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Discussions

Interactive comment on "What controls biological productivity in coastal upwelling systems? Insights from a comparative modeling study" *by* Z. Lachkar and N. Gruber

Z. Lachkar and N. Gruber

zouhair.lachkar@env.ethz.ch

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We would like to thank this reviewer for his comments, suggestions and remarks that have greatly helped to improve the quality of our manuscript. Subject to the editor's agreement, our plan is to submit a revised manuscript for publication in Biogeosciences.

Responses to Reviewer 2:

The paper addresses a relevant scientific question by wishing to give insights for explaining the differences that exist between the California and the Canary Current System in terms of biological production. Using a modeling approach,

authors provide convincing ideas to explain these differences. Nevertheless, the authors have only been able to explore some of the hypotheses, partly because they use a simple model which takes into account a single limiting nutrient (nitrogen), and they do not mention the other hypotheses put forward in the literature. I believe it should appear in the introduction as well as in the discussion.

Done. In the revised manuscript, we now mention and discuss the various hypotheses proposed earlier in the literature both in the introduction and in the discussion section. See our detailed response below.

The paper has an interesting architecture since the reflection of the authors looks like a scientific journey with the presentation of a successive and logical questioning. Nevertheless, this construction prevents the authors to write the discussion in a broader context. A larger perspective is missing to the manuscript.

Done. We added a new section (section 3.7, page 19), where we discuss our results in a broader perspective, in particular by reviewing the other hypotheses proposed in the literature. In particular, findings by Minas et al (1986), Feng et al (2000), Carr and Kearns (2003), Lachkar and Gruber (2011) and Gruber et al (2011) are discussed. Moreover, we also discuss in section 2.3 of the revised manuscript the strengths and weaknesses associated with our use of an NPZD-type ecosystem model and the resulting robustness of our findings with regard to the chosen model complexity (see page 11, lines 11-21). See also our responses to specific comments below.

Authors need to justify their temporal window in their lagrangian experiment which, to me, is susceptible to bias the result on the role of retention for explaining the differences between the two CS (which is one of the two main results of this study).

Done. See our detailed response to the specific comment below.

I believe the paper brings a significant contribution to the hypotheses put forward to explain the differences that exist between eastern boundary currents in terms of production and subject to the consideration of the above remarks should be accepted for publication.

Thanks.

Specific comments:

The title reflects the content of the paper. Introduction In the second paragraph of the introduction, authors should mention the hypotheses put forward by previous studies (e.g. Carr and Kearns, 2003; Chavez and Méssié, 2009) to explain the better efficiency of the Atlantic Current Systems in terms of primary production.

Done. In the revised manuscript, we review hypotheses put forward in Carr and Kearns (2003) and Lachkar and Gruber (2011) to explain contrasting productivities between Atlantic and Pacific EBUS. See the Introduction section, page 4.

Section 2.1.1: A word about the temporal scheme used is missing in the description of the model.

The time stepping is a leapfrog/Adams–Moulton, predictor–corrector scheme, which is third-order accurate in time. This is now explicitly stated in the revised manuscript (see page 5, lines 7-9).

You are using ETOPO to define your bathymetry but ETOPO is known to generate spurious features on continental shelves, the use of gebco1 would be more relevant for your areas of investigation.

Despite its more patchy look, we believe that ETOPO dataset is a better representation of ocean bathymetry than Gebco. This is because ETOPO is based on digital satellite gravity data, whereas GEBCO grid is based on hand-contoured paper maps that were later digitized. The paper maps used in the digitization were only contoured at 500 m intervals. This leads to issues such as flat spots (or “terraces”) that appear around

multiples of 500m (e.g., see Marks and Smith (2004)).

Section 2.1.2: Could authors be more precise about their choices of parameters, did they take the same values as Gruber et al. (2006) and did they take the same values for both current systems (CS)?

All biological parameters used in both the California CS and Canary CS simulations are identical to the ones employed by Gruber et al (2006). This is now clearly stated in the revised manuscript. See section 2.1, page 7, lines 8-9.

Section 2.2: It would be better to give the resolution in degrees rather than kilometers since the range of latitude is large enough for getting differences throughout the domain.

The Canary model has a horizontal resolution of 1/20^o. The grid of the California model is not a Mercator type of grid, but a curvilinear grid that follows the orientation of the U.S. West coastline with an average resolution of 5km. Therefore, in the revised manuscript we give the model resolution both in degree and equivalent distance in km for the Canary CS, and in km only for the California CS. See section 2.2, page 7, lines 19-21.

Authors should give the values of the deformation radius when referring to Chassignet and Verron (2006), especially since the effect of the resolution and the mesoscale activity is discussed in section 4.

We now give the average values of deformation radius in the Canary CS and California CS which varies locally between 20 and 60 km according to the climatology of Chelton et al (1998). See section 2.2, page 7, lines 21-22.

Section 2.3: A new evaluation step is indeed needed because the solution obtained in this 1/18th degree resolution experiment is significantly different from the one presented by Gruber et al. (2006), notably the chlorophyll distribution along the California Current System which is much more diffuse. Authors eval-

uate the model performance by looking at SST, surface chlorophyll and mixed layer depth. As the paper is about biological production, we could have wish some evaluation of the primary production because values are available in both CS (e.g. Kahru et al., 2009 for the California CS; Morel et al., 1996; Tilstone et al., 2009 for the Canary CS). This confrontation with in situ or satellite measurements of primary production could take place at the beginning of section 3.

Done. We now compare simulated NPP to Kahru's observation-based estimates for the California CS and to Tilstone in-situ observations for the Canary CS. See Table 1 and Table 2 and the discussion in section 2.3, page 11, lines 5-26.

For the surface SST, authors claim it is related to the AVHRR data used but don't mention other possible factors like wind forcing while the shape of the wind stress curl at the coast is crucial (e.g. Capet et al., 2004).

We agree that the shape of the wind stress curl is crucial near the coast as pointed out previously by Capet et al. (2004). However, this effect is restricted to the immediate nearshore area, whereas the cold bias of our model covers the whole domain (including the far offshore areas). As we think that uncertainties with the wind stress profile near the coast might also contribute to this temperature underestimation, we now mention this in the revised manuscript as a potential additional cause of the cold bias in the coastal zone (see section 2.3, page 9, lines 25-27).

Section 3.1: In this section, authors are wishing to relate the biological productivity to the upwelling intensity. For doing so, why do they plot the NPP as a function of the total inorganic nitrogen (TIN) which takes into account nitrate plus ammonium, the later coming from the remineralization of the organic matter ? NPP as a function of gamma(Nn,Nr) pause the same question. Upwelling intensity is given by nitrate contents, not the TIN.

NPP is the sum of new and regenerated production, with the latter representing that part of NPP that is driven by the assimilation of ammonium. Therefore, NPP depends

on the availability of total inorganic nitrogen = NH4 + NO3 and not only on NO3. Nevertheless, we investigated the relationship with nitrate as well. Considering the relationship between NO3 and new production, similar contrasts emerge between the two upwelling systems, i.e., Canary CS supports a larger new production than the California CS for a given NO3 concentration. The slopes of a linear regression of new production on the NO3 content vary, from 1.17 (±1.2) yr-1 for the California CS to 2.36 (± 0.7) yr-1 for the Canary CS. The slopes of a linear regression of new production on the NO3 limitation factor vary from 19.94 (± 3.62) mol C/m2.yr for the California CS to 44.65 (±8.7) mol C/m2. yr for the Canary CS.

Therefore, it appears that plotting NPP as a function of TIN or new production as a function of NO3 does not change the results discussed in this section. We opt for the first as the focus of this study is on NPP.

Section 3.2: The second paragraph treats one of the key point of the paper which is the light control of the production and the photoacclimation. Incoherences are present between Figure 6 and Table 1, I believe the columns corresponding to the normalized nutrient-replete growth rates to PAR, theta and T are mixed up and I couldn't tell to what the 50% difference mentioned in row 7 (page 5629) refers to.

We thank the reviewer for pointing out this problem. The titles of several columns were indeed switched in the original version of the manuscript. This has been fixed in the revised manuscript. As stated in the text, the difference between the maximum growth rates in the Canary CS and California CS which amounts to 0.18 day^{-1} gets increased to 0.31 day^{-1} in the theta-normalized case (which represents more than 50% increase).

Authors say why they normalized the nutrient-replete growth rate to C:Chl=25, PAR=20W/m2 but not why they chose the temperature 20C?

The 20^{o}C correspond to the average temperature in the central Canary CS in the top 40m. Yet, for more consistency, we now normalize the temperature to the central California CS conditions, i.e., to 14^{o}C, as done also for PAR and C:Chl,. This is stated in the revised manuscript (see section 3.2, page 14, lines 9-10) and this change is also reported in Table 3 and Figure 6.

Section 4.1: In this section, authors study the role of the residence time of water masses along the coast. This aspect is at the basement of the scientific results of this paper. In their lagrangian study, authors write that they release particles from April to August. Why not all along the year? If you release particles at this time of the year which is the end of the upwelling season in the southern part of the Canary CS and even cover the relaxation summer period, it is not surprising that the residence time of the particles are high along the coast, specially south of 20N where upwelling favorable winds are seasonal. This point needs to be clearly clarified.

Our initial choice of releasing the particles between April and August was motivated by the fact that this represents the maximum upwelling season in the central California, our region of reference. However, we do agree with the reviewer in that this might bias our comparison as the upwelling seasonality varies between the two systems, and even internally within each system. We therefore decided to run the Lagrangian experiments for the whole year. At the same time, averaging indistinguishably across the seasons can also be misleading as the impact of the residence times on the buildup of biomass is important only during the productive season. Long residence times outside the growing season have indeed little impact on biomass and production. In order to attribute a larger weight to the productive season, we weighted the residence times with NPP, when computing the annually averaged residences times. While the difference between the two systems gets slightly reduced relative to when the particle release experiments were restricted to the April-August period, we find that our results do not change qualitatively as the differences between the 2 systems remain substantial (more than 55% longer residence times in the Canary relative to the California, on average). This difference gets smaller but remains important (about 20%) when the time averaging of water residence times is done without taking into account the seasonality of production. This is now stated in the revised manuscript. (see section 3.4, page 16, lines 15-20). Finally, it is worth noting that the results of the sensitivity experiments remain unchanged when extending the Lagrangian experiments over the whole year.

Section 4.2: Authors mention different experiments with different bathymetric shapes, it would have been interesting to show them.

Done. We now show average profiles of bottom topographies used in the Canary CS 15km control and narrowed shelf sensitivity experiments. See Figure 11.

Section 4.3: The model used by the authors can bias the results stated in the first paragraph. Indeed, the model takes into account a single phytoplankton compartment. The assumption is that ammonium is taken up preferentially over nitrate. This assumption is not valid anymore when considering two phytoplankton compartments where diatoms can be parameterized and for which nitrate is taken up preferentially. The architecture of the model can then bias the results and the authors should discuss this point.

We disagree with the Referee on the detail of this question, as we don't know of any phytoplankton group or species (including diatoms) that would take up nitrate preferentially over ammonium. This is because from an energetic point of view, it is much less expensive to fix ammonium than nitrate. Nevertheless, we agree with the general spirit of this question in that the type and complexity of the ecosystem model can impact the results. We now discuss this point in the revised manuscript at the end of section 2.3, page 11, lines 11-21).

Our reasoning in this discussion refers to a couple of previous studies that show that: 1) when it comes to reproducing ecosystem bulk properties such as NPP or chlorophyll stocks for one single type of ecosystems (here, EBUS), NPZD-type models appear to be as successful as multiple functional group models (Friedrichs et al., 2007), 2) ecosystem model intercomparison show that physical processes drive most of the variability in production irrespective of the complexity of the employed ecosystem models (Friedrichs et al, 2006).

This gives us some confidence that the key results presented in this study do not depend substantially on the complexity of the ecosystem model, as the identified key mechanisms (i.e., the role of light and temperature as well as the residences times) are purely physically driven. We do agree, however, that the details of the ecosystem model can amplify or reduce the potential for these mechanisms to operate. We therefore believe that the overall qualitative picture will highly likely remain when using more complex ecological models while the quantitative details presented here may change. This is now discussed in the revised version of manuscript (see section 3.7, page 20, lines 4-10).

Finally, an additional support for our arguments comes from the results of a separate sensitivity experiment that we don't show in the paper, i.e., where we made 2 additional simulations at 15km for the Canary CS and California CS, with two phytoplankton species, one of them (hypothetical) has a preference for the nitrate over ammonium (mimicking what the reviewer suggested). Although these new simulations bring important quantitative changes including a general increase of nutrient utilization, new production, NPP, f-ratio, and export in both upwelling systems, the difference between the two systems remains unaffected, i.e., a larger NPP, a higher nutrient use efficiency and lower f-ratio in the Canary CS relative to the California CS.

Section 5: If one tests the mesoscale activity relative to the upwelling dynamics, the statement at the end of the third paragraph (page 5635, rows 5 to 7) is not completely fair since authors did not released particles at the fully developed upwelling season.

This statement is now more appropriate as our comparison of residence times is based on particles released year-round in the revised manuscript.

Technical corrections:

page 5619, row 15 : Brink, 1983a

Corrected.

page 5628, row 13 : twice "the" after "examine"

Corrected.

page 5633, row 26 : remove "in the two offshore regions"

Done.

Table 1: The reference to the "last three columns" is inappropriate in the legend.

This statement has been removed.

There is also a confusion in the columns for the normalized nutrient-replete growth rates to PAR, theta and T. According to Figure 6, the first one should refer to T, the second to I and the third one to theta.

The confusion in the titles of some columns has been fixed.

Legend Figure 6: line 4, twice "to" before "constant PAR".

Corrected.

Authors should check carefully their references, seven references are listed but I could not find them in the text : Bograd et al., 2008; Chan et al., 2008; Chavez and toggweiler,1995; Chavez and Messié, 2009; Feely et al., 2008; Kahru et al., 2009; Schwing et al., 1997.

References listed but not cited have been removed. Chavez and Messié, 2009 is now cited in the text (page 19, line 16).

Interactive comment on Biogeosciences Discuss., 8, 5617, 2011.