Review of manuscript #BG-2018-54 entitled "Mesoscale contribution to the long-range offshore transport of organic carbon from the Canary Upwelling System to the open North Atlantic" by E. Lovecchio et al.

General comments:

The authors of the above manuscript used regional modelling to study the mesoscale contribution of the long-range cross shore transport from the eutrophic upwelling to the adjacent oligotrophic gyre, focussing on organic carbon in the Canary Upwelling System. They used two different, yet complementary, methodologies to do so: the classical "turbulence-based" method (Reynolds decomposition) and the more recently developed "structure-based" technics (e.g. eddy- and filament-tracking algorithms). Based on the Reynolds decomposition, they analysed both the mean and turbulent fluxes in several subsystems and in several offshore bands; then they compared these to the estimates derived from the "structure identification" methods. Interesting correspondences emerged and they concluded with the importance of the mesoscale transport in the lateral export of C-org from the coastal upwelling to the open ocean. Integrating the results from both methodologies, they further showed the prominence of filaments and eddies (acting over different regions and at different levels) as a source of carbon and nutrients for the offshore oligotrophic waters.

The topic is highly relevant and the paper is clear in its objectives and its presentation (well written and well referenced). It contains many interesting results that have been thoroughly discussed, and promising conclusions; overall, I liked the paper and I would recommend publication providing that my specific comments are addressed. The suggestions below should help improving the article and the figures, polishing the text and correcting remaining typos.

In addition, my only real concerns with this paper is its length. This contribution is significantly long (I acknowledge it contains many interesting results but some are a bit redundant) and the writing is slightly too didactic sometimes (it could often be more "to-the-point" and more condensed). For instance, it seems weird to have both "Summary and Synthesis" and "Conclusions" sections in the same article (although I enjoy the reading of both). I agree it has a lot of content (complex analyses & interesting well-supported results) but it may be "lost-at-sea" considering that, nowadays (and unfortunately), papers are more-and-more numerous and less-and-less read by the community. Their work could gain in visibility if they opt for shorter and/or separated contributions. I would suggest the authors to try reducing the length of the article (i) by focussing their analyses and the writing and (ii) maybe by keeping aside some content for another paper (especially the regional analyses over the three subsystems -keeping here only the whole CanUS- and the seasonality of the processes, cf. a comment below).

Overall, I recommend the publication of this manuscript in BG providing the authors revise their manuscript to address the points raised here.

Specific comments:

- Typing/English mistakes: p 1 line 19: "anticyclonic" has been perhaps forgotten; p 5 line11: delete "the"; p 6 line 19: filaments; p 9 line 4: delete "eddy"; p 11 line 2: delete "abundant" or "found"; p 11 line 25: delete "a"; p 16 line 11: could it be "going deeper than 500 m within 500 km from the shore (or over 0-500 km offshore)"; p 17 line 8: "which slow down the rotation…"; p 18 line 14: should it refer to Fig. B7 and/or 8 instead of Fig. B6 there?; p21 line 1: estimateS; p 22 line 33: responsible OF a; p 30 line 18: cyclonEs; etc..
- Page 2 lines 9-10: there exist plenty of earlier references to support this statement, including some that are already cited, such as Rossi et al. 2008 and Gruber et al. 2011.

- Page 2 lines 31-32 and page 30 lines 32-33 + page 31 lines 1-2: the "coherence" of eddies is still an open question to my mind; there is a bunch of papers around those questions, both from the observational and modelling point of views, and many situations (from very coherent to quite leaky) have been reported; so it seems no agreement has been reached yet. Please rewrite.
- Page 4 line 11: in addition of in-situ observations and modelling studies, there also exist purely satellite studies (e.g. Capet et al. 2014, not yet cited) as well as merged modelling & satellite study (e.g. Hernandez-Carrasco et al. 2014, already cited). Capet et al. 2014. Implications of Refined Altimetry on Estimates of Mesoscale Activity and Eddy-Driven Offshore Transport in the Eastern Boundary Upwelling Systems, Geophysical Research Letters, 41 (21), 7602–7610, doi:10.1002/2014GL061770. It could also be cited at page 6 line 27-28 (assumption of "prismatic" volume) and page 30, line 25.
- Page 5 lines 12-15: the turbulent field (e.g. mesoscale activity) exhibits temporal variability, at monthly and especially seasonal time-scales. Why did you include the monthly/seasonal variability of the fields into the mean fluxes? I presume that this choice could impact your results about the "turbulence-based" methods, as well as when comparing it against the "structure-based" approach (has it been done similarly?). These additional analyses could be kept for future work anyway.
- Page 6 lines 18-20: Why is the reference mean SST field coarsened onto a 2x2°grid? This coarsening procedure is not applied to the snapshots, right? What could be the impact of a different coarsening? How was the threshold of -0,3°C defined? What is the sensitivity if your results to these threshold?
- Figures 3 and 4: for ease of understanding, please report in the caption that the unit of some panels is "number of eddies per 1 degree bin per day" (if I understood correctly).
- According to their specific behaviours showed in Fig 7 inserts, it would be interesting to see the equivalent of Fig. 8 but for the different subsystems (to be kept for future work I suppose).
- Page 18 line 5-7: Were these velocities estimates taken from the Hovmöller plots of Fig. 10 or from a direct check of exemplary velocities modelled at the boundaries of those structures? If these propagation speeds (for each structure type) were derived from the Hovmöller of Fig. 10, are they in accord with those derived from Fig. B6?
- Fig. 12: inconsistency between the caption and the plots displayed. I am also wondering if the explanations described lines 1-14 p 20 were obtained after having analysed the simulated fields of a few typical structures (if so, please show some figures in Appendix)? If not, I presume those lines of text are rather "discussion", so that they should have more references and they could be transferred somewhere else.
- Page 21 line 10: I found that the expression "coastally confined" is a bit misleading to describe a process prominent over a region extending from the shores to more than 500 km offshore; maybe "shelf-confined"? (although the shelf might be even narrower than this…), else?
- Page 25 lines 7-11 and page 26 line 4: to further support those arguments, you could refer also to Rossi et al. 2013 (already cited) who documented (from in-situ observations in the Iberian Peninsula Upwelling) a filament of 50-60 m depth which experienced subduction when moving offshore (see their sect. 3.2.2 and 3.6).
- These "intermittent hotspots of downwelling" are puzzling; I wonder what mechanisms are involved and why are they so transitory?
- Page 26 line 10: compilations of offshore transport due to upwelling filaments were also reported in Sanchez et al. 2008.Physical description of an upwelling filament west of Cape St

Vincent in late October 2004. Journal of Geophysical Research—Oceans, vol.113, C07044, http://dx.doi.org/10.1029/2007JC004430.

- Page 29 lines 3-9: why maps of chlorophyll are mentioned here? I thought that your structure detection algorithms were indeed applied to modelled SST maps only, is this right? Or did you check also consistency with chloro maps? Please clarify.