

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 #1

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This is an interesting paper that looks at the chlorophyll dynamics around Tahiti using primarily data from two BGC-floats. While the paper is publishable, it is not in its current state.

Major issues

1) 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

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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 uniformly 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? 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. 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.

2) 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. And why isn't the same information shown (as sections) for the two floats? For FOpenO only sections of chlorophyll and density are shown. 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.

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

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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.

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.

Point-by-point Comments

Page 1, lines 23-24: “observations collected with two Biogeochemical-Argo (BGC-Argo) 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.

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.

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.

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.

Page 1, line 35: “bio-optical measurements suggest that the composition of phyto-

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plankton community could differ in the Tahitian wake vs. the open ocean area.” How so?

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.

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.

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.

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.

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 isolines of chlorophyll? This seems pretty intuitive and a strange way to start the discussion section.

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 what are the results that have been confirmed here?

Tables and Figures Table 2. The asterisk on bbp^* is not easily noticed (see my comment below about Figure 3), and it is not an intuitive representation of bbp/chl . Why

not just refer to it as bbp/chl? I think you mean to say Potential Temperature, not Conservative Temperature.

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.

Fig. 3. The y-axes of d) and e) have the same variable, but with different units

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.

Figure 6. What is the point of this figure? What is it telling us about the dynamics of the region?

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.

Figure 9. Show the MLD on the figure.

Typographical errors

*Pg 1, line 25: change to "The first float transited more than 1000 km"

- *Pg 1, line 26: island coast, not island coasts
- *Pg 1, line 28: consistent, not consistently
- *Pg 1, line 29: change “At the opposite” to “In contrast”
- *Pg 1, line 33 (and many other occurrences): precipitation, not precipitations
- *Pg 1, line 33: leeward of Tahiti
- *Pg 2, line 2: information into the water column
- *Pg 2, line 2: can, not could
- *Pg 2, line 16: uncertainties in (not to)
- *Pg 2, line 19: enhances the (remove the “to”)
- *Pg 2, line 27: limited to 20°S (remove “the”)
- *Pg 2, line 27: remove geographical zone
- *Page 3, line 4 (and in other place): remove all uses of the “so-called” descriptive, as its use can cast doubt on the authenticity of the term is it be used on.
- *Page 6, line 11: used not investigate
- *Figure Caption 9 and 10: no “the” before dates, ie should be 300 m depth for 04/12/2015. Same with specific time period – there should be no “the” before period 1 or period 2 etc (page 10, lines 5 and 24)

References

- * Bell, and the first Johnson et al. references are missing the journal information.
- * Lomas reference title should not be all capitalized.
- * Double-check all references for the correct syntax, and make sure extraneous information isn't in them

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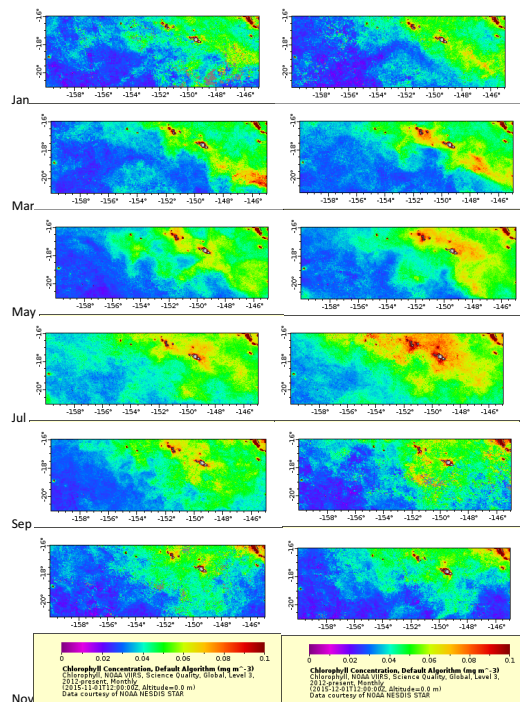


Fig. 1.