

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.

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We would like to thank the Anonymous Referee 1 for his review. In the following we respond to the reviewer’s comments and we follow most of his proposition in order to improve our work and its presentation. For clarity, the reviewer’s remarks are copied in bold.

In seeking to explore causes and consequences of spatial gradients in the Western Tropical South Pacific, as part of OUTPACE, this manuscript tackles the role

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of physical advection at different scales. More specifically it investigates how the circulation at large, mesoscale and submesoscale relates to the observed biogeochemical and biological fields. This is important context for the OUTPACE experiment and should be published. There are issues I'd like to see addressed before I can recommend this. The most significant issue is that the authors overstate the robustness of their results. It would always have been a difficult task to interpret the influence of the physical circulation at all scales studied given the linear nature of the cruise.

Indeed, we agree definitely with your point but, if it is very difficult to fully explore the influences of the physical circulation considering only data collected during the cruise combined with satellite data, it remains one important step to explore before using models and build other observational strategy. Here we wanted to give the most complete picture as possible, given the available data from the cruise, of the circulation at scales that are known to play a major role and their potential influence on biogeochemical variabilities. This, of course, leads to make some assumptions that are not verified yet in this paper. We agree to pay attention in separating the observations part and the hypotheses made to explain them as well as the potential influences raised by these observations.

The claim of demonstrating 'the influence of fronts in controlling the distribution of bacteria and phytoplankton' on the basis of 2 transects and a weak (but admittedly significant) correlation is optimistic. An example of this is the interpretation of Fig 6 on page 12 (lines 21-23). The Chl and other fields decrease over a much larger area than the location of the "tip". The "tip" could presumably have occurred anywhere along this section of decreasing biological fields with the authors drawing the same conclusion.

We agree with the reviewer that the focus should not be on the tip of the FSLE as, also considering the resolution of the satellite data, the "real" physical barrier might be offsetted by a few kilometers. What is interesting in this studied case is that the FSLE

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delimit a region of relatively high abundance of organisms and a region of relatively low abundance on each side of it. In section 3.4, we describe and discuss what appears to be the clear influence of fronts in the two case studies. These are two examples of the potential influence of fronts on the horizontal phytoplankton distribution. We are not suggesting that every front will lead to a separation of phytoplankton community, but that some of them can. Here we propose to rename section 3.4 to “Example of physical barriers’ influences on phytoplankton community” and to add the following sentences to help the readers to clearly understand the conditional form on such influence: “In this section we present two case studies that highlight the potential influence of fronts on phytoplankton horizontal distribution. To test the hypothesis of Bonnet et al..”.

The combination of Results and Discussion risks being misleading as in several places statements based on direct observation are followed by conjecture written in a similar direct way, sometimes with neither data nor further analysis nor reference to support them e.g. discussion of El Nino and winds at end of Section 3.1 (“data not shown”), “eddy-eddy interactions might be responsible for the emergence of complex paths” on page 10 (line 11), lines 13-18 on page 10, talking of microbial growth with no observations of it on p13 line 9, “isolating areas with different biogeochemical characteristics” on line 17 on page 11. The latter in particular is over-played. Figures 6 and 7 are interpreted as showing coincidence of FSLE and organisms or segregating organisms but this might be guided by the eye of the faithful. Interpreting a “relatively better correlation” (page 11, line 27) as evidence for “not randomly distributed” with 75% of cases still not showing a match is another example as the upper threshold of 25% that is possible from satellite altimetry comes with no evidence to support it. I don’t have a problem with conjecture as I think it’s an important means of directing future research, but I would recommend pushing analysis further to back these thoughts up and either splitting Results and Discussion or else making much clearer where the reporting of observations ends and speculation begins.

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We understand the reviewer's concern about the combination of Results and Discussions together as this question was raised during the article scripting process. In consequence, we decided to describe the circulation from large to submesoscale through a descending approach and it was a difficult task to split the Results and the Discussion into two different sections as the Discussion will refer to different sections of the Results part. To avoid the reader a back and forth exercise between what was described in the Results and what was suggested in the Discussion, we believed (and still do) it is more convenient to directly discuss the results highlighted. So we tried to be extremely rigorous with the tenses: using the direct way to talk about the results and the conditional way (could, may, might...) to talk about the potential influence of the observations. However we understand that some parts, listed by the reviewer, were still suffering from a lack of clarity. To avoid confusion, we propose to clearly split the results and discussions into different paragraph in each existing subsections (3.1, 3.2, 3.3, 3.4) and to add few sentences to clarify the transition between the observations of the circulation and the potential impact on biogeochemical distributions:

- Section 3.1 "Considering the large scale biogeochemical distribution, the meridional transport observed could lead to ..."
- Section 3.2 "Coherent mesoscale features are well-known to participate in the surface biogeochemical variations ..."

The comparison of satellite-based advection proxies with drifter data seems to be a more significant piece of work than acknowledged here and I would like to see it rescued from the Appendix and given a more thorough account in the main body of the paper. As part of this it would be good to see a discussion of the possible bias of comparing trajectories of just a few real floats with many more virtual ones and an acknowledgement of the fact that the streamfunctions only really do a reasonable job compared to drifter trajectories for LDC (Figure A1) and why this might be so.

We agree with the reviewer's suggestion and we propose to add a new subsection in

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Material and Methods to detail the comparison between the different satellite-based advection and in situ drifters. The new subsection would be *2.4 Comparison of satellite products with in situ drifters*. It would include the Figure 1 and discussion raised in the third comment of Referee#2.

Minor: Page 3, line 6: I'm not sure I'd describe the surveyed area as 'relatively high kinetic energy' or 'intense' given that it only visits the high value areas intermittently in Fig. 1.

The “relatively high eddy kinetic energy” refers also to the study by Qiu et al., 2009. That's why we propose to change the sentence as follows : “ As displayed in Figure 1, the OUTPACE cruise was conducted in the transition area between a zonal band of relatively high eddy kinetic energy south of 19°S (Qiu et al., 2009) and low eddy kinetic energy to the north. ”

Page 4, line 21: how was DIP turnover time calculated and why is it of interest?

As describe in Moutin et al. (this issue), Dissolved Inorganic Phosphate (DIP) turnover time represents the ratio between Phosphate natural concentration and Phosphate uptake by planktonic species (Thingstad et al., 1993). It is considered the most reliable measurement of phosphate availability in the upper ocean waters (Moutin et al., 2008). In our region of interest, the phytoplankton growth, and in particular nitrogen fixers, is often limited by Phosphate availability and Phosphate may appear as a key factor controlling carbon production (Van den Broeck et al., 2004). This parameter thus gives an important information on the biological activity. As these clarifications are important, we add them in Material and Methods : “Dissolved inorganic phosphate turnover times (TDIP) were determined using a dual 14C-33P labelling method following Duhamel et al. (2006) and described in Moutin et al. (this issue). As describe in the latter, DIP turnover time represents the ratio between Phosphate natural concentration and Phosphate uptake by planktonic species (Thingstad et al., 1993). It is considered the most reliable measurement of phosphate availability in the upper ocean waters (Moutin et al.,

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2008). In the WTSP, the phytoplankton growth is often limited by Phosphate availability. This parameter thus gives an important information on the biological activity in relation to resource availability : a very short DIP turnover time means rapid utilization of the ambient phosphate present in limiting concentration, whereas a long DIP turnover time represents a slow utilization of the ambient phosphate present in higher concentration.”

Page 7, line 33: “...and 0.2...as thresholds...” In several places ‘west’ and ‘east’ are confused. e.g. page 8, line 27; page 10, line 2; page 10, line 28

We checked and corrected the points highlighted.

Page 8, line 29: explain location/extent of Melanesia

We modify the sentence as follows : “ Moreover the path through the Melanesian area, which includes the multiple islands from Papua New Guinea to Fiji (140°E-170°W), may enrich these waters due to the contact with islands whereas... ”

Page 11, line 7: I think it is debatable that chlorophyll shows a “reasonable correlation in Fig. 5. Scatter plots and correlation n coefficients would be more convincing.

Correlation coefficients are mentioned in Section 2.2 p5 L34 : 0.8 for both temperature and chlorophyll, which is a reasonable value when comparing in situ data and satellite-derived data. Below we plot the difference between in situ temperature (chlorophyll) and satellite data (Fig. 1 and 2 respectively). In temperature, we get differences between +1.5°C and -1°C which allows us to confidently use satellite temperature. For chlorophyll, the differences are smaller than $\pm 0.1 \text{ mg m}^{-3}$ which is also a reasonable deviation between satellite and in situ measurements, besides considering the color-bar scale of Figure 5 with values that vary from 0 to 1 mg m^{-3} . We can also note that the satellite data clearly underestimate chlorophyll concentrations in the Melanesian area. We believe this figure and explanation could help the reader, so we decide to add the figure in the Appendix and to add this text in Section 3.3 : “The differences

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between in situ temperature (chlorophyll) and satellite data are plotted on Figure A4 (top and bottom respectively). In temperature, we get differences between $+1.5^{\circ}\text{C}$ and -1°C which allows us to confidently use satellite temperature. For chlorophyll, the differences are smaller than $\pm 0.1 \text{ mg/m}^{-3}$ which is also a reasonable deviation between satellite and in situ measurements, besides considering the colorbar scale of Figure 5b with values that vary from 0 to 1 mg/m^{-3} . We can also note that the satellite data clearly underestimate chlorophyll concentrations in the Melanesian area.”

Figure 4: The backward and forward streamfunctions cross, particularly for LDA. This warrants comment.

We agree with the reviewer: this is very interesting that around LDA backward and forward paths cross. This highlights again the complexity of the circulation between New Caledonia and Vanuatu, characterized by meanders and recirculations. We propose to add a comment about this in Section 3.2 as it reinforces the previous observations of complex path in this area (Rousselet et al., 2016) : “Backward and forward streamfunctions cross around station LDA which suggests that the area between New Caledonia and Vanuatu is a region of complex recirculation with waters that stay in this region for a while before exiting the Coral Sea.”

Page 22, Fig 5 middle: why are the data points for the TSG so far apart given that it is a flow-through system?

Indeed the TSG provides data every 1min30, however plotting these data on a scatter plot including the whole cruise route requires a large amount of memory to finally obtain a not so clear information. Thus to reduce the size of the figure and provide an useful comparison with the satellite image, we decided to plot a weighted mean over 5 days that is the time interval used to produce the composite satellite images. As a consequence the position of the point depends on the position of the boat every 5 days. We performed the same calculation for underway chlorophyll data (Fig. 5b). In order to avoid misunderstanding, we precise in the caption that TSG and chlorophyll data point

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correspond to weighted mean data over 5 days as follows : “ Top : ... superimposed with 5 days weighted mean of sea surface temperature (°C) from TSG... .Center : ... superimposed with 5 days weighted mean of surface chlorophyll concentration...”.

Figures 6 and 7: it is difficult to relate top and bottom panels with one labelled in degrees and the other in km.

Referee #2 also pointed out this issue. We agree to change both figures and to plot Fig 6b. and Fig 7b. in degrees (Fig.3 and 4).

Figure 7: what are the white squares? Missing data due to cloud ?

Indeed white squares are missing data due to cloud covering as we use satellite data from a specific day. This missing information will be added in the caption for more clarity : “ White squares are missing satellite data due to cloud cover.”

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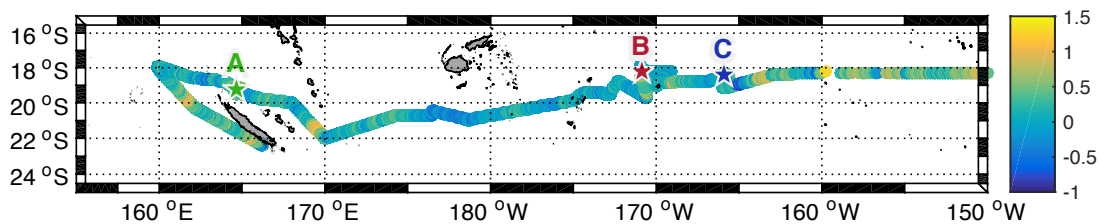


Fig. 1. Difference between in situ surface temperature from TSG and satellite-derived sea surface temperature from CLS (°C)

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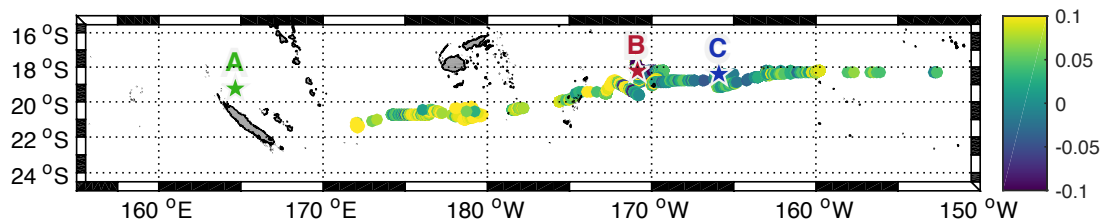


Fig. 2. Difference between in situ surface chlorophyll concentration from the underway survey and satellite-derived sea surface chlorophyll concentration from CLS (mg m^{-3})

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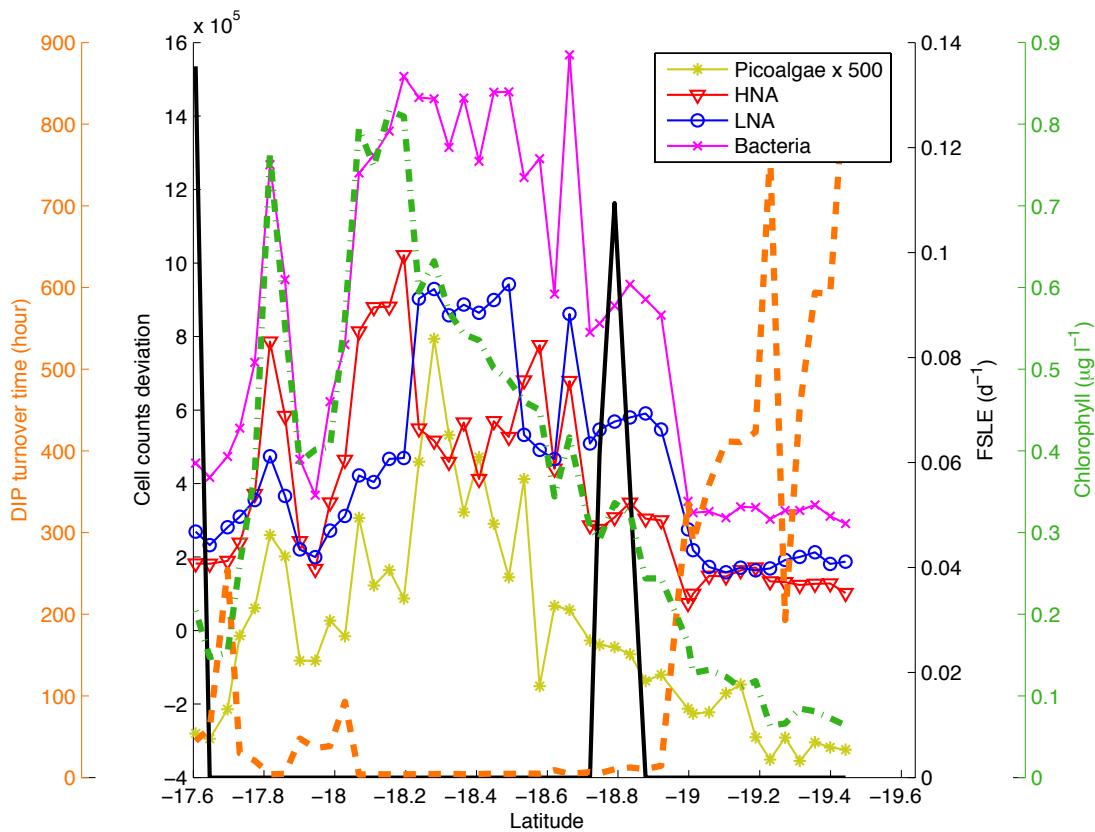


Fig. 3. Modified figures 6b

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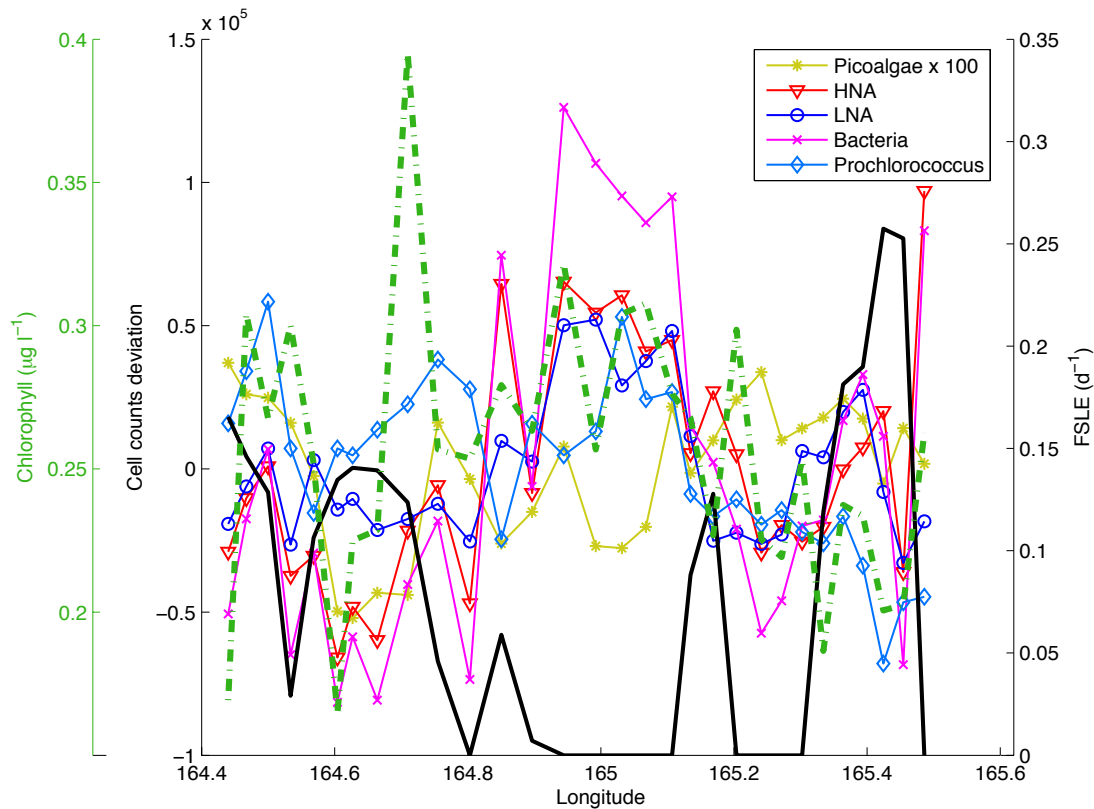


Fig. 4. Modified figures 7b

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