

Interactive comment on “Large to submesoscale surface circulation and its implications on biogeochemical/biological horizontal distributions during the OUTPACE cruise (SouthWest Pacific)” by Louise Rousselet et al.

Anonymous Referee #2

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Review of “Large to submesoscale surface circulation and its implication on biogeochemical/biological horizontal distribution during the OUTPACE cruise” by L. Rousselet et al.

This paper aims to highlight the role of large to submesoscale surface circulation on biogeochemical/biological horizontal distribution during the OUTPACE cruise. This cruise, held in Feb-April 2015, was dedicated to obtain a precise representation of the complex interactions between planktonic organisms and the cycle of biogenic elements along a zonal section in the WTSP ocean crossing contrasting environments

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(from oligotrophy to ultra-oligotrophy). Results from this cruise are being valorized and some papers are already published. Moutin et al. (2017) described the hydrological and dynamical context of biogeochemical sampling. De Verneil et al. (2017) analyse a surface chlorophyll a bloom by combining in situ and remote sensing datasets. They characterize the role of the physical circulation, and in particular the role of surface mesoscale circulation responsible for the bloom’s biogeochemical properties. This new paper wants to put the OUTPACE cruise into a more synoptic view by looking at surface satellite data in order to illustrate the role of large scale as well as meso/submesoscale dynamics on the biogeochemical/biological horizontal distributions of OUTPACE data. This is clearly a worthwhile exercise, and the authors have done a substantial work by compiling different satellite data set, and by using original diagnostics. The paper is well written, easy to read. The description of surface conditions by satellite data confirms most of the existing literature. But at first order, the motivation of this study is to do the link between this synoptic approach and the OUTPACE zonal section. It is not an easy task, and as a consequence most of the results are highly speculative. It is not really discuss what is the new contribution of this paper in comparison with the other existing papers dealing with the OUTPACE data. So, at this stage I am not convinced that the materials presented are strong enough to justify a publication. My general comment is to not recommend the paper for publication until an effort was done to better give evidence of the results that motivate this paper.

In the following I address some remarks wishing that it could help the authors to revise their manuscript.

I.4: “several studies indicate a strong mesoscale variability due to barotropic instabilities and the interactions of the major currents and jets with the numerous islands of the region (Qiu et al., 2009; Hristova et al., 2014).” Barotropic instability doesn’t appear as the only source of instability in the WTSP. If barotropic instabilities are related to the NVJ/CSCC/NCJ system (at 16°S) in most places baroclinic instabilities prevail as in the STCC-SEC system (Qiu and Chen, 2004; Hristova et al., 2014).

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General comment: A great number of satellite data are used. Four different altimetry-derived velocity product are tested: The classic Ssalto/Duacs product that provides maps of absolute geostrophic velocities at $1/4^\circ$ of resolution, a similar product at higher resolution ($1/8^\circ$) that may include an Ekman component, and a cyclogeostrophy correction. The authors identify the last product as the most accurate in situ surface currents with regards to the trajectories of the SVP floats launched during OUTPACE. It is not surprising than adding the Ekman component improve the comparison with surface drifters. The cyclogeostrophy correction must improve the current associated with eddies by taking account centrifugal acceleration. In my knowledge, it is a really new altimetric product and it could be interesting to illustrate the improvement brought by this correction. On this part, I am reserved because we don't know what are the respective contributions of high resolution, Ekman component, cyclogeostrophy. The result is based by looking at trajectories of numerical particles launched at only the three particular locations where SVP floats were deployed. Besides the relative limited number of locations for the validation, it is difficult to validate such conclusion based only on Fig. A1. If the LDC plot provides a good qualitative agreement, it is not the case for LDA and LDB.

p.6 l.29: "It is clearly more interesting to use CLS data instead of commonly used AVISO geostrophic surface currents because they include the wind effect and cyclogeostrophy with higher resolution, as well as better represents in situ data." Okay, I agree that the last product should represent in a better way the surface circulation. The authors argue that it is the most interesting data set. I am not against this opinion but it depends on the objective. Is it really interesting to add an Ekman component when you are interested by mesoscale features? The Ekman velocity is a strong component of the surface circulation but it is limited to the thin Ekman layer. So what is the importance of transport in such layer with regard to OUTPACE objectives where processes on the vertical must be crucial and partly related to submesoscale features? In my opinion, it lacks a discussion on the interest of such product for your objective.

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p.7 l.1: Figure 4 is cited before Fig. 3

Table 1: For me it is mysterious why the statistics of meanders vary so much between backward/forward experiments. Any explanation?

General comment: The authors have used Lagrangian approaches based on the recent LAVD method, and the combination of OW and RP parameters used in d'Ovidio (2013). They are very interesting methodologies as they ensure that eddies correspond with trapped waters. It seems more robust that the more classic linear parameter used in Chelton et al. (2011). As it is written both methods are in good agreement so what is the advantage to mix both methods? There is always some subjectivity when defining eddies, also when looking at Fig. C2, it seems that the RP contours are more in agreement with the velocity field. If one result of this paper is to test different methods to detect and track eddies, it could be interesting to test the methods classically used based on sea level extrema (Chelton, 2011; Chaigneau et al., 2009, Isern Fontanet,).

FSLE results are compared with surface gradient measured from the in situ OUTPACE data. Because the OUTPACE section is zonal, the corresponding surface gradient is mainly representative of cross track fronts. This aspect that limits the comparison of the two datasets is not mentioned.

p.8 l.7: "using altimetric..including the wind effect" and the cyclogeostrophy correction?

P.8 l.14: "or an artefact.. due to the short averaging.." It seems in contradiction with the sentence in section 2.3.1: ".. these simulations ensures that the use of a one year time period doesn't significantly modify statistical outputs". Also, with a RT of 15 days max for eddies it would be surprising that mesoscale dynamics have a strong influence on the mean circulation.

p.8 l.15: "The meridional transport does not correspond to any surface current but is mainly due to the south-easterly trade winds. This meridional component appears due to the addition of the Ekman component in altimetry surface velocities (Fig.B1, Supple-

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mentary Material). The large scale transport of surface waters in the OUTPACE area is thus a combination of the transport by general well-known surface currents and wind-driven circulations.” What are the dynamics which refer to these “surface currents”?? This sentence is a little bit ambiguous. If the “surface current” refers to geostrophic balance, it is normal that it doesn’t take account for the well-known poleward Ekman transport. Now if we look at the circulation inferred from Sverdrup theory, it is not so different of the streamfunction in Fig.2 despite its depth integrated estimation.

P.8 l18: “surface waters travel from northeast to southwest” Fig. C1 shows that most of the waters of importance for OUTPACE comes from the north and the northeast as written in l.22.

p.8 L27; “west of 170°w” → East of??

P.8 28: “.The wind-driven surface transport highlighted here could participate in the biogeochemical variations between western and eastern waters. Indeed the path through the Melanesian area may enrich these waters due to the contact with multiple islands whereas waters that directly recirculate within the gyre keep their ultra-oligotrophic characteristics. “ The authors here want to do a link between the wind driven circulation at the surface and biogeochemical variations. It seems very speculative because their argument is the path through the Melanesian area that is also valid for what is named “surface current”. Also, what is the role of these surface waters in the biochemical properties against deeper water and vertical processes??

p.9 l.4: “the OUTPACE cruise took place during an El Niño phase but they determined that climatological effects, upon the results of the cruise, were minimized.” The trade winds are very sensitive to ENSO conditions in the WTSP. So the authors argue for little effects of the wind driven circulation upon the results of the cruise. So, it seems to be in contradiction with the fact to use the altimetric product including the wind effect. Also, the authors argue the interest to investigate the mesoscale circulation. I am not sure that the altimetric product they used is well suited for such purpose. At first order,

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meso and submescale are driven by internal dynamics.

p.9 l. 20: “If the major part propagates westward, the meridional band between 180°W and 170°W is identified as a region with mostly eastward propagation of mesoscale structures.” Based in Fig.3, it is very hard for me to see propagation. This result is developed in the next sentences to argue for the importance of mesoscale dynamics to transport enriched-fluid into nutrient-poor gyre waters. But this eastern propagation is not really shown and as said by the authors there are no in situ data to illustrate this point. At this stage the discussion is highly speculative.

P.10 l. 25 “enhanced by the mesoscale transport” Fig.4 shows an eastward transport at LDB but the link with mesoscale is not obvious. Are there particles trapped into eddies that propagate eastward until LDB? And what their retention time and their distance travelled?

P.11 l. 7 “correlations” It is not really a correlation here

P.11 l.25; “These latter results also exhibit that an FSLE existence does not necessarily create a gradient but probably needs pre-existing tracer gradients and a lifetime longer 25 than few days.” The orientation of the front against the direction of the density gradient could be also an explanation?

Fig.6. It should be better if the axis in Fig.6b is in ° (as in Fig. 6a) despite than in km.

Section 3.4 is the section that best fits with the objective of the paper highlighted in the title. It shows two interesting case studies of interaction between fronts and biogeochemical properties. In my opinion, it is the most interesting part of the paper but it is only 1 page. It is regrettable that such results are not discussed with regard to the results of De Verneil et al. (2017). Do strong density gradients correspond with the FSLEs discussed LDB and LDA?

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