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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 1 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 major issues raised by Reviewer 1 (mentioned as R1 hereafter):

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R1's point 1a: They downplay the utility of satellite studies of the Island Mass Effect (IME), as they can't resolve subsurface dynamics, yet there is a crucial thing that satellite studies can do, and what is missing from their paper, and that is supply the big picture view of the spatial chlorophyll distribution. They spend a lot of time talking about the chlorophyll dynamics in the "Tahiti wake" but there is no clear idea of what area might be encompassed by the wake. Furthermore, what dynamic is really going on – is it the Tahiti wake, or the Tahiti IME? They use these terms interchangeable but the two terms imply different things. The Gove et al [2009] paper treats the IME as a chlorophyll increase more or less uniformally distributed around the island, while a wake implies an increase on the downstream side of an island. The two dynamics have different scales as well. The Gove et al [2009] shows the IME extends to only 20-30 km from the coast, whereas chlorophyll increases from wake effects can be seen for hundreds of kms [Andrade et al, 2014]. Which is happening with Tahiti?

Author's response (mentioned as AR hereafter in this document) to point 1a: We agree with R1 that satellite Chl data can supply the big picture view of the spatial Chl distribution. However, we have examined the monthly and 8-day composites of Chl satellite data from 2002 to 2017 in the studied area but we have not seen any clear biological enhancement leeward of Tahiti from these data (see in addition the AR to point 1b and 1c below).

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.

R1's point 1b: The climatological chlorophyll image shown in Figure 1 doesn't help

with this. They claim the cloudy conditions in the region (pg 12, lines 3-5) preclude the use of satellite data however this is not true. As seen on the attached monthly composites of chlorophyll for 2015, the chlorophyll distribution around Tahiti can be visualized throughout the year.

AR's point 1b: We agree with R1 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.

According to the R1's suggestion, 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 sub-parts (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 the new Figure 1 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."

R1's point 1c: What these figures show is a regional chlorophyll enhancement around Tahiti, not a local one. There are three months, Jun-Aug, ie austral winter, when the satellite chlorophyll levels are markedly higher on one side of the island, however this occurs on the NE side of Tahiti, not on the lee side as the discuss in the paper. Their float data needs to be interpreted in the context of the larger-scale information available from satellite chlorophyll.

AR's point 1c: We agree with R1's suggestion and consequently, the basin-scale Chl satellite data has been added to Figure 1 (see Fig. 1a below) to provide a more exhaustive view of the Chl spatial distribution. On this new Figure 1a, Tahiti is located in the upper-right corner of the white rectangle (see the white point). The general trophic conditions in the South Pacific Subtropical Gyre are shown in this figure with higher Chl in the NE side of Tahiti toward the equatorial mesotrophic region, and lower Chl southwestward toward the oligotrophic gyre. The increase of chlorophyll during winter, pointed out by R1, is consistent with our investigation of the seasonal variability from FOpenO (see Figure 3c).

R1's point 2a: In the comparison between FOpenO and FTWake in Figure 3 why are just the surface measurements shown? Particularly when the whole point of the BGC floats is to get subsurface data? Also why is the comparison shown before any of the data from FTWake is shown? Figure 5, the sections from FTWake, should directly follow Figure 3.

AR's point 2a: We reproduced plots 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 1a below) with the eastern ultra-oligotrophic area of M2014 (see the white star in the new Figure 1a 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 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.
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)."

R1's point 2b: And why isn't the same information shown (as sections) for the two floats? For FOpenO only sections of chlorophyll and density are shown.
AR's point 2b: Information shown in Figures 2 and 5 differ because FOpenO does not have some nitrate and oxygen sensors. Chl as a function of density (Figure 5g) aims to illustrate that the vertical distribution of Chl is disturbed during period 2 (IME). This plot does not add any more information for FOpenO seasonal dynamics.

R1's point 2c: There is quite a bit of discussion about the procedure for processing the backscatter data but as far as I can tell this data is only shown as depth-averaged values in Fig 3. Likewise for the PAR data.
AR's point 2c: We would like to point out that the PAR and b_{bp} data are also used in Figures 2, 4 and 5. The PAR data are represented as isolumes in Figures 2a and 5a while the b_{bp} is represented in Figure 4 and in Figure 5a. Hence, we think that it is

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important to well describe the processing of these data.
According to R1 and R2's suggestions, we will add the time series of Particulate Organic Carbon (POC) that is derived from b_{bp} and PAR in Figures 2 and 5 of the revised manuscript. We also have transformed every b_{bp} values into POC values in order to make it more representative to the reader (for example in Figure 4). The POC conversion is already explained in the manuscript (lines 25-33 page 4 and lines 1-3 page 5 of the initial manuscript).

R1's point 3: The primary objective of this paper is to examine dynamics in the Tahiti wake, and their primary source of data is from two BGC-Argo floats. However the one from the wake area is only three months long, so it prohibits examining these dynamics on a seasonal scale. The short length of this float is glossed over in the paper, and never explained why it is so short in duration. They need to be upfront about this shortfall.
AR's point 3: We agree that the seasonal dynamics in the phytoplankton biomass leeward of Tahiti cannot be fully investigated with our 3 months-data. We thought that the reason of the short length of this float was implicit. The float 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."

R1's point 4: There are a number of grammatical errors. None of them are major, but there are a lot of them. I have noted some of them but the list is by no means exhaustive. They should have the manuscript edited by a native English speaker.
AR's point 4: The revised manuscript will be corrected by a native English speaker.

Answers to R1's point-by-point minor comments:

R1: Page 1, lines 23-24: "observations collected with two Biogeochemical-Argo (BGCArgo) profiling floats from April 2015 to November 2016. This implies that they are using data from two floats that both collected data from Apr 2015-Nov 2016, but in fact only one of the floats did. The wake float only collected data for three months. Why is that? This is not explained in the manuscript. The short length of this float is glossed over in the manuscript.

AR: We will modify the sentence to be more precise as follows: "Physical and biogeochemical observations collected with two Biogeochemical-Argo (BGC-Argo) profiling floats are used to investigate the dynamics of phytoplankton biomass. The first float drifted from April 2015 to November 2016 over more than 1000 km westward of Tahiti (open ocean conditions). The second float was deployed leeward of Tahiti in October 2015 and stopped communicating in January 2016. Over these 4 months, it remained within 45 km off the island coast."

R1: Page 1, lines 27-28: "Vertical observations show a light-driven deepening of the deep chlorophyll maximum (DCM) from winter to summer" This was not shown in the paper. The only PAR data shown was in Figure 3 where it was averaged within the MLD, and there was no representation of the DCM plotted in Figure 3. Its puzzling that they stress in the Intro the importance of their study of having PAR data and then they do not show all the data.

AR: The seasonal variability along depth of the PAR is shown in Figure 2a (Section 3.1). The 0.1, 1 and 10 $mol\ photons\ m^{-2}\ d^{-1}$ isolumes are represented as dotted

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black lines while the DCM depth is represented by the red line in Figure 2a. However, 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 2 below). In addition, the importance of the PAR is highlighted through the correlation between the DCM depth and the 1 $mol\ photons\ m^{-2}\ d^{-1}$ isolume: "the depths of the DCM and of the specific isolume of 1 $mol\ photons\ m^{-2}\ d^{-1}$ are significantly correlated (r = 0.89 and p-value < $2.2\ 10^{-16}$ using a Pearson's test)" (lines 5-6 page 7 of the initial manuscript).

R1: Page 2, line 27: unclear which "this study" is referring to – just specify Gove paper again otherwise the reader might think you are referring to your own paper.
AR: The sentence has been corrected as suggested.

R1: Page 1, line 30-31: "the physical mechanisms involved in the disturbance of phytoplankton seasonal cycle in the Tahitian wake have been investigated" Since the float in the wake only collected three months of data it is not possible to look at an entire seasonal cycle from the wake, as they claim here.
AR: In agreement with R1's comment, this statement has been modified here, but also all over the manuscript when necessary. Consistently, we have changed the title of the paper which is now "Enhancement of phytoplankton biomass leeward of Tahiti as observed by Biogeochemical-Argo floats".

R1: Page 1, line 35: "bio-optical measurements suggest that the composition of phytoplankton community could differ in the Tahitian wake vs. the open ocean area." How so?
AR: The ratio of b_{bp} to Chl has been used as a proxy for community composition (see

for instance Cetinic et al., 2012). This is shown and discussed in Section 3.2 of the initial manuscript (lines 3-14 page 8). To be clearer, the sentence will be completed as follow: "Moreover, a bio-optical-based community index suggests that the composition of phytoplankton community could differ leeward of Tahiti vs. the open ocean area."

R1: Page 3, line 10: unclear which "this study" is referring to – just specify M2014 paper again otherwise the reader might think you are referring to your own paper.
AR: The sentence has been changed as suggested.

R1: Page 3, line 11: "Indeed M2014 only covered one year of measurements" Careful here – you shouldn't criticize this study too much as a year of data is much more than the FTWake float that forms a central part of your study.
AR: We agree with R1 that the sentence was awkward as our aim was to highlight our opportunity to investigate an additional year and a half of BGC-Argo observations to the sole year of data described so far. The sentence has been modified. Moreover, we have revised the manuscript to be clear that the seasonal cycle is studied only from the open ocean float.

R1: Page 3, line 19-20: "These two pathways allowed the observation of phytoplankton biomass dynamics over a broad range of scales from seasonal to shorter time scales". Again, this falsely leads the reader to assume that seasonal time-scale can be observed for both floats, when in fact this is only the case for the open ocean float. It should be mentioned here that the FTWake float only lasted a few months.
AR: We agree with R1. This sentence has been reformulated to be clear on this point (as anywhere else in the manuscript).

R1: Page 6, line 20-21: "very different drifts experienced by each float allow addressing a broad range of spatial and temporal scales" There is a bit of an overstatement since there is only a three month overlap in the two floats. It's never explained anywhere why FTWake only lasted three months.
AR: We agree and consequently corrected the manuscript (see comments above).

R1: Page 7, line 4: The moving average filtering bit should be in the methods, not in the results. The black lines referred to on Figure 3 are barely visible on the figure. What is the significance of the DCM being correlated with the isolumes of chlorophyll? This seems pretty intuitive and a strange way to start the discussion section.
AR: According to R1's suggestion, the moving average description has been displaced to the Method section. We assume that R1 refers here to the black lines on Figure 2 and not Figure 3.
As mentioned above, we have added a panel with the PAR time series to Figure 2 (see the new Fig. 2 below) to better see the isolumes. The correlation of the DCM depth to isolumes (PAR) highlights that the DCM depth is light driven. This result confirms, for the oligotrophic central Pacific in 2015/2016, what Mignot et al. (2014) reported in the ultra-oligotrophic area in 2009. This result is not so intuitive as the light-driven seasonal dynamics of chlorophyll was previously described only by Letelier et al. (2004) and Mignot et al. (2014) for other oligotrophic environments.

R1: Page 11, lines 14-15: "The open ocean observations confirm the only previous study describing the seasonal vertical dynamics of phytoplankton biomass in the eastern:" You can confirm the results of a previous study, but not the study itself. But

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what are the results that have been confirmed here?
AR: Here we have confirmed 1) the light-driven deepening from winter to summer of the DCM depth and 2) that the Chl wintertime increase in the upper layer is likely due to photoacclimation process. According to R1's comment, we have reformulated the sentence as follow: "Using in situ PAR measurements acquired from FOpenO float (vs. PAR derived from downward irradiance in M2014), we confirm that the wintertime Chl increase in the upper layer is likely due to photoacclimation and that the seasonal DCM vertical variability is light-driven in the present open ocean oligotrophic environment."

Answers to R1's comments on "Tables and Figures":

R1: Table 2. The asterisk on b_{bp}* is not easily noticed (see my comment below about Figure 3), and it is not an intuitive representation of b_{bp}/chl. Why not just refer to it as b_{bp}/chl? I think you mean to say Potential Temperature, not Conservative Temperature.
AR: As suggested by R1, we have replaced b_{bp}* by b_{bp}/Chl. We really mean Conservative Temperature that is the new standard for ocean temperature following the TEOS-10 procedures as described by McDougall et al. (2011, 2012).

R1: Figure 1. Rather than use the climatological chlorophyll as the background in Figure 1 it would be better to use one of the monthly average during the FTWake float (see attached figures). Climatological distributions show situations that statistically never actually occur. Since this paper focuses primarily on the roughly three month time period that FTWake float was active it would be much more instructive to show conditions during that time period. Also it is hard to interpret the different symbols. Color the two floats different colors, not colored by time, and indicate a few time

markers along the trackline of the FOpenO. Also indicate the time period of FOpenO that corresponds to the time period of the FTWake deployment.
AR: We modified Figure 1 as suggested by R1 (see Fig. 1 below). Moreover, we have split this figure in 3 sub-parts (one at basin-scale, one centered on FOpenO and one zoomed on the FLeeT trajectory). FOpenO profiles concomitant with FLeeT profiles acquisition are colored in red.

R1: Figure 3. The y-axes of d) and e) have the same variable, but with different units
AR: In Figure 3, the y-axis in (d) represents surface b_{bp} whereas the y-axis in (e) represents the surface b_{bp}*. As mentioned above, to avoid confusion, we have changed b_{bp}* in b_{bp}/Chl.

R1: Figures 2, 5, 8 and 9. Label the color bars with the variable they are depicting. The black lines referred to in the text for 3a are barely visible. It would be easier to interpret if the months on the x-axis were labeled with month names rather than numbers (ie May not 05). Why is Fig. 9, Oxygen from FTWake separated from the other FTWake sections in Figure 5? It would be much easier to follow the manuscript if all the data from FOpenO was shown together, followed by all the data from FTWake.
AR: Figures 2, 5, 8 and 9 have been modified 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.

R1: Figure 6. What is the point of this figure? What is it telling us about the dynamics of the region?
AR: The point of this figure is to show 1) the relatively low sea surface salinity over periods 2 and 3 likely induced by rain events and 2) that a change in water mass can be evidenced from period 2 to 3. According to a comment of the second reviewer, we have colored the T-S diagram with respect to oxygen and nitrate concentrations instead of Chl in the revised version to provide insights on nitrate and oxygen sources in the lower layer, which is of interest when considering the transition from period 2 to 3 (see Fig. 3 below).

R1: Figure 8. This figure is hard to interpret. Could you show this information on a map instead? Show vectors of the average surface current on the trackline? The vectors could be three different colors corresponding to period 1, 2 or 3.
AR: The vertical distribution cannot be presented on a map and we think that representing the average surface current would remove some interesting information discussed in the manuscript such as the change in the ocean dynamics between the periods 2 to 3 down to 300 m (lines 25-27 and lines 30-31 page 10 of the initial manuscript). Therefore, we prefer to keep this figure as in the initial version.

R1: Figure 9. Show the MLD on the figure.

AR: the MLD has been added in Figure 9 as suggested.

Finally, all typographical errors were corrected according to R1's suggestions. We checked all references for the correct syntax.

Legends of the attached figures below :

Fig.1 (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. 2 (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. 3 (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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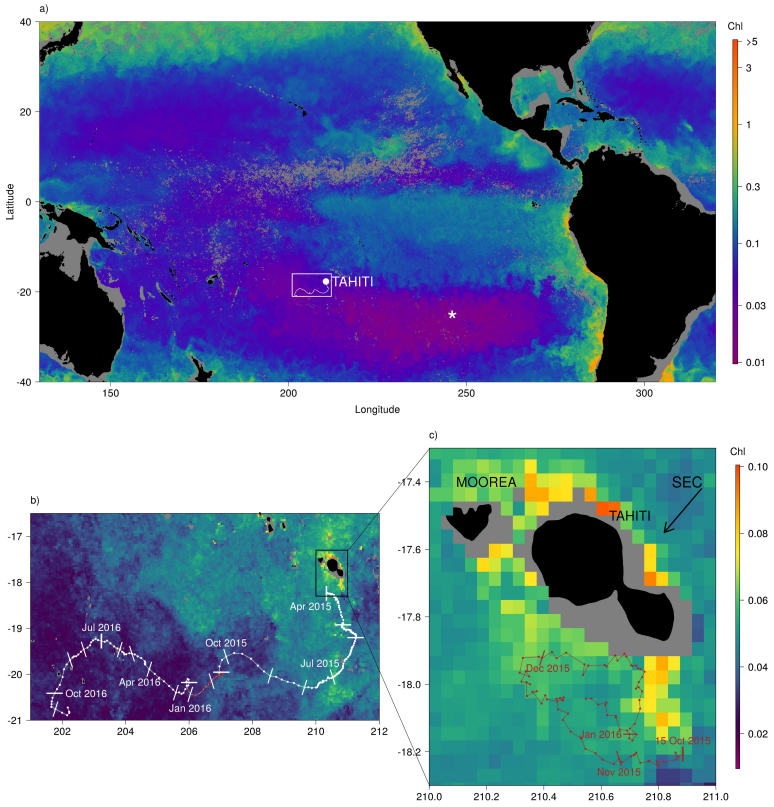

Fig. 1. New Figure 1 (see legend above)

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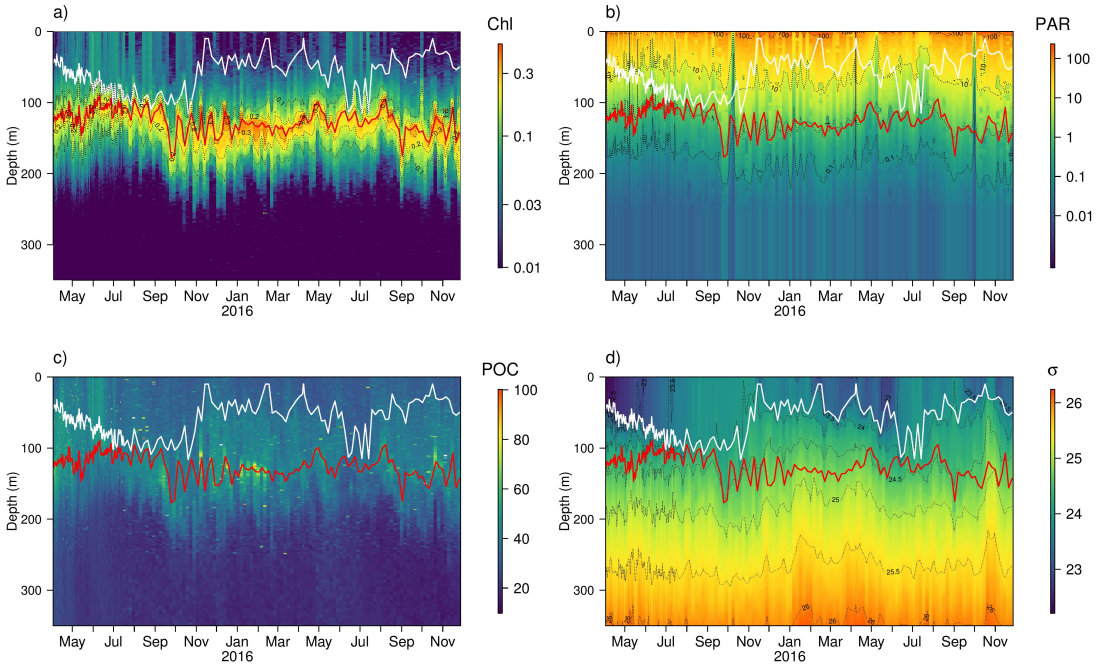

Fig. 2. New Figure 2 (see legend above)

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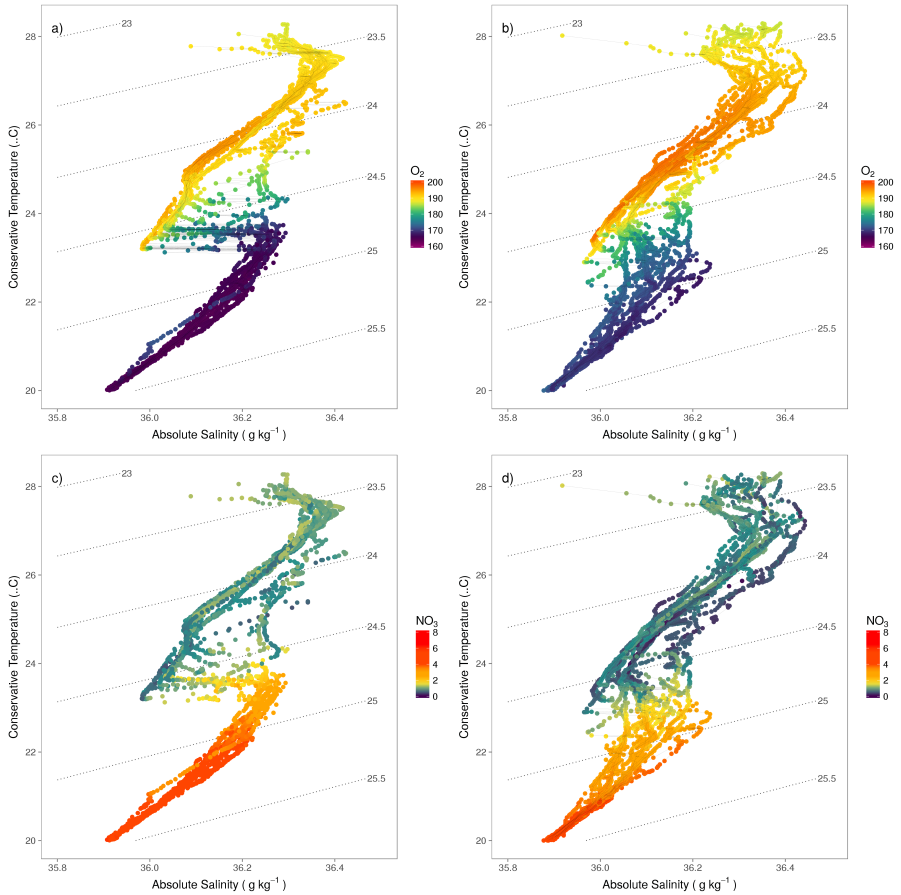

Fig. 3. New Figure 6 (see legend above)

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