitrate supply..". Then, remove "(see above)" and add (Fig. 3d) at the end of "cross-shore velocities".

We agree and will take into consideration this suggestion.

3) I think it will be nice to indicate somewhere (for example in section 3.1.1, end of line 6) that negative velocities indicate outside transport (out of the box) while positive values indicate inward transport (inside the box). Am I correct? If this is the case, what does negative advection mean in bottom waters?

Indeed, it is mentioned in the figure captions, velocities and tracer fluxes are « defined positive inward, so vertically upward » the boxes, but we will add this clarification in the text at the beginning of Section 3.1.1.

Moreover, « Bottom velocity » will be replaced by « bottom vertical velocity » as suggested by the referee in Minor Comment 10b.

4) Section 3.1.2 (page 7 and 8): This section is a bit difficult to follow due to its structure. Why not start this paragraph with the description of Figures 3e-h and

then introduce Figures 4 and 5? So mainly move "Off Cape Blanc, phytoplankton biomass....(Fig. 3d)." to the beginning of the paragraph. Then continue with "Phytoplankton biomass, averaged over ... (Fig. 3a)." and then focus on PP. In this case, I would plot figure 4c (phyto) in position 4a, figure 4a (PP) in position 4b and figure 4b (f-ratio) in position 4c. For consistency, I would also rename the title of the section as: "Phytoplankton biomass, primary production and phytoplankton fluxes". Perhaps. the paragraph will then need some rephrasing.

We do not agree with the reorganization proposed by the referee because we find more logical to go from primary production to phytoplankton biomass, than the contrary.

5) Figures 7 and 13 are very similar and it takes some time to understand their main differences, as well as why both are relevant. It will be good to stress out more the differences and mention why both are relevant to this work. It is also not very clear why the spring average is now more relevant than the annual average, and why spring more than other seasons (i.e. besides being the most productive, what are the advantages to show spring processes?). Is there a way to combine these 2 figures and their respective descriptions in a summary section?

This comment is related to comments of the 2 other referees who asked for introducing the choice of the spring period earlier in the paper.

The main justification is that observed offshore extension of Chl-a do present a marked seasonal variability with a peak in boreal spring. Therefore, focusing only on annual averages would have raised questions about the significance of our results during the time period that sees most of the offshore export. Choice has thus been made to show annual average but also the spring period.

Interestingly, there is very little change in the repartition of the fluxes driving nitrate and phytoplankton concentrations in spring compared to annual average, as shown by the little difference between Figures 7 and 13. We feel that this finding is a result on its own and would not appear as evident to a new reader. Even if *a posteriori*, Figures 7 and 13 appears as redondant, we feel that, *a priori*, a new reader would ask to see the seasonality of the fluxes we are discussing, particularly during the peak period. Therefore we feel that it is justified to keep those 2 figures unchanged.

However, we will stress the little difference between both figures and what it means in the Discussion Section 4.3, p.16/l.18 : « Nonetheless, our conclusions are also valid on annual average since the drivers of nitrate and phytoplankton biomass in offshore boxes are similar on spring and annual average (see Fig. 7 and 13). »

6) Section 3.2.1, line 32: What does "vertical velocities are pointing downward" mean in this context?

This sentence will be modified as follows : « The vertical nitrate supply by advection off the northern and southern Saharan Bank is particularly weak (inward nitrate transport despite averaged outward velocities due to episodic inward events) in comparison to vertical diffusion. »

7) Section 3.2.2, lines 20-25: Is there any other model or measurement data also showing the strong diffusion in this region? It would be nice to compare these results to published data or at least mention how reliable these results are.

Indeed, the strong vertical mixing in this region is found in the global climatology of mixed-layer depth (MLD) of de Boyer Montégut et al. (2004), as mentioned in the Discussion Section 4.3.

8) Section 4.1, lines 10-16 (page 14): Same as comment 7. Is there any model or measurement data to compare these results to?

To the authors knowledge, no data or model results are available to validate our findings. These are actually innovative model results that would require further observational studies, as it will be stressed at the end of this Discussion Section 4.1., p.14/l.14 : « In the NW African region, coastal topography effects and alongshore geostrophic flow (related to large scale circulation patterns) may noticeably influence the convergence/divergence of coastal water masses. They would modulate the coastal divergence driven by the Ekman transport, i.e. the response of coastal upwelling to the wind forcing. This modeling work stresses processes that are yet difficult to study with observations due to their scarcity. Therefore, this work strongly advocates for dedicated observational studies. »

9) Section 4.3, line 18 (page 17): Are the results of intrusion of nutrient-rich waters in the Senegalo-Mauritanian region in agreement with Lazaro et al., 2005 and Mittelstaedt, 1991? If so, it will be good to specify it: "originating from the Guinean upwelling. This is in agreement with previous finding of Lazaro et al., 2005 and Mittelstaedt, 1991." How do the results of Lazaro et al., 2005 and Mittelstaedt, 1991 compare to those of this paper?

This sentence will be clarified as follows : « Nevertheless, our model results indicate that the nutrient input is not only from the coastal region. Indeed, we identified a significant impact of transient southern intrusions of nutrient-rich waters in the Senegalo-Mauritanian region likely originating from the Guinean upwelling due to the presence of the Guinea Dome, a large scale cyclonic feature centered south of the Cape Verde archipelago (Lázaro et al., 2005; Mittelstaedt, 1991). »

10) Figure 14: Could these model parameters be compared to measurements, other model results or a theoretical average?

The strength and seasonality of surface horizontal currents is validated against a global drifter-derived climatology of surface currents (see Fig. 1). Concerning vertical velocities which we mainly attribute to the coastal upwelling, they fall in the range of observed (Benitez-Barrios et al., 2011) or modeled values (Mason et al., 2011) off Morocco (so north of our study region). To the authors knowledge, observational data are missing in the study region to validate our estimations. This will be mentioned in the Results Section, p.7/l.11 : « Observational data are known to be scarce in our study region. However vertical velocities fall in the range of the few studies that has been published with observed (Benitez-Barrios et al., 2011) or

modeled values (Mason et al., (2011) focusing on northern Morocco. »

11) Section 3.2.2 starts with "Annual mean PP", but it should be "Spring average PP..", I think.

Indeed, this will be modified.

12) Section "Discussion", line 11: This comment relates to point 5). Because it is not really mentioned before, I would shortly indicate, at the end of this paragraph, why spring processes are analyzed.

See response to comment #5.

13) Section 4.1, line 22 (page 13): "Therefore it gives us confidence..." how much confidence? Some, good, very good...? I would indicate it here.

No data for quantitative validation is available but we can infer qualitatively that the model represents well the surface circulation of water masses in the study domain. This sentence will be modified as follows : « The simulated spatial and temporal variability of surface circulation are in good agreement with the satellite-tracked drifters (see Section 2.2), so the model can be used to infer the factors responsible for the sensitivity of coastal upwelling to the wind forcing. »

14) Section 4.1, line 23 (page 13): "For this latter purpose, we further ..." This sentence does not really explain in what way the analysis of seasonal cycles at the edge of the coastal boxes will add confidence to the model. Especially because these sensitivity results are not compared to observations or theoretical behavior. Please be more specific about the importance of this analysis.

We think that the correction made on the previous sentence (see previous comment) makes it clear that the analysis presented in Figure 14 allows to « infer the factors responsible for the sensitivity of coastal upwelling to the wind forcing. » (i.e. the « latter purpose »).

15) Section 4.2, lines 33 (Page 14) and line 1 (page 15): Are these new results or do they include those of Barton et al., 2004, García-Muñoz et al., 2004 and Karakas et al., 2006? Please specify which part of these results are new or used to confirm previous ones.

To clarify this point, we modified the sentence from :

« The combined effect of this local growth and the high filament activity around Cape Boujdour (Barton et al., 2004; García-Muñoz et al., 2004; Karakaş et al., 2006) results in an offshore transport of phytoplankton-rich and nitrate-depleted water masses. »

to :

« The combined effect of this local growth and the high filament activity around Cape Boujdour (the latter point being documented in Barton et al., 2004; García-Muñoz et al., 2004; Karakaş et al., 2006) results in an offshore transport of phytoplankton-rich and nitrate-depleted water masses. »

16) Section 4.2, line 15 (page 15): "Zooplankton excretion..." What is the explanation of the zooplankton excretion being more important for regenerated production in areas with lateral transport?

We actually mean that the zooplankton excretion « participates to enhance regenerated production in areas where the lateral input of plankton biomass is elevated » because zooplankton biomass, and then zooplankton excretion, is in this case high where the phytoplankton biomass is high. Accordingly, we propose to modify the end of the paragraph from p.15/l.9 :

« Usually the regenerated production relies on high residence time favourable to efficient recycling (see Fig. 15a). However, the water masses residence time in the south Saharan Bank and off Cape Blanc is low and can not explain the high level of regenerated production. In this region the regenerated production is rather due to the remineralization of organic matter supply and zooplankton excretion (Fig. 5a). Note that the meridional variability of secondary production (grazing rate) follows that of PP (not shown) suggesting a bottom-up control of the phytoplankton biomass rather than a top-down control by zooplankton grazers. Zooplankton biomass and excretion activity are then enhanced when the plankton biomass is elevated, which is especially the case in the South Saharan Bank and off Cape Blanc. »

17) Section 4.3, lines 17-19 (page 15): "This supports the idea that, ..." Is this sentence in agreement or in opposition to the previous hypothesis? What do other authors say about lateral transport of organic matter in this area?

This sentence is in agreement with the previous hypothesis which is that regenerated production is controlled by lateral inputs of organic matter (either living or dead) rather than by residence times. Indeed, in this area, the regenerated production is precisely low in spite of high residence times. Accordingly, we propose to modify the first sentences of the paragraph from p.15/l.16 :

« In the Senegalo-Mauritanian region, only moderate regenerated production is found year round although residence time is relatively high with respect to the southern Saharan Bank and Cape Blanc areas. This supports the idea that in the southern Saharan Bank, Cape Blanc and Senegalo-Mauritanian regions, regenerated production is rather driven by the amounts of organic matter supplies through lateral boundaries than by high residence time. »

18) Section 4.3, lines 14-19 (page 16): "For this purpose, we focus...", this sentence should be briefly mentioned before as mentioned in comment 12, and here, it should be improve. Please clarify why this specific setting (maximal coastal upwelling at the Saharan Bank and maximum phytoplankton extension off Cape Blanc) is a good scenario to be tested with the model.

As indicated to answer to comment 12, the choice of the spring season for the analysis of offshore boxes will be motivated in the introduction, and at the beginning of the Results Section 3.2.1. Moreover, the sentence concerned by this comment will be improved :
« For this purpose, we focus on the offshore region when the maximum chlorophyll extension is found (i.e. the spring period,

Lathuiliere et al., 2008). In spring, maximum coastal upwelling is found off the Saharan Bank (Fig. 2c). Following the hypotheses of Lathuiliere et al. (2008), this should traduce in a maximum of phytoplankton biomass extension at the Saharan Bank latitudes. Instead, the phytoplankton biomass extension is found maximum off Cape Blanc and in the Senegalo-Mauritanian region, as attested by the meridional variation of phytoplankton biomass in the offshore region (Fig. 10). »

Minor comments/Technical corrections

We agree with most of the minor comments made by the referee, and we wish to thank his/her for his/her great effort. We only answer to a few comments either to justify a disagreement, precise our thinking, or indicate an important correction that will be made to the manuscript.

1) Section "Abstract" line 29: Did you mean "lateral advection transports coastal nutrients..."? **OK**

2) Section "Introduction" line 2: "... and seasonally variable one...". I would remove "one". **OK**

3) Section "Introduction" line 29: There is a missing comma, "Here, we ...", **OK**

4) Subsection "Model validation" line 22: Are AVHRR initials? If so, they probably should be written in full, here. **OK**

5) Subsection "Model validation" line 25: To ease the understanding of Figure 1, I would replace the titles "SeaWiFS/Drifters SVP" and "ROMS" by "Observations" and "Model", respectively. I could not figure it out what SVP means. Then, add in caption "from observation given by SeaWiFS satellite data... Same for ROM-PISCES model in..."

We prefer to mention the name of the datasets shown on the validation Figure 1 as it does not requires more space and provide more informations to the reader. Nonetheless, we will change the titles to « SeaWiFS/Drifters GDP observations » and « ROMS model ». The GDP (Global Drifter Program) acronym will be added in the figure caption.

6) Subsection "Model validation" line 6 and 7 (page 5): I would remove "see" from both "see Fig. 1" in this paragraph. I would also move the second fig. reference, line 7, to the end of the sentence: "both in the model and in the data during summer (Fig. 1)". This is, because the NECC is not entirely shown in the figure and creates some confusion if the figure reference is placed after mentioning the NECC. **OK**

7a) Subsection "Model validation" line 14 and Figure 1: If maximum values of chlorophyll in both satellite and model data go up to 10 mgChl m⁻³, why stop at 5 mgChl m⁻³ in the color bar of Fig. 1?

We chose to stop at 5 mgChl m⁻³ the color bar of Fig. 1 because there is increased overestimation of the satellite data with increasing

chlorophyll concentrations (Gregg and Casey, 2004).

7b) Figure 1: At first I could not see the 5 coastal areas. Maybe by coloring the lines in white, instead of black, the boxes will be more visible. In the caption of Figure 1, "The ten boxes" could also be replaced by "The 5 coastal and 5 offshore boxes", so it is even more clear that the 5 coastal boxes are shown in the figure. **OK**

7c) Missing figure: Please remove the sentences "Main surface currents and deep water masses over ... South Atlantic Central Water" or add the missing plot and describe it in the main text, but only if it adds relevant information to the article. **OK**

8a) Section 3.1, title: To simplify, why not just call it "Meridional variability in the coastal region"? **OK**

8b) Section 3.1, title: Maybe add "Annual average of the meridional variability in the coastal region"

We will change the title of Section 3.1 to « Meridional variability in the coastal region »

9) Section 3.1.1, line 7: "on the edge of coastal boxes" gives a vague description of what we are actually looking at in the figure. Maybe replace by "on each edge of the coastal boxes (i.e. North, South, West and bottom)". **OK**

10a) Figure 2: Are the "BOX" and the black lines needed for figure 2a and 2b? I think they do not add much information, unlike for Fig. 2c, 2d, 2e, and 2f. These plots (Fig. 2a and 2b) alone clearly represent the entire box. **OK**

10b) Figures 2-3: These figures are good, but they are a bit difficult to get at first (especially Fig. 2, since it is the first one of the series). To make Fig. 2 easier to understand, I would separate the main title "Wind and currents" to "Wind (m s^{-1})" and "Velocities (m s^{-1})". This will make it clear that this figure is showing 2 different things. I would write "Wind (m s^{-1})" on top of Figure 2a and 2b and "Velocities (m s^{-1})" on top of Figures 2b, 2c, 2d, 2e and 2f, as done for Fig. 3. Please use velocities instead of currents, because the word current is not as often used as velocity in the main text. Please also change the title of figure 2c to "c-Bottom (upwelling)" or "c-Bottom (vertical velocities)". The chosen title, "c-Bottom velocity" is misleading, as it suggests horizontal bottom velocities. **OK**

10c) Please check the sub-numbering of the figures, some do not follow a logic order. **OK**

11) Section 3.1.2, line 30: Please rephrase "... of PP in the boxes north and south of Cape Blanc are simulated, respectively." **OK**

12) Section 3.1.3, lines 26-30: This sentence is difficult to follow, maybe rephrase to "... the total rate of change of nitrate concentration and phytoplankton biomass in each coastal box are presented..." **OK**

13) Section 3.1.3, line 8: replace "is not anymore" by "is no longer" **OK**

14) Section 3.1.3, line 13: rephrase "from the northern Saharan Bank and the Senegal-Mauritanian region, respectively." **OK**

15) Section 3.1.3, line 31 (page 9): remove "tentative" **OK**

16a) Section 3.2, title: Same as comment 8: To simplify, change it to "Meridional variability in the offshore region". **OK**

16b) I think, here, the title of section 3.2 should also mention that the results are now only for spring: "Spring meridional variability in the offshore region". For me this was not clear until the discussion.

We agree and will change the title of Section 3.2 to « Spring meridional variability in the offshore region ».

17) Section 3.2.2, line 27: I would remove "finally". **OK**

18) Section 4, line 10: Please replace "we will seek to explicit" by "we explain" **OK**

19a) Section 4.3, line 10 (page 17): Space missing, please correct "that filaments" **OK**

19b) Section 4.3, line 14 (page 17): Please replace "hypothesis" by "processes". The points mentioned before are not hypotheses. **OK**

20) Figure 8: Same comment as 10a for Figure 2 regarding the black lines and the BOX. **OK**

21) Figure 11a and 11b are missing.

This should be « a- Bottom advection » and « b- Bottom diffusion », so this will be modified.

22) Figure 14: Why Fig 14c is suddenly represented with dashed lines? What do the plain and dashed lines represent in Fig. 14d? **OK**

23) Please check the usage of acronyms, especially in the legend of the figures. I would always use the full subregion names or always the acronyms. Personally, I would avoid the acronyms (also in the text).

We agree and will always use the full subregion names.

24) Section 4.3, line 8: Please correct "2ZOLTLathuiliere2008" **OK**

25) I would replace most "see Fig." by "Fig.", except maybe the one in line 24, page 4. **OK**

Further suggestions

We agree with all these further suggestions made by the referee, and we wish to thank her again for his/her great effort.

1) Section 4.1, line 15-16 (page 13): Rephrase "It appears that coastal upwelling" by "However, coastal upwelling". This makes it more clear that

this information comes from the literature and not the model results. **OK**

2) Section 4.1, line 20 (page 13): Replace "The simulated" by "Our simulated" **OK**

3) Section 4.2, line 4: "for 50 % of new production", please indicate if it is a model result or a general statement. Can this be seen in one of the figures? If yes, please indicate in which one.
This is shown in figure 6.

4) Section 4.2, lines 6-8: Would it be possible to compare this with observations of mixed layer depth (MLD) off Cape Blanc? If the MLD is large there, it will give some extra validation for this result.
At the coast off Cape Blanc, there is phytoplankton subduction by advection and not by diffusion, so this feature is not related to the vertical mixing.

5) Section 4.2, lines 9-10: I would replace "Usually" by "In general", and in the following sentence, I would add "However, in our results, the water" **OK**

6) Section 4.2, line 35 (page 15): The word "provide" is maybe not the best word, here. I suggest "lead to", instead. **OK**

7) Section 4.2, lines 1-2 (page 16): To ease the reading, I would briefly mention why this is the case, instead of referring to the previous section. Section 4.1 mentions several points and it is not exactly clear to which one this sentence refers to. I would also remove "(see above)", it is not clear to what it is referring to. **OK**

8) Section 4.3, line 27: Please add "In our results, the vertical mixing..." and replace "Indeed" by "This is even more visible in the results for spring, where the mixed layer...". **OK**

9) Section 4.3, lines 33-34: To better differentiate between the new results and published data, please rephrase the sentence as follows: " In fact, the vertical mixing, as previously suggested by Huntsman and Barber 1977, is also responsible for ..." **OK**

10) Section 4.3, line 1: Maybe replace "participate to" by "partly". **OK**

11) Section 4.3, line 7: To help the reader, it would be nice to briefly mention what is the hypothesis of Lathuilère et al., 2008. **OK**

12) Section 4.3, lines 8-9: To better differentiate between the new results and published data, please add " In our results, the advection by ..." and "...off Cape Blanc, in agreement with Kostianoy and Zatspein, 1996. **OK**

13) Section 4.3, line 10: To better differentiate between the new results and published data, please add "may enhanced cross shelf transport, as also shown by satellite data in Lathuilère et al., 2008". **OK**