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Interactive comment on "Seasonal dynamics and disturbance of phytoplankton biomass in the wake of Tahiti as observed by Biogeochemical-Argo floats" *by* Raphaëlle Sauzède et al.

Anonymous Referee #2

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This paper analyzes data from two Biogeochemical-Argo floats deployed from the southwestern side of Tahiti during which one float persisted and drifted westward for approximately 1200 km from April 2015 through November 2016 and the other remained within 45 km of the Tahitian coast for a brief period from October 2015 into January 2016. The paper incorporates meteorological data from the Tahitian coast, model output from the Hybrid Coordinate Ocean Model, and remotely sensed altimetry to aid in interpretation of the observations. This is a valuable complement to observations and the results provide new insights into a generally sparsely sampled portion of the world ocean. However, I believe that the results as presented are not yet up to the high-quality standards required for publication in Biogeoscience. Thus I suggest that

some significant revisions are needed before the article is ready for acceptance. As presented, the authors primarily describe the importance of the seasonal dynamics in the open ocean far field west of Tahiti and distribution of phytoplankton biomass in the wake of Tahiti. The 3 months of in-situ data from the Tahitian wake are insufficient to adequately resolve seasonality of the phytoplankton biomass in the immediate wake and it is not clear that this is necessarily more than one season, or perhaps a transition between seasons. Improvements in presentation and some restructuring could improve the manuscript to a level where it could be acceptable for publication.

The scientific objective of the manuscript is to investigate the seasonal dynamics in phytoplankton biomass in the open ocean compared with observations from the Tahitian wake. The paper's title indicates that the paper will provide insight into the seasonal dynamics and disturbance of phytoplankton biomass of the Tahitian wake. However, the float near the island was out for only 87 days with only 10 of those in the operationally defined summer period limiting the ability to statistically assess and differentiate the summer period from the spring period. Thus, I don't think that the data set for the island wake allows for true resolution of seasonal variability. Although the authors show one year's data for the open ocean float, this data set is not fully exploited nor explained. I suggest the authors consider a title that better represents the results and conclusions of the paper.

Bellow, I list some suggestions to include in the revised manuscript.

1. Given the typical time span of Biogeochemical Argo floats, it would be helpful to explain the short duration of the island wake float. Was this by design, and if so, what is the design criteria? Or was it due to a failure in the float? It would help put the data set into context and understand the limited time duration, given the 19 months of the open ocean deployment.

2. In section 3.1: The authors apply a moving average of +/-5 observations (total of 11 observations/average) in time. Because the open water float's profiling frequency is

shifted from once per day to once every 5 days after 16/07/2015, the averaging period shifts from a 10-day (11 points) average to a 50-day average after 16/07/2015. The implications of this smoothing are much different before and after the breakpoint. The authors in this section describe the dynamic of phytoplankton biomass in the central SPSG. For that purpose, the authors should include the vertical distribution of backscattering in their description, as is an important component to understand the phytoplankton dynamic (in Figure 2). In addition, the authors start the description of Figure 3 about the surface expression of Zpd, MLD, Chlsurf, bbp_surf and their ratio for the open water float without link the data with the vertical distribution (For example integrate CHL0-200m). In that part of the paper, the authors mix the results between the open water float and Tahiti float in Table 3.

3. The authors also try to use mean and range values for the 4 seasons (Table 3), however the data for each season do not have the same temporal distribution and number of observations. I would suggest the authors try to structure the data in Table 3 per month to investigate the mean values of CHL as they did in Figure 3 so you can have a better understanding the phytoplankton biomass distribution. In addition, I would like to suggest the authors rewrite this part, as it is hard for the reader to follow.

4. The authors try to compare the SPSG and Tahitian wake using in-situ data in section 3.2. In the introduction part, the authors point out the importance of subsurface measurements. However, they are using only the surface data to make the comparison between the two study areas. I recommend the authors to restructure this part of the paper and include the vertical distribution of the exam parameters in this sub-section. The authors must show a comparison of the vertical distribution along time of the exam parameters. Several times the authors generalize and compare the spring and summer seasons from both seasons. The Tahiti float has two months of data in the spring and only 10 days of data for the summer season as they call it. In addition, the authors mention in that section that phytoplankton biomass is not visible from remote sensing but both monthly and 8-days products are available (MODIS or CCI). Using the remote

sensing ocean color they will be able to understand better the surfaces changes of phytoplankton dynamics in space and also to compare with the surface in-situ data from the floats. In the last paragraph of this section, the authors present the monthly mean vertical profiles. However, the authors show data only for the 12 months of the open water float without mention and explain the reason? Do the authors find differences in the monthly mean vertical profiles between 2015 and 2016 for the open water float?

5. The authors should explain and justify if they talking about the phytoplankton dynamic in the Tahiti wake or Tahiti island mass effect in section 3.3.1. It is unclear and confusing for the reader to follow up as the change these terms so many times in the manuscript. Furthermore, the authors should answer how the IME effect is relative to the Tahiti Wake as different spatial-temporal processes occur?

6. The discussion in section 3.3.2 is unsatisfactory to me. Nitrate concentrations appear to be elevated in the upper 100 m, in some cases with uniformly high concentrations (>1 μM) from the surface to 100m, especially in the middle of the period. Rather than showing the T-S plot which is interesting, the actual vertical profiles of temperature an salinity would be helpful. Density, as the authors indicate, shows stratification in the upper layer which is consistent either with the possible or upwelling or land runoff of fresh water which will be buoyant and should only be reflected in the near-surface region.

7. Section 3.3.2 – Lines 16-23 – you suggest possible entrainment from a lagoon driven by a strong current around the island. It is interesting that the average O2 concentration in the upper 300 m during this period declines and there is clearly decreased oxygen and increased nitrate deeper in the water column (Figures 5e and 9). Do you know anything about the AOU/Nitrate relationship for the region? Is there any possibility that some sort of mixing or upwelling (upstream) might have brought this about?

8. The differential in the water masses based on your T-S diagrams might give some insight. You code the T-s plot with chlorophyll. It could be useful to look at this with

respect to oxygen to try to understand the sources of nutrients during period 2.

9. The speculation using the Hycom model is a useful exercise, but how confident are you in the results? Is the model assimilating the regional float data and regional altimetry?

10. The authors in the summary again refer to IME effect using data from Tahiti Wake float. Did the authors examine remotely sensed chlorophyll and SSH to provide further characterization of the region? Perhaps there could be some clues

Other comments

Authors have to harmonize the abbreviations in the document

In Section 3.2, Line 4-5 the following statement is made – "likely reflect differences in phytoplankton community composition (or in the nature of the particle assemblage) in the island wake as compared to the open ocean." Besides bbp*, is there any other data like HPLC data from profiles near the Wake float that provide additional insight?

Figure 2: Units and labels are missing. Isolumes lines are not visible in the CHL plot (panel a) in the PDF submitted for review.

Figure 5: Units and label are missing from the figure from c-h.

In the legend of the figure 5e and 5f the authors refer that the zoom is in the first 150m but in the plot shows only the first 100m of the water column

In figure 5b the top boxes that indicate the periods are not in same locations as in the others subplots

It will be useful for the reader if the authors can add the MLD in figure 5e and 5f

Are you confident in the nitrate concentrations shown in Figure 5? During late October – early November, you show nitrate concentrations on the order of 0.5-0.8 μM after the water column has stratified. And the concentrations seem to be uniform throughout the

upper 100 m. These values seem high for an oligotrophic sea, but I have no experience or literature understanding of this region.

Figure 7. The section symbols are small and hard to identify on the map

Figure 8 Units and labels are missing

Figure 9: The authors should show the MLD and the DCM. Also, I suggest the authors show all the data from the Tahiti Wake float together.

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