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Biogeosciences Discussions

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

Anonymous Referee #2

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General comments

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. 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. 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). 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.

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 andMéssié, 2009) to explain the better efficiency of the Atlantic Current Systems in terms of primary production.

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

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)?

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. 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.

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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 evaluate 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. 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).

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.

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

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 20°N where upwelling favorable winds are seasonal. This point needs to be clearly clarified.

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

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.

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.

Technical corrections

page 5619, row 15 : Brink, 1983a page 5628, row 13 : twice “the” after “examine” page 5633, row 26 : remove “in the two offshore regions”

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

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

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,

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1995; Chavez and Messié, 2009; Feely et al., 2008; Kahru et al., 2009; Schwing et al., 1997.

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