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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.

Anonymous Referee #1

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Review of the manuscript entitled "On the long-range offshore transport of organic carbon from the Canary Upwelling System to the open North Atlantic" by Lovecchio et al.

This study focuses on the offshore transport of organic carbon from the very productive regions of the Canary Upwelling System (Can US) to the central regions of the Atlantic Ocean. This study is based on a model configuration of the whole Atlantic Ocean using a telescopic grid which allows to have an eddy-resolving scale over the CanUS region. The authors use a simple NPZD model with two sinking POC pools (a slow sinking and a fast sinking pool). The model does not represent dissolved organic carbon. The model predicts an intense offshore transport of organic carbon which extends

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beyond 1500 km into the interior of the subtropical Atlantic Ocean. Nevertheless, the upper ocean remains autotrophic all over the domain. When integrated, NCP is net heterotrophic everywhere except in a very narrow coastal band. The central region near Cape Blanc is very efficient at collecting organic carbon and at transporting it offshore. This explains the large net heterotrophic characteristics of the region, which extends far offshore into the interior of the subtropical Atlantic Ocean (even further offshore than 2000 km).

This study is really interesting and clearly demonstrates the critical role organic matter plays in the lateral transport of carbon and nutrients from productive coastal upwelling systems to offshore more oligotrophic areas. Furthermore, this study emphasizes the regional differences in the CanUS area with the central part being an efficient collector of organic matter which is exported very far into the subtropical Atlantic Ocean. I really appreciated the detailed budget as well as the quite insightful diagnostics. So, I definitely think that this study deserves publication in Biogeosciences. However, I have two major concerns that would need to be addressed, or at least discussed.

The NPZD model is very simple which is not a problem by itself. It performs enough well to be suitable for that study. This is quite clearly shown in the validation section of the paper. However, according to me, it lacks a critical reservoir especially concerning the objectives of that study: DOC or more precisely semi-labile and semi-refractory DOC. Concentrations of semi-labile DOC range from typically 20 to 40 umol/L in the upper ocean (Hansell et al., 2009; Hansell and Carlson, 2014). Its lifetime is also quite long and ranges from weeks to years which makes it possible for that pool to be transported far away from its production region. It has been shown to potentially play an important role in the subtropical gyres (e.g., Roussenov et al., 2006; Torres-Valdés et al., 2009). In the present study, this pool is omitted and thus, a potentially large contribution to the lateral export of organic carbon is not represented. This needs at least to be discussed in the discussion section.

In this study, the importance of mesoscale features is emphasized several times but

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never clearly quantified. It would have been nice to have such a quantification. I would suggest two possible means to do that: 1) to perform a classical separation technique between the mean and eddy components of the transport; 2) to perform a simulation in which the non linear terms in the Navier-Stokes equations for momentum iare cancelled such as in Gruber et al. (2011). Otherwise, any discussion of the effect of the mesoscale circulation remains quite speculative and qualitative.

I have some more specific comments which are listed below:

Page 2, line 1 - "resuspension of bottom sediments and can create ... " I guess something is missing in this sentence.

Page 4, line 33 - In the list of state variables that are listed, you should add O2.

Page 5, lines 11-12 - Phytoplankton can coagulate with small POC to form large POC. Is it also the case for small POC with small POC?

Page 7, lines 28-33 - You should refer to figure 3 to illustrate the different regions.

Page 10, lines 8-13 - Almost everywhere, except near Cape Blanc, high values of Chlorophyll are too narrow and too much trapped near the coast. As mentionned by the authors, this bias is especially strong in the Southern part of the CanUS domain.

Page 11, lines 1-6 - The authors here discuss the characteristics of the modeled subsurface maximum of Chl (DCM) and they refer to Figure B2. This is not always easy to see from Figure B2. The most obvious bias that emerges from the figure is the too high values of Chl at depth below 50m. Otherwise, it is hard to quantify from that plot the depth of ithe DCM in the model and in the data.

Page 17, lines 3-6 - For sure in the interior of the ocean, the contribution of small POC to the vertical sinking flux of organic matter should drop very quickly with depth. A figure showing the contribution of the different pools of organic matter to total organic carbon would be nice.

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Page 18, Figure 9 - The fluxes in the different boxes are not balanced (the imbalance is however small). Is it because the model is not fully at steady state or because of the internal variability related to the mesoscale activity?

Page 18, lines 1-6 - The DeltaE diagnostics is interesting. It accounts for two processes that can increase the export without changing the NCP: 1) The organic matter that is being transported laterally and that sinks out of the upper ocean increases the export and thus DeltaE. 2) The organic matter that is being transported laterally and that remineralizes in the upper box. This stimulates the biological activity which produces more organic matter which is sinks out of the upper ocean. In that case, NCP is not changed (the increase in PP compensates for the remineralization of the laterally supplied organic carbon) and export is increased which increases DeltaE. This two mechanisms should be explained here, especially because in the discussion section it is shown that the second process dominates.

Page 20, line 22 "and quantify the contribute of the different zonal bands ..." I guess it should be contribution

Page 22, line 25 "and quickly channell water ..." It should be channel.

Page 24, line 8 "becomes particularly important the offshore waters" Some words are missing here.

Page 25, lines 18-20 - The splitting between the contribution of the mean flow and of the eddy transport is not really clear here. See my second major concern above.

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