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Discussions

Interactive comment on "On the long-range offshore transport of organic carbon from the Canary Upwelling System to the open North Atlantic" *by* Elisa Lovecchio et al.

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This study uses a coupled physical-biogeochemical numerical model to investigate the export of organic carbon off NW Africa, from the coastal upwelling band into the deep ocean. The numerical data is used to carefully assess sources and sinks of organic carbon in the coastal and offshore regions, including the horizontal and vertical fluxes as well as primary production and remineralization. The exhaustive data analysis provides insight into the main mechanisms that control the sign of net community production (NCP) in a very extensive region off NW Africa. This manuscript represents a comprehensive study that should eventually become a work of reference for future research on coastal upwelling systems.

In my opinion, however, there are a number of important issues data need to be either solved or clarified before the paper may be accepted for publication. These relate to the following: (1) evaluation of the model's performance, (2) latitudinal partition of the domain, and (3) the role played by the supply of subsurface inorganic nutrients in the coastal and offshore upwelling regions. The final paper will also benefit from both better revision of literature, particularly on the circulation patterns in the Canary upwelling system (CanUS), and more concise and less speculative writing. I have no doubts that the authors can address these points, I encourage them to carry out the additional effort.

I will next explain in more detail the above major issues and later will comment on some other minor points that also require attention.

Major issues

(1) Evaluation of the model's performance

This is a very critical aspect and the authors dedicate a significantly long section, including an appendix, to evaluate the performance of the model. They compare the numerical output with observations using different datasets: the near-surface seasonal circulation as inferred from surface drifters; the annual-mean sea surface height, sea surface temperature (SST) and sea surface salinity; the annual-mean mixed layer depth (MLD); the annual-mean surface chlorophyll; and the annual-mean and seasonal-mean net primary production (NPP).

I value this effort very much but, honestly, at the end of the Evaluation section I have important doubts on how good the model's performance is. Throughout this section the authors recognize the existence of substantial differences between model and field data, and also talk about model bias. In Figure 2 they show the spatial distribution of model-data differences for several surface fields. The differences are not negligible at all, as clearly seen by the range of values in the mean fields and the differences, e.g. SST (range of values is 12°C and range in deviations is 4°C) and MLD (range of values is 100 m and range in deviations is 60 m). I particularly miss a comparison between the depth distribution of the modelled and observed particulate organic carbon (POC), which is of capital importance for this study. The seasonal results (Figure 5) show very large differences, possibly too large.

The authors end this section referring to a Taylor diagram presented in Appendix B (Figure B3), concluding that there is a "good correlation between the modelled and observed fields both in the annual and in the seasonal means." They show the Taylor diagram for the annual-mean results and for the mean of the seasonal results. The authors argue that the Taylor diagram shows results comparable to other studies for upwelling systems. Rather than comparing with other studies, it would be better to look at the statistics and discuss whether the results are convincing or not. For the annual-mean, for example, SST, CHLA and MLD respectively have a (normalized) standard deviation of about 1.2, 0.6 and 1.4, and a (normalized) root-mean-square difference of 0.3, 0.7 and 0.8. The authors should discuss whether these values are reasonable or not. I am particularly confused by Figure B3b: how is this calculated, just an average mean? What is the meaning? Wouldn't it be much better to show all four seasonal diagrams? It would also help to include, as supplementary materials, diagrams for each subregion.

(2) Latitudinal partition of the domain

In several places of the Introduction and Discussion the authors recognize that the Cape Verde frontal zone is a natural boundary between the subtropical and tropical domains. Nevertheless, for most of their analysis on latitudinal variability they use a partition in three areas or subregions, as shown in Figure 3b, which is not properly justified. I imagine this is done as an attempt to grasp the character of the meridionally convergent region near Cape Blanc but, as it is clear from the velocity fields in Figures 3 and 5, this is not correct. In my opinion only the southern subregion would comprise an area with approximately coherent dynamics.

My suggestion here is to use four subregions of different size: the northern one (25-32°N) would correspond to an area with substantial mesoscalar activity, with eddies and filaments generated both south of the Canary Archipelago and at the upwelling front; the second area would represent the permanent and intense central upwelling region (21-25°N); the third are would concentrate on the convergent region immediately south of Cape Blanc, which is the root of the Cape Blanc giant filament (about 17-21°N, though these limits change with longitude); the southern area (9.5-17°N) would correspond to the tropical region. Right now most of the discussion is either on the results for the latitudinal-average picture or (to a lesser degree) for the three proposed regularly-spaced subregions. With this alternative partition, the paper would certainly become much more informative.

I value very much the authors' efforts to provide bulk figures for the entire region but I think that plotting these results may be very misleading. For example, the data in Figure 8 suggests that the zonal flux of organic carbon is more intense than meridional one. I doubt this very much: in my opinion this is only an artefact that the latitudinal average tends to cancel the contributions of the southward Canary Upwelling Current and northward Mauritania Current and Poleward Undercurrent (please see references below regarding the main currents in the CanUS). My suggestion is to produce fewer plots on the results for the entire region (Figure 9 is fine but some other plots may be replaced by tables) and instead show what is happening in each area: the CanUS is so large that it surely deserves a closer view for each subregion.

(3) Upwelling of coastal and offshore inorganic nutrients

The coastal upwelling region is a source of inorganic nutrients to the surface layers in the coastal transition zone that are later exported offshore (e.g. Pelegrí et al., 2006; Pastor et al., 2008, 2013). Such a flux of inorganic nutrients is a prime element in the offshore net primary production and the sign of the NCP north of Cape Blanc. However, this issue is not mentioned in the manuscript until the Discussion. The subject is important enough to deserve careful attention when examining the sources and sinks for

NPP, it is the difference between new production using the subsurface load of inorganic nutrients or production after remineralization.

The offshore waters in the southern subregion are also largely affected by the presence of upward Ekman pumping, i.e. offshore upwelling resulting from positive wind-stress curl. Again this is an important aspect in the dynamics and NPP balance of this subregion, which is again acknowledged very late in the manuscript and only partly discussed.

The model could be used to assess these different contributions. Perhaps this was not the objective of the authors, which is fine, but then the potential relevance of the upwelling and transport of inorganic nutrients on the NPP and NCP within the entire region should be properly discussed since early in the manuscript.

Minor points

(4) p 1, l 10: divergence or convergence?

(5) p 2, l 11: replace “and can create” by “can create.”

(6) p 2, l 15: “Arístegui.”

(7) p 3, l 5: other relevant references are Pelegrí et al. (2006) and Pastor et al. (2013).

(8) p 3, l 24: also Pastor et al. (2013).

(9) section on Methods: how does the model calculate the vertical velocity?

(10) caption of Figure 1: Gran Canaria is cited in the caption but not located in the map.

(11) p 10, l 10-13: please clarify.

(12) caption of Figure 4: VGPM is first mentioned here but it is defined nowhere in the manuscript.

(13) p 12, l 21-24: asides the Canary Current and the Mauritanian Current you should also probably refer to the Canary Upwelling Current (associated to the coastal up-

welling jet) and the Poleward Undercurrent (please see references below).

(14) p 12, l 31-32: “. . . NPP is a better measure than chlorophyll for evaluating. . .”

(15) p 13, l 1: Pastor et al. (2013) is probably a better reference.

(16) caption Figure 6: panel b also includes sediment remineralization?

(17) p 18, l 3-5: are you using two different definitions for excess export?

(18) p 20, l 1-2: here and elsewhere it is best to not refer to lines, they should be defined in the figure’s caption or legend (otherwise you would have to define them everywhere).

(19) p 20, l 27 and 33: “north of the Cape Verde front. . .”

(20) p 22, l 2: “. . .south of Cape Blanc.”

(21) please revise caption of Figure 12.

(22) Figure 13: I suggest that you also show the meridional fluxes.

(23) caption Figure 13: “vertical” rather than “vertcal.”

(24) p 24, l 14: “(Figure 13).”

(25) p 25, l 6-8: this is likely an artefact of the SW-NE orientation of the coast.

(26) p 25, l 10 and 15: please include references.

(27) p 27, l 8: see also Pastor et al. (2013).

(28) p 28, l 7: “these.”

(29) p 28, l 10-18: usage of so many conditionals raises doubts on the reader.

(30) p 28, l 27: remove “from Section 4.1.”

(31) p 28, l 28: is this the right way to cite a figure within a reference?

(32) p 29, l 3: here and elsewhere separate numbers from units, i.e. “2000 km” rather

than “2000km.”

(33) p 29, l 16-17: please revise writing.

(34) additional references: asides those mentioned above, there are other works that would help better describe the circulation patterns in the CanUS, such as Mason et al. (2011), Peña-Izquierdo et al. (2012, 2015), Pelegrí and Peña-Izquierdo (2015), Pelegrí and Benazzouz (2015).

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