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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.

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First of all, we are deeply grateful to Reviewer 2 for his/her constructive comments and suggestions to improve our manuscript. Here we address in details and point-by-point these comments. Our responses follow each comment in blue.

Answers to the general comment of Reviewer 2 (mentioned as R2 hereafter):

R2: 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.
Author's response (mentioned as AR hereafter): We proposed our previous title in order to highlight: 1) the seasonal dynamics in the open ocean (from FOpenO) and 2) the signature of an IME on phytoplankton biomass (from FTWake). However, we agree that the title was a little bit misleading as the seasonal dynamics in the phytoplankton biomass leeward of Tahiti cannot be fully investigated. Hence, in agreement with R2's comment and, in some way with R1's response, we propose another title as: "Enhancement of phytoplankton biomass leeward of Tahiti as observed by Biogeochemical-Argo floats".

Answers to R2's list of suggestions:

R2's point 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.
AR's point 1: We thought that the reason was implicit. The FTWake float (now FLeeT according to R1's comments and R2's point 5) stopped communicating after three months and was declared lost and inactive. A sentence has been added to clarify this point in section 2.1: "Because of a technical issue, the FLeeT stopped communicating after 3 months of data acquisition, limiting our study to only 3 months of data leeward of Tahiti."

R2's point 2a: 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.
AR's point 2a: According to R2's suggestions, we redid the calculation with a moving average of 1) +/- 15 observations (total of 31 points/average) when the float has a daily profiling frequency (i.e., until the 16/07/2016 for FOpenO and during all the lifetime of FLeeT) and 2) +/- 3 observations (total of 7 points/average) when the FOpenO float has a 5 days frequency of acquisition. Hence, a steady moving average period of 30 days is applied for the 2 floats over their lifetime. In the corrected manuscript, this moving average description has been added in the Method section.

R2's point 2b: 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).

AR's point 2b: In agreement with R2, we included the time series of POC (linear transformation from b_{bp}) in Figure 2 (see the new figure attached as Fig. 1 below). We have also transformed every b_{bp} values in POC values in the revised manuscript in order to make it more representative to the reader (for example in Figure 4). The POC conversion was already explained in the initial manuscript lines 25-33 page 4 and lines 1-3 page 5.

R2's point 2c: In addition, the authors start the description of Figure 3 about the surface expression of Zpd, MLD, Chlsurf, bbpsurf 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.
AR's point 2c: We reproduced plot based on surface measurements similar to the one of Mignot et al. 2014 (M2014) in order to compare the central oligotrophic region of the South Pacific Ocean (our studied area, see the white rectangle in the new Figure 1, see Fig. 2a below) with the eastern ultra-oligotrophic area of M2014 (see the white star in Figure 2a below).
Figure 3 a) shows that our results are in agreement with M2014 results in the SPSG, b) provides the open ocean seasonal context of our studied area which then c) allows us to introduce and highlight the IME from FLeeT that is presented in Figure 5. Hence, we believe that moving Figure 5 directly after Figure 2 would perturb the common thread of the article. However, according to R2's comment, we propose to add some information at the beginning of this section to clarify our strategy and the plan of the manuscript.
In section 3.1 of the revised manuscript, we propose to add the following sentence: "The seasonal dynamics of phytoplankton biomass in the central SPSG is investigated using the 18 months of observations from FOpenO. Our findings are compared with M2014 results to provide a new insight of the seasonal dynamics of phytoplankton

biomass in the central region of the South Pacific Ocean which is less oligotrophic than the eastern ultra-oligotrophic area of their study (see the white rectangle and the white star in Figure 1a respectively)."
According to R2's comment on Figure 3, we have removed the FLeeT data from Table 3 and created a specific Table 4.

R2's point 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.
AR's point 3: As suggested by R2, we organized Table 3 per month instead of seasons. This part has been rewritten in the revised manuscript.

R2's point 4a: 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.
AR's point 4a: Comparisons between the open ocean and leeward of Tahiti have not been performed on surface data only. The mean vertical distributions of the examined

parameters are also presented and compared in Figure 4.
We agree that we cannot generalize the seasonal dynamics using only the 3-month data from FLeeT. Therefore, we consequently corrected the manuscript.

R2's point 4b: 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?
AR's point 4b: We agree with R2 that we were not clear enough on this point. The mention of cloudy conditions preventing the use of ocean color satellite observations in the text specifically referred to the study of the IME period of interest (i.e., during ~ 15 days in December 2015). Considering the short time scales of this phenomenon, the use of monthly composites is not adapted to follow the dynamics and the evolution of the IME. The 8-days composites have a too sparse spatio-temporal coverage as only the composite covering the 11th to the 19th of December period shows some data leeward of Tahiti. The other three 8-day composites in December are extremely cloudy. We agree that we were not clear enough on this point. The sentence has been consequently modified and completed.
We have also substituted the previous annual climatological satellite image in Figure 1 with the OC-CCI monthly composite of December 2015 (when the IME is observed). Moreover, we have split this figure in 3 (one at basin-scale, one centered on FOpenO and one zoomed on the FLeeT trajectory) to provide a more exhaustive view of the studied area (see this new figure as Fig. 2 below).

In section 3.2 of the revised manuscript, we propose to add the following sentence: "The OC-CCI monthly composite in December 2015 (comprising the biological enhancement event observed in Figure 3) shows high Chl ($\sim 0.8\ mg\ m^{-3}$) eastward of the FLeeT trajectory and nearby the southern coast of Tahiti (Figure 1c). This imprint on ocean color satellite observations might be linked to the biological enhancement highlighted in Figure 3. However, the use of monthly composites is not adapted to follow the dynamics and the time evolution of this event and the 8-days composites have a too sparse spatio-temporal coverage due to a dense cloud cover."
We agree with R2 that showing FOpenO vertical profiles for 12 months without taking into account years 2015 or 2016 in Figure 4 only provides a partial view. The Figure 4 as a function of years (i.e. 2015 and 2016) is presented below as Fig. 3. There is no significant difference between years 2015 and 2016. Therefore, we think that the figure as in the initial manuscript is easier to follow as years do not bring any more information to that part of the study. However, we added in the text the information that there is no significant difference between years.

R2's point 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?
AR's response to point 5: In the manuscript, we used the term "Tahitian wake" to refer to the geographic position of the float leeward of the island, not to the physical process. The term "IME" refers to the biological enhancement induced by the island, as defined by Doty and Ogury (1956). Considering the R1 and R2's comments, we agree that there can be some confusion with this terminology. To avoid any misunderstanding, we removed the term "island wake" used in the manuscript and replaced it with "leeward of Tahiti". The FTWake float is now referred to as FLeeT float.

R2's point 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 and 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.
AR's point 6: Time series of temperature and salinity (for both the 0-350 m and the 0-100 m layers) are shown in Fig. 4 below. On one hand, a salinity decrease can be observed in the upper layer during period 2, in agreement with land runoff of fresh water (as in the TS diagram). On the other hand, there is no temperature decrease in the upper layer or uplift of isotherms possibly associated with an upwelling event. The upwelling assumption has also been investigated through satellite Sea Surface Temperature observations during period 2 with no conclusive outcomes. However, we agree that these information were missing in the text, hence the following sentence will be added: "Temperature measured from FLeeT remains constant along time and depth in the upper layer over period 2 (figure not shown). Moreover, sea surface temperature observations derived from satellite during this period do not show any cold-water pattern leeward of Tahiti. Hence, the coastal upwelling assumption to provide nutrients toward the surface has not been retained."

R2's point 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?

AR's point 7: First of all, we would like to bring to the attention of R2 that a) the decline of O2 is observed at a depth of 300 +/-10 m (this is not a 0-300m average that is represented in Figure 9b) and b) Figure 5e is a zoom of Chl not of nitrate concentrations (the increase of nitrate at 300 m can be seen in Figure 5d). As far as we know nothing is reported about the AOU/nitrate relationship for the studied region.

R2's point 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.

AR's point 8: During the second period, Figure 5 shows an input of nitrate in the upper 100m likely coming from the surface (Figure 5f) and not from an uplift of the deep-rich layers (consistently with the physical mechanisms presented in Figures 5 to 7). However, we agree with R2 that the T-S diagram colored with respect to oxygen and nitrate concentrations (see Fig. 5 below) provide insights on nitrate and oxygen sources in the lower layer, which is of interest when considering the transition from period 2 to 3 and R2's comment on point 7. Hence, we propose to change the TS diagram along with Chl by the new figure below (attached as Fig. 5).

R2's point 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?

AR's point 9: There is no other way to get a high spatio-temporal resolution picture of the circulation around the island. We are confident in the HYCOM results as the NCODA system (Cummings, 2005; Cummings and Smedstad, 2014) that is used by the HYCOM model, assimilates available satellite altimeter observations, satellite and /in situ/ Sea Surface Temperature (SST) as well as available /in situ/ vertical temperature and salinity profiles (from XBTs, Argo floats and moored buoys). It should be noticed that the Temperature and Salinity profiles from our Argo floats are flagged good or probably good, meaning that these data are likely considered by the reanalysis. However, as the gray list of observations that are not assimilated by the model is not available, we cannot answer definitely to the question.

R2's point 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.

AR's response to point 10: We have examined satellite chlorophyll concentration but without conclusive outcomes. In fact, when examining the monthly and 8-day composites of Chl satellite data from 2002 to 2017 in the studied area, no clear biological enhancement induced by the presence of Tahiti can be evidenced. When considering the IME period observed from the FLeeT float, as explained above (point 4b), it is difficult to have a good interpretation based on satellite data because of the short time scales of the studied process (~ 15 days). For remotely sensed chlorophyll, the best that could be done when focusing on period 2 is shown in the new Figure 1 (see point 4 and Fig. 2 below).

Sea Surface Height have been analyzed with no conclusive outcome about specific dynamics leeward of the island. Moreover, as pointed out in the initial manuscript (lines 29-31 page 9), we also ensured that the FLeeT float did not encountered any eddies that could have induced nutrient uplift and explain the biological enhancement of period 2.

Answers to R2's other comments:

R2: Authors have to harmonize the abbreviations in the document
AR: We checked and harmonized the abbreviations throughout the document.

R2: 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 b_{bp}*, is there any other data like HPLC data from profiles near the Wake float that provide additional insight?
AR: Unfortunately, there is no HPLC data near FTWake during the IME period.

R2: Figure 2: Units and labels are missing. Isolumes lines are not visible in the CHL plot (panel a) in the PDF submitted for review.
AR: We agree that the black dotted lines were barely visible in this figure so we added the PAR time series to Figure 2 (see the new figure attached as Fig. 1 below).

R2: 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

AR: Figure 5 was revised and the figure legend corrected.

R2: 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 M 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.
AR: The limit of detection of NO3- concentration estimated from the SUNA sensor measured from BGC-Argo float is ~ 1 micro M (Pasqueron de Fommervault et al., 2015). In our study, we more discuss the dynamics/variation of nitrate concentration than the values itself.

R2: Figure 7: The section symbols are small and hard to identify on the map
AR: Symbols have been changed.

R2: Figure 8: Units and labels are missing
AR: Figure 8 was revised.

R2: 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.
AR: Figure 9 was revised as suggested.
We believe that the common thread of the article is easier to follow keeping Figure 9 separately from Figure 5. Indeed, Figure 5 mostly highlights biogeochemical changes

from period 1 to 2 and 3 over the 150-200 upper meters. It allows us to start the discussion on rain events and island run off as a mechanism to explain changes from period 1 to 2 (Figures 6 and 7). Then, Figure 8 gives insights on the transition from period 2 to 3 through dynamical changes over 0-300 m, which are finally further investigated in Figure 9 and later in the manuscript. Hence, we believe that moving all together the FOpenO figures, followed by all the FLeeT Figures would perturb the common thread of our manuscript. However, according to R1's comment, we propose to add some information at the beginning of this section to clarify our strategy and the plan of the manuscript.

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Legends of the attached figures below :

Fig. 1 (new Figure 2 in the revised manuscript): FOpenO vertical distribution along time of (a) Chl ($mg\ m^{-3}$) with the 0.1, 0.2 and 0.3 $mg\ m^{-3}$ isocontours as dotted black lines, (b) PAR ($mol\ photons\ m^{-2}\ d^{-1}$) with the the 0.1, 1 and 10 $mol\ photons\ m^{-2}\ d^{-1}$ isolumes as dotted black lines, (c) POC ($mg\ m^{-3}$) and (d) density ($kg\ m^{-3}$) with isopycnes (interval = 0.5 $kg\ m^{-3}$, dotted black lines). In each panel, the white and red lines show the MLD and the depth of the DCM, respectively.

Fig.2 (new Figure 1 in the revised manuscript): (a) Spatial distribution of OC-CCI surface satellite Chl ($mg\ m^{-3}$) in December 2015 for the Pacific Ocean. Our studied area and the Tahiti island are represented by the white rectangle and the white point respectively. The white star represents the geographic area of the M2014 reference study (see Section 3.1). The trajectories of (b) FOpenO float and (c) FLeeT float are shown with color in background representing the OC-CCI surface satellite Chl ($mg\ m^{-3}$) distribution in December 2015. In panel b FOpenO profiles concomitant

with FLeeT profiles acquisition are colored in red. Grey pixels represent missing data because of clouds or the 500 m bathymetric mask that removed nearshore pixels that could be biased by land. Islands and continents are indicated in black. The Moorea and Tahiti islands are indicated and the mean direction of the South Equatorial Current (SEC) is indicated by the arrow in panel c.

Fig. 3: Yearly Vertical distribution of monthly average (from top to bottom): Chl ($mg\ m^{-3}$) and POC ($mg\ m^{-3}$). The left column represents the FOpenO observations and the right column represents the FTLee observations. The color code represents months.

Fig.4: Time series of temperature and salinity from FLeeT float for the 0-350 m layer (top panels) and the 0-100 m layer (bottom panels).

Fig.5: new Figure 6 in the revised manuscript): T-S diagrams issued from FLeeT during period 2 (a and c panels) and period 3 (b and d panels) as defined in the text (see Section 3.3.1) and Figure 5. Black dotted lines represent isopycnal surfaces (interval= 0.5 $kg\ m^{-3}$). As shown in color, T-S measurements are associated with O2 concentrations ($mol\ kg^{-3}$, a and b panels) and NO3- concentrations ($mol\ kg^{-3}$, c and d panels).

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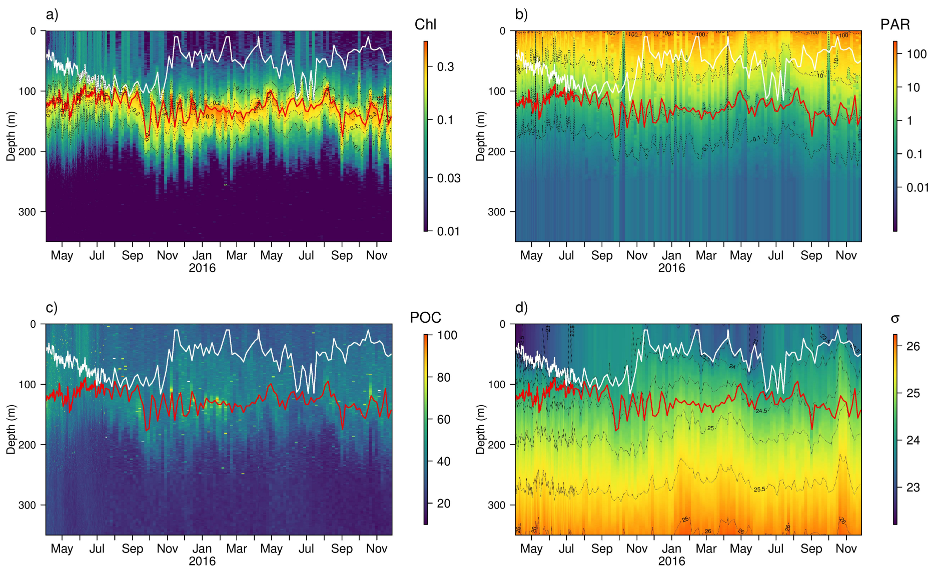

Fig. 1. New Figure 2 (see legend above)

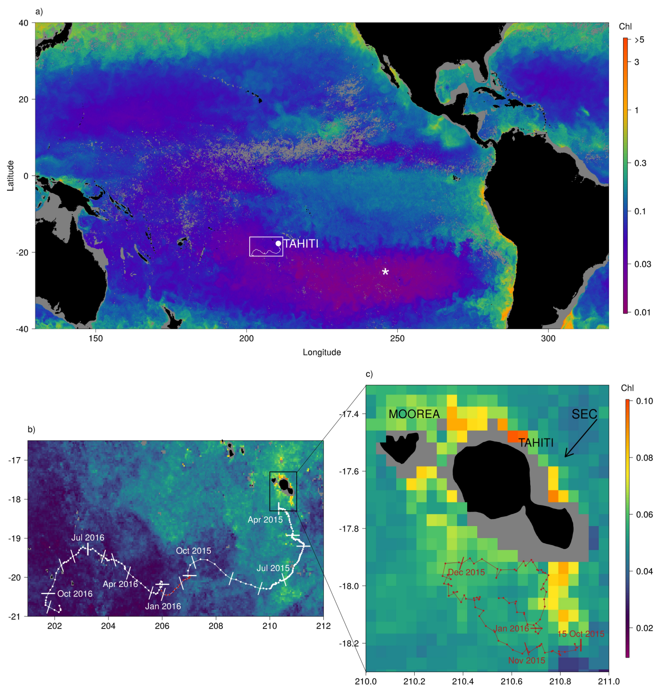

Fig. 2. New Figure 1 (see legend above)

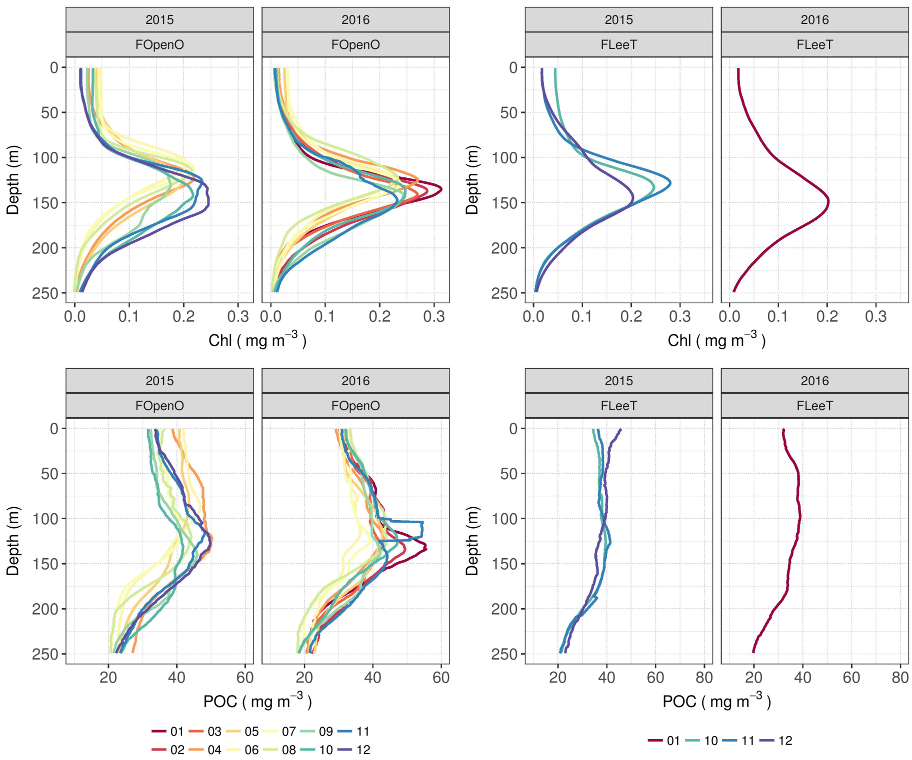
2015
FOpenO
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Fig. 3.

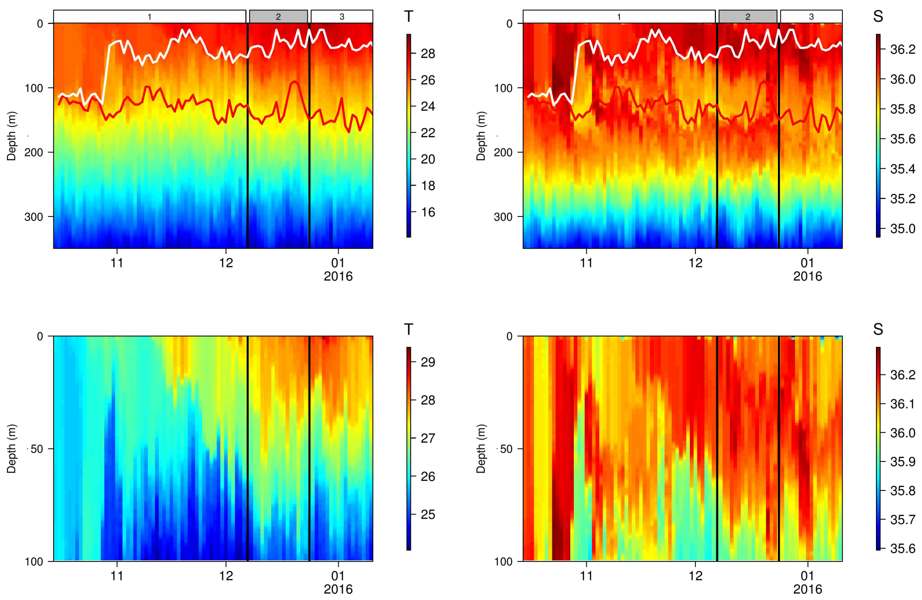
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Fig. 4.

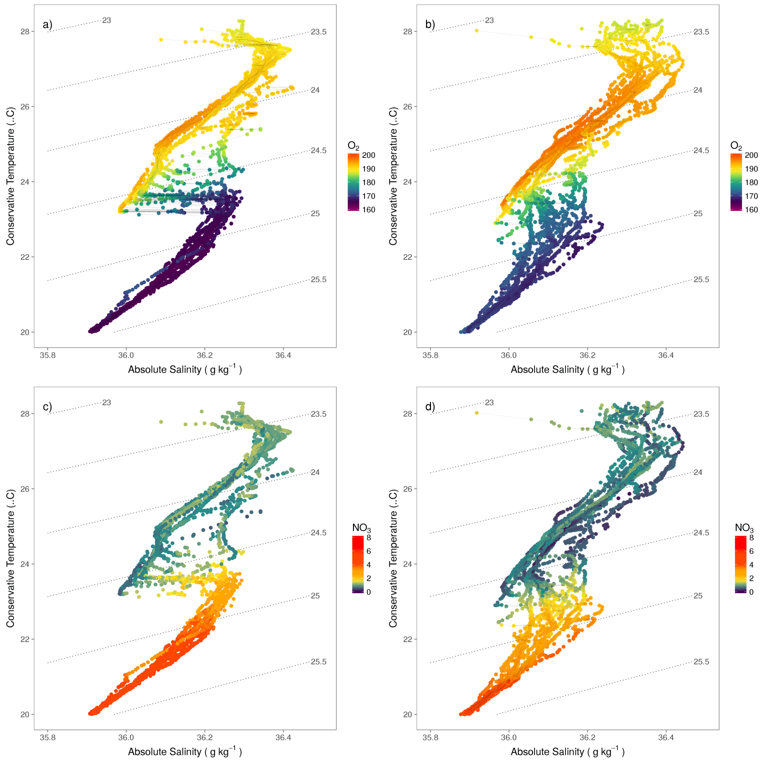

Fig. 5. New Figure 6 (see legend above)