Response to Reviewer 2

for "The Fate of a Southwest Pacific Bloom: Gauging the impact of submesoscale vs. mesoscale circulation on biological gradients in the subtropics"

by Alain de Verneil, Louise Rousselet, Andrea M. Doglioli, Anne A. Petrenko, and Thierry Moutin

We thank Anonymous Reviewer 2 for their time and effort in both reading the manuscript and writing their review. Below the review is reproduced with our responses to the concerns raised.

In this manuscript, the authors use hydrographic data as well as remotely sensed data to describe the evolution of a phytoplankton bloom which was observed during an oceanographic cruise. In-situ data used in this study comes from 3 stations, SD12, LDB and SD13, which were taken from Mar 11 thru Mar 21 2015. The authors conclude that the mesoscale eddy field is responsible for the horizontal advection of the bloom and do not find submesoscale motions to be relevant in the study region during that period, as diagnosed from the gradient and the balanced Richardson number.

The manuscript is well written and describes in detail the analysis and how the authors base their conclusions. At times, though, it reads much like a cruise report. I believe the authors could be more concise and to the point.

My main concern about the manuscript is what is exactly new in this study. The authors rule out the role of submesoscale motions in the horizontal distribution of the bloom. However, the main role of such motions in oligotrophic regions would be to ignite surface chlorophyll blooms by supplying limiting nutrients to the surface. This would occur, by definition, at the onset of the blooms. The in-situ sampling in the study took place from Mar 11 to Mar 25, when the bloom, as seen from the satellite images (Fig. 6) was relatively mature.

The authors point out that it probably started on the previous December in the vicinity of an island. They are probably correct that some

type of island-induced fertilization occurred, thus alleviating nutrient limitation (Dore et al. 2008), with chaotic advection transporting material over long distances, as shown previously (Rypina et al 2010). However, with the evidence shown it is not possible to infer if submesoscale processes were at work at the beginning of the bloom. Also, Law et al. 2011 report high rates of nitrogen fixation in an oligotrophic region after the passage of a tropical cyclone, which supposedly fertilized the ocean prior to a bloom. Strong winds may or may not be important for the ignition of the observed bloom, but the authors do not mention anything about it. The horizontal evolution of the bloom is most likely controlled by mesoscale currents, as shown in previous studies (Calil et al. 2011).

Response to Comment

In this section the reviewer first details the expected role of submesoscale vertical motion in starting a bloom, and that no in situ data in this study can corroborate submesoscale motion in the bloom's ignition. In the subsequent paragraph, the relative roles of island-induced fertilization, chaotic mesoscale advection, and strong wind forcing are mentioned with references to highlight the purported lack of novelty. Framed in this manner, we understand the reviewer's opinion and here we will better communicate the novelty in this work, and what hypotheses are being tested that contribute to the scientific literature.

Firstly, the reviewer is right in that the vertical motions due to submesoscale dynamics are probably of most interest to biologists, given their enhanced magnitudes relative to mesoscale motion (Mahadevan and Tandon, 2006). However, horizontal motions are also important for biological applications by influencing patch dynamics. Biological gradients are important because they can be hotspots of predation and other trophic interactions, where in terrestrial environments these are called 'edge effects' (Harris, 1988). Therefore, it matters which circulation regime is advecting a patch of bloom water. Both mesoscale and submesoscale regimes are expected to stir and strengthen gradients in a forward tracer variance cascade (Klein et al., 1998), but submesoscale motions would concentrate more of that variance at even smaller scales. Having more patches (and by definition more biological gradients at their boundaries) at small scales means greater opportunity for these biological dynamics. Therefore, we have added the following to our introduction to better highlight this focus on horizontal motions and why it is important to diagnose submesoscale vs mesoscale regimes (and also remove unnecessary sentences mentioned later in the review):

> Introduction, Sect. 1, Page 2, Line 4 (additions in bold italics, deletions shown in strikethrough):
> Marine biological communities at any moment reflect a time-integration of the many complex interactions that occur both within the community and with the physical environment (Longhurst, 2010). ~~Despite the constant shifting and stirring that exist in a fluid medium, investigators often espouse the assumption that near "quiescent" gyres, the mean circulation's long timescale means that shipboard observations provide static, representative snapshots of a community that remains physically coherent long before and after in situ sampling. This assumption, however, is not always valid.~~ ***An important structuring mechanism of biological communities is the presence of gradients. In the terrestrial and conservation biology literature, these impacts are dubbed 'edge effects' (Harris, 1988), and have important implications for predation processes and species survival. Horizontal patch edges and biological transitions in the Ocean are liable to being advected by the surface circulation. Thus, it is necessary to identify the character of the flows that shape these horizontal gradients.***

We agree with the reviewer that it would be very interesting to know what happened at the beginning of the bloom. Nevertheless, as the reviewer noted, the proceeding of cruise events meant that we have no in situ data for the bloom at this period. Consequently, we focused on the "Fate" and not "Birth" of a Southwest Pacific bloom, as granted in the title of the paper. However, we do go into some detail regarding what can be inferred about the bloom's biogeochemical starting point in the Discussion. Therefore, as a means of better exploring the possible explanations for the bloom's origins, we have taken into account the mechanisms raised by the reviewer and tested whether these can be attributable to the bloom.

Since *Trichodesmium* blooms are rare (Westberry and Siegel 2006), they need to be treated on a phenomenological basis, as in the literature cited by the reviewer (Calil et al. 2011, Law et al. 2011). We thank the reviewer for pointing out these previous studies. Investigating these alternative mechanisms for the bloom in this manuscript can provide a further means of hypothesis testing on a rare event, which is a useful contribution to the scientific community.

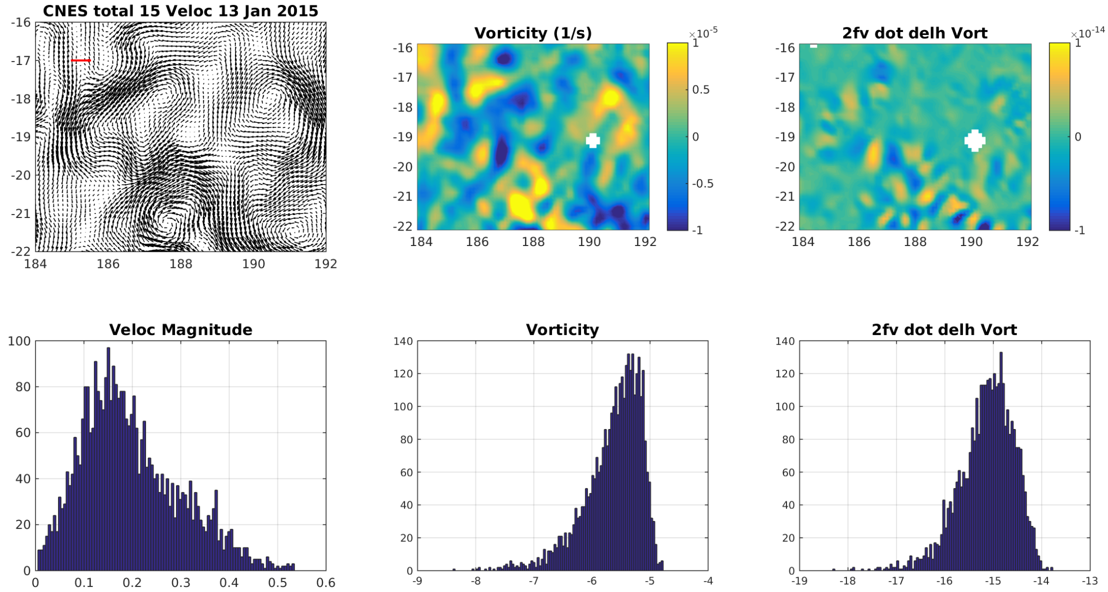
Calil et al. (2011), cited by the reviewer, diagnosed upwelling motions due to mesoscale frontogenesis/frontolysis as estimated by the Omega equation (Hoskins et al., 1978), albeit with an alternate formulation and simplifying assumptions so that satellite altimetry data can be used. We have taken their approach, and used their Eqn. 2, ignoring the second deformation term, and calculated the right hand side of the equation, eg:

$$2f_0\tfrac{\partial}{\partial z}(\mathbf{v}_g \cdot \nabla_h \zeta_g)$$

simplified further to:

$$2f_0\mathbf{v}_g \cdot \nabla_h \zeta_g$$

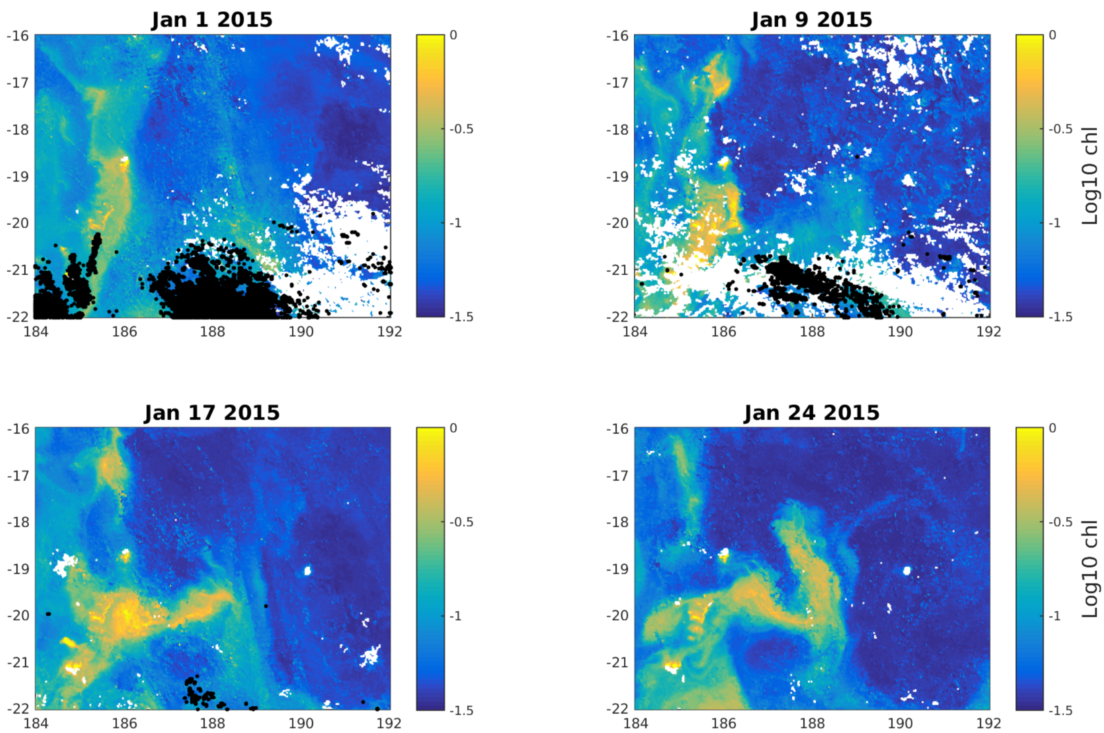
Whether this quantity is negative or positive will imply a upwelling or downwelling velocity, respectively. Below is a figure of the currents, vorticity, and this Omega equation term for Jan 13, 2015, as the bloom is about the leave the island group. Each quantity is shown with its histogram directly below it (log_{10} of absolute magnitude for vorticity and Omega term).



These distributions are similar for other days (figures and data can be provided, if requested). Of note, the omega equation upwelling/downwelling term has minima/maxima with $O(10^{-14})s^{-3}$, whereas in Calil et al. (2011), the values were three orders of magnitude higher. Additionally, the frontogenesis/frontolysis regions in Calil et al. (2011) coincided with low SST anomalies, which are not seen in our dataset (a SST version of Fig. 6 is to be added to the Supplementary Material in response to Reviewer 1).

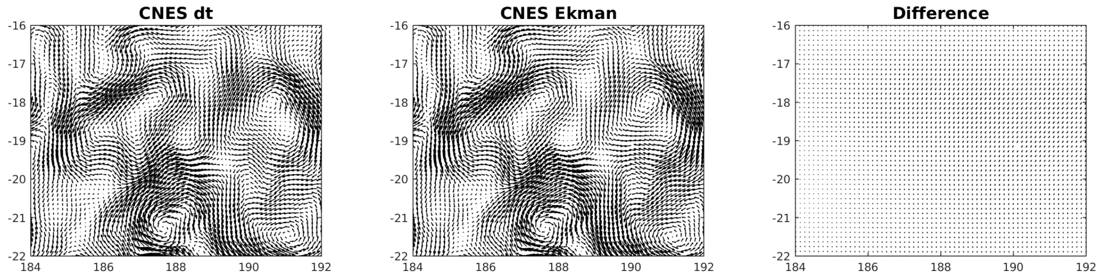
Moreover, the upwelling cited in the 2008 bloom of Calil et al. (2011) advected nutrients from a 40 m deep mixed layer, where climatological data from station ALOHA indicate nutrient reservoirs exist. The in situ data from OUTPACE, with a shallower mixed layer near 20 m, show that phosphate, one of the limiting nutrients for nitrogen fixation, was not present immediately below this layer. Instead, the phosphocline was observed near 80 m depth for both LDB and SD12, the non-bloom station in the same region. High phosphate near the surface was instead only observed to the East, in SD13 associated with the subtropical gyre. The weak forcing, lack of SST gradients, and low phosphate in the upper 80 m help rule out this mechanism as judged by remote sensing data.

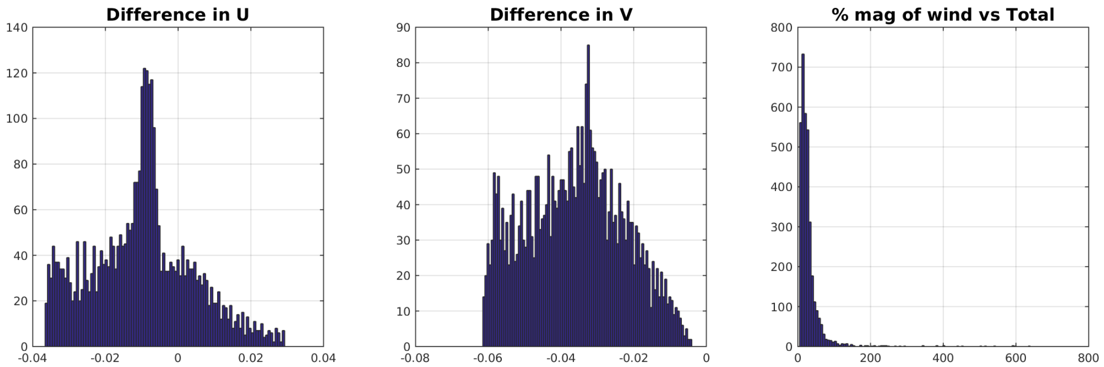
Calil et al. (2011) also highlight the requisite condition of 25°C for bloom ignition in the 2010 North Pacific bloom. The advancement of the 25° isotherm is invoked to explain the apparent eastward propagation of the large, contiguous bloom. Eastward mesoscale advection is discounted due to lack of evidence from satellite altimetry data. For the OUTPACE bloom, the SST near the island group immediately before the bloom was mostly above 25°C, though small regions of water below 25°C can be found (see Jan 2015 subpanels of Chl below, with black dots for values $\leq$ 25°C). After Jan 10, however, only a very limited region is below this threshold, and yet only a subset of all the remaining warm water experienced a bloom.



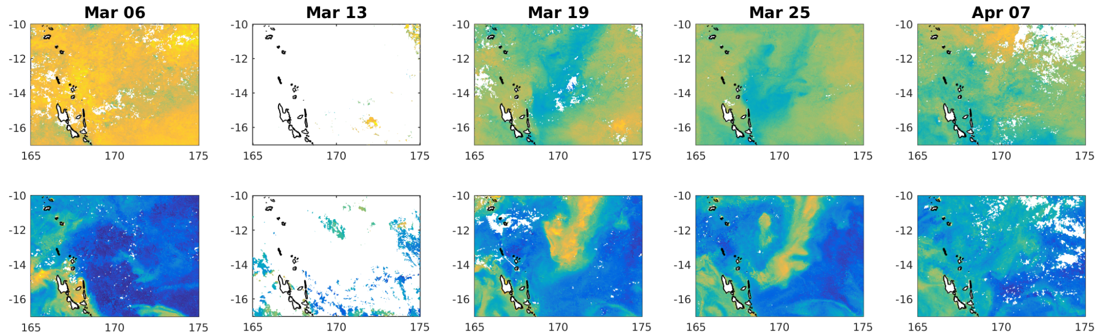
Therefore, while sufficiently warm waters may be ultimately better for diazotroph physiology (in particular *Trichodesmium*), our data support the notion that it is necessary but not sufficient to invoke a bloom. Calil et al. 2011 also noted this, with nutrients such as phosphate and iron being needed.

The reviewer points out the role of tropical cyclones in providing the necessary nutrients for blooms with reference to Law et al. (2011). Yes, wind forcing from strong events such as tropical cyclones can fertilize the ocean and bring about N_2 fixation blooms. The value-added altimetry products from CLS/CNES included an Ekman component, derived from the ECMWF ERA INTERIM windstress model. Below, moving left to right we plot the velocities without wind, with wind, and the differences, respectively, for Jan 13. Immediately below these three panels are histograms of the u, v component and % magnitude ratio of the wind-nowind difference to the magnitude of the no-wind product.





The differences between velocities with wind effects and without were small, and the magnitude differences in velocities averaged around 30% of the total. These results suggest little changes due to wind in the circulation. However, there was a strong wind forcing event within the OUTPACE region, just not near LDB. Cyclone Pam entered the Southwest Pacific in early March, and a drop in SST and increase in Chl followed in its wake. The relevant figures are shown below (the islands are the Vanuatu archipelago, top row is SST, bottom is Chl-a. Color scale is 25 to 31°C and -1.5 to 0 log_{10} Chl-a, respectively):



As can be clearly seen, the storm did indeed have a fertilizing effect. Unlike the LDB bloom, however, the elevated Chl signal did not last for an entire month. Therefore, in the face of an extreme forcing event, the biological response was transient in opposition to the LDB bloom, where there was no large-scale forcing and the bloom was persistent for over two months. While we cannot be certain that nitrogen fixation was a major factor in the storm-induced bloom, the longer timescale for the LDB bloom suggests that ongoing new production (we remind the reader that nitrogen fixation was directly observed in situ at LDB) was an important factor. This further validates our examination of the in situ data which, although not present for the bloom's ignition, can still be used to examine the circulation impacting the continuing new production being created.

In light of the previous examination of mechanisms elicited by our response to the reviewer's comments, we propose to amend our manuscript as follows:

The previous figures, which look at, in turn, frontogenetic/frontolytic forcing, SST thresholds, wind forcing, and a contemporaneous short-lived bloom due to wind forcing, will be added as Supplementary material. In particular, since in response to Reviewer 1 there are currently Figs. S1-6, these will constitute Figs. S7-10. Fig. S7 displays the frontogenetic forcing. Fig. S8 shows the wind-nowind differences. Fig. S9 has the timeseries of SST and Chl following Cyclone Pam. Finally, Fig. S10 shows the position of 25°C water.

The above discussion weighing alternative mechanisms for the bloom's ignition has been added in condensed form in the Discussion, starting where advective fluxes and forcing is considered:

Discussion, Sect. 4.1, Pg. 12, Line 1 (additions in bold):
These data therefore also remove the possibility of a massive diapycnal mixing event.
Similar surface blooms in oligotrophic regions have been investigated before, with varying mechanisms to explain their initiation. In particular, upwelling due to mesoscale frontogenesis and wind forcing, are possible causes for surface blooms (Calil et al., 2011, Law et al., 2011). While there are no in situ data during the bloom's appearance in mid-January 2015, sufficient data exist to judge these mechanisms, which would provide advective flux and diapycnal mixing, respectively. Upwelling due to mesoscale frontogenesis can be diagnosed using the Omega equation (Hoskins et al., 1978) with the assumptions employed by Calil et al. (2011) for its use with altimetry data. Calculating this forcing for the OUTPACE bloom resulted in values three orders of magnitude smaller than those for the 2008 bloom of Calil et al. (2011) (Fig. S7). As further comparison, climatological data from station ALOHA in that study place phosphate reservoirs for N_2 ***fixation at 40m depth, shallower than the depths observed during OUTPACE. These results, in addition with the lack of SST gradients one would expect (Fig. S4), make this mechanism unlikely.***

> ***Another mechanism is strong wind forcing, such as that provided by tropical cyclones. These storms have been shown to fertilize blooms in oligotrophic waters (Law et al., 2011). Using the value-added altimetry dataset with wind component, the impact of wind was evaluated and found to be relatively small (Fig. S8) and could not create deep mixing. By contrast, another region in the OUTPACE domain witnessed the passage of Cyclone Pam in early March, 2015. The satellite imagery before and after its passage corroborate the fertilizing effect of storms in this region (Fig. S9). Whereas the LDB bloom lasted for over two months, this increase in chl-a lasted approximately a month. Therefore, given the lack of strong forcing, a mechanism must be invoked that can produce blooms of greater magnitude and duration than those produced by passing storms.***

Passing reference to the temperature criterion will be added in the subsequent paragraph:

> Discussion, Sect. 4.1, Pg. 12, Line 4 (additions in bold italics): Diazotrophs, the organisms responsible for N_2 fixation, are normally concentrated in the surface layer ***in sufficiently warm water (≥ 25°C in Calil et al., 2011).*** ~~, exactly where the bloom was found, and during station LDB~~ ***The LDB bloom was found in the upper surface layer, satellite SST was warmer than the 25°C threshold for its entirety (Fig. S10), and finally*** this process was observed directly (Caffin et al., this issue).

The authors claim to use a formulation from Thomas et al. 2013, based on the balanced Richardson number, to determine "how submesoscale the observed velocity shear is." However, the criteria described in Thomas et al. 2013, as seen by the pie chart in their Fig. 1, characterizes the flow as stable or unstable to a number of instabilities.

Moreover, it considers the relative vorticity of the flow field. Therefore, while I don't think submesoscale processes were at play during the survey, this diagnostic by itself is not fully accurate for the purposes intended in this study and may be misleading for readers.

Response to Comment

The reviewer is correct in noting that relative vorticity is included in the conditions described by Thomas et al., 2013 for different instabilities. To clarify, the function of Ri_g in our manuscript is not to primarily search for instabilities. Instability criteria were not the reason that the value of $Ri_g \leq 1$ was chosen to indicate the submesoscale regime. This choice instead comes from other studies (Mahadevan 2016, McWilliams 2016). Ri_g served as a convenient formulation isolating the balanced flow component contributed by submesoscale circulation.

True, relative vorticity is included in the treatment of Thomas et al. (2013), but this is mainly to give mention that inertial instability occurs when anticyclonic relative vorticity becomes stronger than planetary vorticity. Relative vorticity of one sign or another would not affect the value of Ri_g. Therefore, if the intention is to diagnose instability conditions, as the Reviewer suggests the relative vorticity needs to be taken into account. Additionally, the Ri_g diagnostic needs to be transformed into the ϕ_{Ri_b} variable presented in the pie charts of Fig. 1 in Thomas et al. (2013).

To make these distinctions clear, we have added the following to the Materials and Methods section:

> Materials and Methods, Sect. 2.4, Pg. 6, Line 7 (additions in bold, deletions in strikethrough):
> In submesoscale flows, instabilities such as Symmetric Instability (SI) appear when $Ri \leq 1$ (Stone, 1970). ~~In order to determine how "submesoscale" the observed velocity shear is,~~ ***The submesoscale regime is commonly accepted to begin near*** $Ri \sim$ ***1,*** $Ro \sim$ ***1 (Mahadevan 2016, McWilliams 2016). In order to diagnose dynamical regimes from in situ data,*** we used a formulation from Thomas et al. (2013) to find the geostrophic component of shear, expressed as:

Materials and Methods, Sect. 2.4, Pg. 6, Line 12 (changes in bold italics):

In this paper, we characterized the flow as submesoscale when Ri_g reached this value of 1.

The Ri_g diagnostic was originally designed for instability criteria. Here, we are not searching for instabilities. The fact that Ri_g solely looks at shear due to buoyancy gradients is useful for considering submesoscale features. In order to more fully investigate the instabilities that are possible in a given dataset, the relative vorticity is required in addition to Ri_g (see Fig. 1 in Thomas et al., 2013; for example, sufficient cyclonic vorticity can make a water column stable to SI below $Ri = 1$), which is out of the scope of this paper.

As a general comment, it has long been recognized that subtropical gyres, despite their low biomass, are far from being "oceanic deserts" (Emerson et al. 1997) as they are responsible for approximately half of the export of organic carbon of the oceans.

Response to Comment

We agree with the reviewer, that though subtropical gyres have low biomass, biological rates in the region are high and contribute to an appreciable fraction of organic carbon export. We have removed this characterization from the Introduction.

An additional comment: the authors tend to use sentences such as "investigators often espouse the assumption that" or "which are what most investigators focus on." These sentences, without specific references are vague and unfit for a scientific paper. The authors should explicitly cite the works or assumptions they are supposedly challenging or simply remove these sentences.

Response to Comment

The following sentences have been removed (in strikethrough):

Introduction, Sect. 1, Pg. 2, Lines 5-8 (Same deletion as shown in Pg. 3 of this document):
~~Despite the constant shifting and stirring that exist in a fluid medium, investigators often espouse the assumption that near "quiescent" gyres the mean circulation's long timescale means that shipboard observations provide static, representative snapshots of a community that remains physically coherent long before and after in situ sampling. This assumption, however, is not always valid.~~

Discussion, Sect. 4.2, Pg. 14, Lines 29-32:
This was possible due to the bloom occurring in water no associated with the coherent, elliptic structures that move west~~, and which are what most investigators focus on for mesoscale transport~~. Instead, the bloom occurred in water outside these structures, with tortuous trajectories hyperbolic in nature (Kirwan et al., 2003 **, Rypina et al., 2010**)

Additional References (not cited in original manuscript or Reviewer's submission)

Harris, L. D. (1988). Edge effects and conservation of biotic diversity. Conservation Biology, 2(4), 330-332.

Hoskins, B. J., Draghici, I., & Davies, H. C. (1978). A new look at the ωequation. Quarterly Journal of the Royal Meteorological Society, 104(439), 31-38.

Klein, P., Treguier, A. M., & Hua, B. L. (1998). Three-dimensional stirring of thermohaline fronts. Journal of marine research, 56(3), 589-612.

McWilliams, J. C. (2016, May). Submesoscale currents in the ocean. In Proc. R. Soc. A (Vol. 472, No. 2189, p. 20160117). The Royal Society.

Westberry, T. K., & Siegel, D. A. (2006). Spatial and temporal distribution of Trichodesmium blooms in the world's oceans. Global Biogeochemical Cycles, 20(4).