We would like to greatly thank the reviewer for the time he/she granted to this review. We gratefully acknowledge his/her in-depth reading and thoughtful comments on our manuscript. We carefully considered the provided feedback and criticism in preparing our revision. Please find below our response. We have made major efforts to improve the paper, including:

1. *Addition of comparison to shipboard chlorophyll data from KEOPS2*
2. *Expansion of the description and details regarding the correction for quenching (noting that no conclusions of the paper depend on this correction, because use of only the night time data yields the same outcomes).*
3. *Addition of Lagrangian trajectory maps and analysis to relate chlorophyll inventories to water parcel histories relative to the Kerguelen plateau*
4. *Expanded analysis of bio-profiler chlorophyll observations, using water column inventories rather than concentrations (as recommended by both reviewers).*

We believe the paper is now greatly improved and that the conclusions are well qualified.

Anonymous Referee #2

Received and published: 15 January 2015

General comments

The study by Grenier et al. presents the analysis of data collected by four bio-optical profiling floats in the Kerguelen region. The general goal of the study is to gain insights into the role of water-column physical properties in controlling the distribution and dynamics of biological properties, especially phytoplankton biomass. Three specific questions are addressed: i) Do ocean color satellites provide an accurate view of the dynamics of the water-column phytoplankton biomass? ii) Are the physical and biological properties correlated, where and why? iii) What is the fate of the organic matter produced in surface waters and can carbon export be (roughly) estimated from bio-profiler measurements? The study region and the scientific questions are sound, exciting and very relevant to BG. The authors make use of innovative, appropriate tools to address their objectives. Nevertheless, the methodology is unclear on several occasions and the interpretation of the results need to be improved and strengthened. This is especially true for questions i) and ii) (sections 4.1 and 4.2) where the methodology need to be reconsidered (see my comments below). Therefore I recommend substantial revision before the paper can be accepted for publication.

Specific comments

Method (sections 2.1 and 2.2):

- Throughout the text “backscatter” should be corrected to “particulate backscattering” or ideally “particulate backscattering coefficient”.

Authors’ response:

We corrected the term as suggested throughout the paper, and added detail on how this value was calculated from the sensor measurements.

- p. 17418: It might be desirable for readers to provide a justification of the choice of the sensors, for example O2 for biological production and respiration, chlorophyll fluorescence for chlorophyll concentration as an indicator of phytoplankton biomass, particulate backscattering as a proxy of particle load or POC, etc.

Authors’ response:

We added the justification of the choice of the sensors.

Modified text:

Chlorophyll-a fluorescence is a useful proxy for chlorophyll-a concentration and standing stocks of phytoplankton biomass (Falkowski and Kiefer, 1985; Huot et al., 2007). Particulate backscattering provides a good proxy for particulate organic carbon (Stramski et al. 2008; Cetinić et al, 2013).

- p. 17419 l. 11-12 “oxygen, phytoplankton fluorescence, and particle backscatter were sampled at 10 decibar intervals”: Why such a coarse depth resolution for the biological parameters, especially when one of your goals is to study biological subsurface maxima? As indicated in section 2.2 p. 17422 l. 15-16 “the low 10 m vertical resolution of the observations… so we have to use the unfiltered observations”. How do you determine whether you are observing a spike or a maximum?

Authors’ response:

We agree that higher resolution would have been desirable, but was not achievable at the time. We have made every effort to not over-interpret vertical variations owing to the low resolution of the data. Interestingly, as discussed in more detail in our response to Reviewer1, our new Figure 2c comparing to the 2 m resolution of the shipboard data measured during KEOPS2 shows that the lower resolution bio-profiler data nonetheless captured the main features and with similar spikiness.

In order to determine if we are observing a spike or a maximum, we estimated the error of the bio-optical sensors (“noisiness”) by calculating the standard deviation (SD) around the mean value of the bio-optical variables between 500 m and 1000 m depth –where the signal is close to its background value– and calculated the coefficient of variation $SD/MEAN_{500\text{-}1000}$ ratio to express it in percent. As bio-profiler #1 did not extend as deep as the following ones, there are no available data as these depths so we also calculated the error of the fluorometer between 250 m and 300 m depth. We estimated the average fluorometer error to $SD/MEAN_{Chl\text{-}a(250\text{-}300)} = 22 \pm 10$ % and $SD/MEAN_{Chl\text{-}a(500\text{-}1000)} = 21 \pm 7$ % and the average backscatterometer error to $SD/MEAN_{bbp(500\text{-}1000)} = 14 \pm 4$ %. Considering these results, we decided to update the threshold we defined to characterize chlorophyll concentration subsurface maxima. We chose a threshold twice as large as the fluorometer error and increased it to 60% (instead of 30%, which was initially chosen to be consistent with Guinet et al. (2012) study).

- Fig. 2a: I recommend using a different y-axis scale for bio-profiler 1. As is, it is almost impossible to say anything about the other 3 profilers.

Authors’ response:

Accordingly to the reviewer’s suggestion, for the sake of clarity, we added a secondary y-axis in Figure 2a associated to the bio-profiler #1 chlorophyll concentration. To improve the assessment of the drift, we added a new table, Table 2, which characterizes, for each bio-profiler, the slope of the linear trend of the mean chlorophyll concentration and b_{bp} evolutions throughout the sensor lives, for the two considered depth layers ([250-300] m and [950-1000] m). We also compared the mean surface value of each parameter with its average drift along the whole sensor life (third column, in %), an expression that we think to be clearer and more relevant to characterize the drift significance. We used the revisions done for Figure 2a and the Table 2 results to strengthen our discussion about the instrument drifts.

- Fig. 2b: Too many profiles are shown. The process you are trying to illustrate will be more obvious if you select one or two examples with a night profile, day profile and quenching-corrected day profile.

Authors’ response:

We revised this figure accordingly to both reviewers’ suggestions. We now only show 3 subplots (vertical profiles of chlorophyll concentration before the quenching correction, after the correction, and the associated particulate backscattering profiles), with one profile per bio-profiler.

- The quenching correction method should be presented in a clearer (maybe more detailed) manner. This is particularly important because the paper which the method is based on has not reached the publication stage (it is cited as Sackmann et al., 2008, Biogeosciences Discussion). In the presentation of the method you say “Below the depth of daytime quenching we determined the fluorescence to backscattering ratio (over the depth range where it was constant), and multiplied this ratio by the

backscattering signal to extrapolate the fluorescence signal to the surface" (p. 17421 l. 27-29 and p. 17422 l. 1). I think the sentence is misleading as it gives the impression that the depth at which quenching starts to occur is known. It is unclear to me how the authors are able to determine whether the particulate backscattering to chlorophyll fluorescence ratio varies because of the quenching effect or because of changes in the nature of the particle assemblage. Also, does the layer of constant ratio must have a minimum thickness? I am assuming this is all based on the idea that the backscattering and chlorophyll properties should be uniformly distributed within the mixed layer. But then why not simply extrapolate the chlorophyll fluorescence value taken at the base of the mixed layer up to the surface? This would circumvent the hypothesis of a constant backscattering to chlorophyll fluorescence ratio and not introduce noise into the chlorophyll fluorescence data (as indicated by the authors p. 17422 l. 14).

Authors' response:

First we note that none of the conclusions of our paper depend on the details of our quenching correction, because all of the conclusions hold if only the night time data is used. Nonetheless, we have made great efforts to further develop, describe, and quantify our approach to correcting for quenching. In doing this, we thought about your concern for a long time before deciding which method we thought the best to apply. We finally chose to keep the method suggested by Sackmann et al. (2008). However, we did several modifications that, we hope, give more robustness to our approach. First, for several chlorophyll results shown in the figures of the paper (Figures 4, 5 and 7), we distinguished night data from daytime data. We also flagged some of the quenching corrected daytime profiles still exhibiting decreasing surface concentrations leading to values lower than the lowest surface chlorophyll concentration of the night profiles, for which quenching might have been under-corrected (see the details in the caption of Figure 2b). Overall, we observe similar distributions for night and corrected day profiles, which converge consequently toward similar conclusions. The features and statistics that characterized the fluorescence profiles of our study are also consistent with the results of Guiney et al. (2012) and Biermann et al. (2015), who used different quenching correction method.

Furthermore, we believe that testing this method contributes to active discussion of the best way to use daylight Chl-a fluorescence data obtained from platforms which may not have as good night time coverage as our floats (such as sensors deployed on seals, on standard ARGO 10- day profile interval missions, or on float missions that target co-measurement with daytime satellite ocean colour observations). Finally, the assumption of a constant chlorophyll concentration within the mixed layer may not be less strong than the method proposed here, based on the particulate backscattering, considering the small chlorophyll concentration variations observed in night profiles within the mixed layer (see Figure 2b). Please refer to the modified text below to get the details we added about the correction method and the tests we did to avoid to use a spiked surface particulate backscattering value to correct our fluorescence profiles.

Modified text (in section 2.2):

Fluorescence signals were also corrected for daytime quenching. This effect, which derives from the photo-inhibition of phytoplankton by an excess of light (maximum at midday), decreases surface fluorescence (Falkowski and Kolber, 1995; Kiefer, 1973) and, if uncorrected, can produce a false impression of subsurface maxima in fluorescence derived chlorophyll profiles. We explain this correction and its evaluation in considerable detail in the following paragraphs, but note that none of the conclusions of the paper depend on these corrections because the same overall results are obtained if we use only Chl-a fluorescence signals collected at night. Our purpose in detailing the correction is to contribute to active discussion of the best way to use daylight Chl-a fluorescence data obtained from platforms which may not have as good night time coverage as our floats (such as sensors deployed on seals, on standard ARGO 10- day profile interval missions, or on float missions that target co-measurement with daytime satellite ocean colour observations).

We defined the daytime profiles, potentially affected by quenching, as profiles acquired between one hour after local sunrise time and one hour after local sunset time, to allow for dark acclimation since quenching effect could still persist after sunset (Sackmann et al., 2008). Daytime profiles from

the four bio-profilers are shown to illustrate this effect (continuous lines in Figure 2b, left panel). To correct this bias, we applied the method of Sackmann et al. (2008), which uses the particulate backscattering signal as a relative reference. For the sake of consistency with the other studies of this issue, we defined the mixed layer depth, MLD, as the depth where density increased by 0.02 kg m-3 relative to the density at 10 m (Park et al., 1998). Within the deeper half of the mixed layer (targeted to be below the depth of daytime quenching), we determined a mean value of the (relatively constant, see below) Chl-a fluorescence to b_{bp} ratio (at depth defined as $d_{F/bbp}$) and multiplied this ratio by the b_{bp} signal at this depth to retrieve the Chl-a fluorescence. Then, we multiplied this same ratio by the surface b_{bp} value to estimate unquenched surface Chl-a fluorescence, and interpolated between these two depths to obtain the unquenched Chl-a fluorescence profile. This assumes that phytoplankton populations were not stratified within the density defined mixed layer. This works particularly well for deep mixed layers (>50 m) which exhibit relatively constant Chl-a fluorescence/b_{bp} ratios (to within ~10%) in their deeper half. In less than 3% of the daytime profiles, in average, we could not identify a region of uniform Chl-a fluorescence/b_{bp} and apply the quenching correction; consequently, these profiles were not used further.

The greater spikiness of the b_{bp} profiles in comparison to those of fluorescence (as illustrated in Figure 2b, right panels) means that this quenching correction introduces some noise into the daytime chlorophyll estimates. In principle, this could be filtered or smoothed, but the low 10 m vertical resolution of the observations made this rather uncertain and so we have used the unfiltered observations throughout this paper (except in Figure 9f below where we show median-filtered particulate backscattering profiles for the sake of visual clarity). Note that to avoid to correct the surface Chl-a fluorescence with a spiked surface b_{bp} value and create a "b_{bp} spiked" interpolation, we verified before that the b_{bp} surface value did not seem to be spiked, assuming that surface value should not exceed more than ±50% of the b_{bp} value at the depth $d_{F/bbp}$, since within the mixed layer. This threshold was defined after assessing the backscatterometer precision (coefficient of variation of b_{bp}, equal to the standard deviation to mean ratio) between 500 and 1000 m depth of 14 ± 4% in average. If the surface b_{bp} value was considered as spiked (less than 4% of the daytime b_{bp} profiles, except for bio-profiler #4 for which it reached 9%), the test was done with the second depth value, until a "non-spiked" value was found, and the value was then extrapolated to the surface.

Space/time evolution of the biomass plume and sampling by the profilers (section 3.1):

Based on section 3.1 and Fig. 1, I found the space/time evolution of the Kerguelen bloom quite difficult to follow. I recommend several points to be addressed to make this point clearer.
- I suggest the authors provide a general description of the bloom. When/where does the bloom typically start, propagate and decline (if, of course, a recurrent pattern can be observed)? I assume the profilers were deployed to sample specific features of the bloom. Which ones? Please specify how the date and location of profiler deployment were selected.

Authors' response:

General descriptions of the bloom have been presented elsewhere and referenced in the introduction (Blain et al., 2007, Mongin et al., 2009; Trull et al., 2014), and are available in the supplementary material as animations of satellite surface chlorophyll images. As also described in the introduction, the point of our paper is to examine the mesoscale structures, which cannot be presented in a general description of the bloom. The profilers were deployed at locations thought likely to sample a wide range of these mesoscale structures, both north and south of the Polar Front.

- I don't quite understand the authors' selection of the satellite ocean color composites in Fig. 1. Why showing images from year 2013 when there were no profiler deployed in the region? In addition it is almost impossible to distinguish the trajectory of profilers 2, 3 and 4 in panels g, h and i. You may want to show in grey scale the dates of data acquisition for each profiler. Alternatively you may have a few selected composites showing the start, end and intermediate stages of acquisition of the profilers. You may also want to use identical color scales for all panels. This will simplify the reading

of both the text and figures. Finally, adding to the maps important features you frequently refer to in the text may also help, e.g. Polar Front, Gallieni Spur…

Authors' response:

We showed images from year 2013 to illustrate the spring conditions and give information about the surface chlorophyll concentration distribution prior to the deployment of the bio-profiler #2, #3 and #4 around the end of January 2014. We improved the representation and distinction of the trajectories of the 3 bio-profilers using 3 different colours. We also used identical colour scales for all panels to more easily observe the monthly and seasonal differences of surface chlorophyll distribution. Finally, we added the Polar Front location (from Park et al., 2014). Important features such as the Gallieni Spur and the Kerguelen Plateau have been added on the maps in Figure 3 rather than in Figure 1, to not overload too much Figure 1.

- Table 1 should provide the date of the last profile acquired by each profiler. Although essential this information is only found below the x-axes of Fig. 3 (panels 1-4).

Authors' response:

We added this information in Table 1, as suggested.

Water-column chlorophyll content versus surface chlorophyll concentration (section 4.1):

I am not convinced by, or at least don't understand, the authors method for assessing whether satellite-derived surface chlorophyll values reflect the entire water-column chlorophyll content.
- How do you define a subsurface chlorophyll maximum? Fig. 4 right column shows that some of the maxima are located at a depth of 5 or 10 m. I would call these "surface maxima" and they are unlikely to be missed by ocean color satellites.

Authors' response:

The reviewer's concern helped us to clarify this issue for ourselves, and we have significantly modified our calculation and our discussion of these issues:

1. *We estimated the noisiness of the fluorometer measurements using deep ocean data (see the details in our response to reviewer 1) and accordingly changed our criterion for subsurface maxima to 60% to ensure that the detected features were not noise.*
2. *We note that the presence of shallow subsurface maxima are not that important to the issue of potential satellite underestimations of water column chlorophyll inventories, rather the statistics in Table 3 on their occurrence are focused on illustrating the importance of the quenching correction.*
3. *For the issue of the potential satellite underestimations we now focus on only those subsurface maxima that occur below the mixed layer, as shown in the revised Figure 4.*

I don't understand either the criterion of more than 30 percent or 100 percent for identifying how "large" a subsurface maximum is. If your criterion is basically to compare any single chlorophyll value to the surface value (i.e. first data point) then this may be extremely sensitive to fluorometer noise.

- The interpretation that subsurface maxima are "relatively rare and localized" features (p. 17428 l. 12) is not obvious to me from Fig. 4 and Table 2: Subsurface maxima show up throughout most of the study region and their occurrence exceeds 40 percent except for profiler 2.

Authors' response:

As mentioned above, the assessment of the bio-optical sensor error allowed us to update the threshold above which subsurface chlorophyll concentrations are considered as real features. This update reduced the occurrence of bio-profilers #1 to #4 subsurface chlorophyll maxima to 19%, 4%, 20% and 26% (average 17%), respectively. However, we changed the misleading "rare" expression to the more appropriate "occasional" one. Considering the spatial distribution of these subsurface maxima

(Figure 4) and the relationship between water ages and origins and chlorophyll concentration (new Figure 8), the map of their locations in Figure 4 shows that these features are localized.

- I am not sure of the purpose of the comparison of the mean chlorophyll concentrations calculated over the 0-50 m layer vs. the 0-200 m layer (Fig. 5). First, the 0-50 m layer is not representative of the surface layer typically seen by ocean color satellites. I suggest using as a limit the first penetration depth (I realize you don't have PAR measurements but it may be estimated using the chlorophyll profiles) or a depth of 10 m which may be more appropriate for high chlorophyll waters.

Authors' response:

We agree with this, and have accordingly changed to using the surface chlorophyll values (10 m), because while we do not have PAR measurements, it is clear that the penetration was much shallower than 50 m. Using the model of Morel and Maritorena (2001; their Figure 6), and our surface chlorophyll concentrations which do not go lower than 0.4 mg m^{-3} with 0-200 m integrated chlorophyll inventories that do not go lower than 40-50 mg m^{-2} (Figure 5a; excluding flagged profiles), the euphotic depth z_{eu} should not exceed much more than 50 m. Therefore, an upper estimation of the penetration depth z_{pd} would not exceed max(z_{eu})/4.6 (Gordon and McCluney, 1975) or ~ 10 m. Considering our 10 m vertical resolution for the bio-optical variables, we only have one value, the surface value, within the [0-10] m depth range, which is consequently the most accurate value than we can use as the z_{pd} value.

Second, the chlorophyll concentration averaged over the 0-200 m layer does not bring much information on the total phytoplankton biomass nor on its vertical distribution. Instead I recommend using the chlorophyll concentration integrated within the water column (using as a limit either the 200 m depth or the euphotic layer depth).

Authors' response:

We agree and we replaced our use of the 0-200 m chlorophyll average concentration by the 0-200 m integrated chlorophyll amount.

- p. 17430 l. 2-8 "As shown in Fig. 5, … the surface estimates are consistently higher than the total ones for chlorophyll concentrations higher than 1ug L-1. This suggests that variations in surface layer mixing, and the associated impact on the vertical distributions of chlorophyll, contribute insignificant bias where chlorophyll was low (<1ug L-1) but lead to over-estimation where chlorophyll was moderate to high (>1ug L-1)." I think this does not show much but simply results from the averaging over the 0-200 m layer which artificially decreases your index of the water column biomass.

- p. 17430 l. 8-10 "satellite images tend to overestimate the dynamic range of total chlorophyll inventories, although this effect is relatively small, less than a factor of two even for surface chlorophyll concentrations as high as 10 ug L-1 . Given that our bioprofilers did not sample close to the plateau during the early summer peak in biomass as seen in satellite images, it is possible that there could be greater biases under these conditions": I think that if you plot the integrated content instead of the mean concentration a different picture will emerge. Typically surface data from ocean color satellite will fail at representing the dynamics of phytoplankton biomass in relatively low chlorophyll regimes where there is a subsurface or a deep maximum. In such regimes the satellite will underestimate the water column integrated biomass.

Authors' response:

Following your suggested use of water column inventories rather than concentrations, all this paragraph has changed in the revised version, as written below.

Modified text:

Subsurface chlorophyll maxima beyond the reach of satellite imagery can be thought of as a specific class of the wide range of possible chlorophyll distributions (such as varying thicknesses of relatively constant near-surface biomass layers, or changes in the rate of decrease of biomass with depth) that could introduce bias between surface concentration and water column inventory perspectives. To gain

perspective on the overall importance of these possibilities, we compared surface chlorophyll concentrations measured by the profilers (using the shallowest ~10 m depth observation since this was reliably within both the 1/e satellite ocean colour penetration depth and the mixed layer) with their column inventories calculated from all observations in the top 200 m (since chlorophyll distributions generally reduced to background values below this depth). These comparisons, shown in Figure 5a (left column), display reasonably linear relationships over almost the entire range of both night and daytime observations. This was especially true for bio-profilers #1 and #3 (correlation coefficients r^2=[0.60-0.85]), which include high chlorophyll values (greater than 2 mg m^{-3} for the surface concentration and greater than 160 mg m^{-2} for the 0-200 m inventory). Most of the flagged daytime profiles (red circles in Figure 5a) seem to be shifted slightly left of the linear regression lines, suggesting that they may well represent under-corrected quenched chlorophyll rather than true features. Overall, qualitatively, these quite linear relationship between surface Chl-a concentration and 0-200 m integrated Chl-a content suggests that satellite observations are reasonably good indicators of the spatial distributions water column chlorophyll inventories.

Concerning the particulate backscattering signal, the linear correlations between surface values and inventories were generally not as strong as for Chl-a, except for bio-profiler #3, as shown in Figure 5b (right column: r^2 = [0.29-0.74]. It appears that surface b_{bp} values lower than ~2 x 10^{-3} m^{-1} vary similarly to the 0-200 m b_{bp} inventories, whereas higher surface values exhibit noisier correlations when compared to the 0-200 m integrated b_{bp} contents (see the slope breaks in the relationship between surface and 0-200 m integrated b_{bp} in Figure 5b). The origin of this non-linearity is not clear, and its evaluation is potentially compromised by the spikiness of the b_{bp} records and their poor vertical resolution. The particulate backscatter profiles (Figures 2b, 3e and 9e) suggest that spikes may be particularly common at the base of the mixed layer and below, and thus might reflect differential control of phytoplankton and total particle populations. Future deployments with improved firmware to yield higher resolution may be able to advance the interesting possibility that backscatter information can provide ecosystem perspectives beyond phytoplankton biomass alone.

Because our qualitative assessment indicated that surface Chl-a concentrations provide a relatively unbiased indication of the water column Chl-a inventory, we now try to go a little bit further towards a quantitative assessment of possible biases between satellite and in-situ Chl-a perspectives. First, we compared the coefficients of variation (i.e. the ratio of the standard deviation to the mean) of the surface chlorophyll concentrations and of the water column inventories. Using only the night data to avoid quenching correction uncertainties, surface distribution coefficients of variation (#1: 82%; #2: 20%; #3: 39%; #4: 43%) revealed very similar relative dispersions to the water column (0-200 m) inventory coefficients of variation (#1: 84%; #2: 20%; #3: 34%; #4: 31%). Thus, satellite images reasonably reflect the relative range of mesoscale variability in water column phytoplankton biomass accumulations. Surprisingly, surface chlorophyll values (i.e. satellite images) would tend to slightly overestimate the relative dispersion of Chl-a data for bio-profilers #3 and #4, despite those profiles exhibiting the largest numbers of night subsurface maxima (in %, Table 3). This means that the association of high surface chlorophyll concentrations with shallow chlorophyll layers was more important than the presence of subsurface chlorophyll maxima in determining the relationships between surface and water column inventories.

To further explore this issue, we calculated expected water column inventories for chlorophyll layers confined to the physical mixed layer depths at the time of observation (by multiplying each surface concentration by its associated mixed layer depth, MLD). This is akin to trying to improve satellite assessments using mixed layer depth information from, for example, standard ARGO floats that measure only temperature and salinity. These comparisons are shown in Figure 6a and reveal that this approach badly underestimates water column inventories (at least with our MLD definition) and that this underestimation is very common. Most of the "0-200 m integrated Chl-a/(surface Chl-a × MLD)" ratios range from 1/1 to 4/1, with a few profiles of bio-profilers #1 and #3, at the time when they recorded the highest bio-optical values, reaching ratios of 20/1 (profiles ~ 70-130 for bio-profiler #1 and profiles ~ 0-70 for bio-profiler #3). Moreover, the colour coding in Figure 6a shows that this bias is strongest for shallow mixed layers in general. In other words, the presence of significant amounts of chlorophyll below the mixed layer is very common (though generally not as

local vertical chlorophyll maxima, for which our statistics confine the occurrence of those exceeding 60% of surface to 17% of the sampled locations and those exceeding 100% of surface to 11% of the sampled locations). Notably, this bias still persists strongly if we change our MLD definition to the much larger criterion of Levitus (1982; density increase of 0.125 kg m^{-3} relative to the density at 0 m). For this criterion, the (surface Chl-a × MLD) estimation ranged between half and twice the 0-200 m integrated Chl-a content for MLD deeper than 60 m (close to half for MLD ~ [60-90] m and surface Chl-a < 2 µg L^{-1} to close to twice for MLD > 120 m and surface Chl-a > 2 µg L^{-1}). However, (surface Chl-a × MLD) estimations are still twice to four times lower than the 0-200 m integrated Chl-a content recorded by the bio-profilers when the MLD ranges between 40 and 60 m (not shown).

The most probable explanation for these observations is that the mixed layer at the time of observation was shallower than at the time of generation of the biomass. This is of course expected as a result of seasonal shallowing of the mixed layer, but the magnitude of the effect is important to recognize (as we have shown above) it is well above what could be corrected using some other mixed layer depth criterion. Interestingly, there appears to be a relatively simple hyperbolic relationship between the ratio "0-200 m integrated Chl-a" / "surface Chl-a × MLD" (hereafter designated as X) and MLD, as shown in Figure 6b for the MLD definition of Park et al. (1998). It also holds for the MLD definition of Levitus (1982). This X vs MLD hyperbola reaches an asymptote of X ~ 1 for MLD values close to the 150-200 m depths of regional winter mixed layers (visible as temperature minima remnant signatures of winter cooling in profiles south of the Polar Front in Figure 3b). Moreover, the curve is reasonably well parameterized by $X \sim MLD^t/MLD^w$, in which the superscripts t and w indicate mixed layer depths at the time of observation and the end of winter, respectively. This relationship could arise if most biomass accumulation occurred in early deep mixed layers with subsequent stratification adding little additional biomass, or if mixed layers shallowed and deepened episodically as biomass accumulation developed throughout the season.

Overall, these results emphasize the major challenges that are present for connecting surface chlorophyll distributions to total water column biomass and primary productivity, since they reveal that physical mixed layer depths are often not a reliable guide to biomass distributions. These physical and biological responses seem to be modulated differently on diel, weather, and seasonal timescales, and are also affected by the mesoscale and sub-mesoscale interleaving of water parcels. The quantification of near surface mixing (i.e. going beyond the limited mixed layer depth concept) is currently under very active exploration and debate in the context of seasonal drivers of production (Behrenfeld, 2010; Taylor and Ferrari, 2011), and these data reveal the need to extend those perspectives to shorter time and space scales. The presence of significant amounts of chlorophyll below the mixed layer is also important to its ultimate fate –if this biomass is not re-entrained then it may well contribute preferentially to export and to mesopelagic oxygen consumption (issues which we revisit in Discussion section 4.3 below).

- p. 17430 l. 13-14 "We also performed the same calculations for the backscatter signal, and found similar non-linearity": on the same page l. 2 you say that for chlorophyll "the surface and total estimates show linear relationships".

Authors' response:

We corrected this inconsistency. We found a smaller correlation between surface concentrations and 0-200 m integrated contents for the particulate backscattering signal than for the chlorophyll data.

Correlation between chlorophyll biomass and oceanographic variables (section 4.2):

As stated by the authors in the introduction, factors such as mixed layer depth and upper water column stratification (p. 17418 l. 12-13) play a role in controlling phytoplankton production. This is through their effects on light availability. This is an important question and bio-profilers should bring interesting insights. Yet I am not convinced by the authors' approach to the question nor by their interpretation of the data. What is the rationale for using surface layer (0-50 m) data instead of mixed-layer or full water-column data? This does not account for inter-site variation and exclude a large

fraction of the phytoplankton biomass. Also why splitting your dataset into two subsets of rich- and moderate-biomass regions, especially when there is such an overlap between the rich (1 to 9 ug L-1) and moderate (0.5 to 3 ug L-1) regimes? Instead I would analyze independently (and then also all together) the time series collected by each profiling float to determine if changes in oceanographic properties can explain changes from low to high biomass. Finally the selection of temperature, salinity, MLD etc. may not be optimal for the goal you are trying to achieve. For example, the MLD is not necessarily a good indicator of active water column mixing, mixing history and light availability to phytoplankton. I suggest trying alternative indicators, e.g. the ratio of MLD to euphotic layer depth may provide insights into the mixing/light conditions. The shape of the chlorophyll profile may also be indicative of photoacclimation processes.

Authors' response:

We understand your concerns and we adopted your suggested approach and completely revised this section of our analysis and text:

1) we replaced the surface chlorophyll concentration by the 0-200 m integrated chlorophyll inventories and

2) we used a threshold, 200 µg L^{-1}, and only represented values above this threshold for the rich areas (i.e. that reach very high Chl-a concentrations) and values below this threshold for the poor to moderate areas and modified accordingly the Figure 7 (old Figure 6).

3) we added comparison to stratification as represented by the maximum Brunt-Väisälä frequency squared (N^2). Based on this metric, stratification seems to play a minor role since no clear relationship was found with the 0-200 m integrated Chl-a distributions.

4) we expanded discussion of the relationship between the depth of the chlorophyll distributions relative to the mixed layer depth (using two different criteria for mixed layer depth).

- p. 17430 l. 3-4 "The distributions of chlorophyll with these properties showed decreases on either side of these values, suggestive of mixing with surrounding water": I do not understand this sentence. To me two major features can be seen in Fig. 6a. One part of profiler 1 time series shows positive correlation between temperature and chlorophyll concentration. Another part of the time series, similarly to profiler 3, shows the opposite trend (i.e. increase in chlorophyll with decreasing temperature). This somewhat reflects in density data, albeit not in MLD data. Which pattern do you interpret as suggestive of "mixing with surrounding waters"? Does this imply that dissolved iron from the plateau locally leads to biomass increase, or that biomass-rich waters from the plateau mix with local waters?

Authors' response:

We believe that 3 different endmembers are characterized in Figures 7a, 7b, 7c and 7f. The first class of waters exhibits warm light waters, slightly oversaturated in oxygen (T ~ 5.5 °C, S ~ 33.81, σ ~ 26.65-26.7 kg m^{-3}, $O_{2\ sat}$ ~ 103%). The second class of waters exhibits much colder and denser waters, also slightly oversaturated in oxygen (T ~ 3 °C, S ~ 33.85, σ ~ 27 kg m^{-3}, $O_{2\ sat}$ ~ 103%). Both have moderate water column Chl-a contents (~200 mg m^{-2}). The third class of waters exhibits intermediate physical properties but much higher oxygen saturation states (T ~ 4.2 °C, S ~ 33.83, σ ~ 26.85 kg m^{-3}, $O_{2\ sat}$ ~ 110%) and much higher 0-200 m Chl-a contents (~1000 mg m^{-2}). The linear relationships between the Chl-a contents and the physical properties represent the mixing zones, as the linear relationship between Chl-a and $O_{2\ sat}$. Thus, from these patterns, we believe that biomass-rich waters from the plateau mix with local waters poorer in Chl-a.

Modified text:

As shown in Figure 7 (a, b and c), the richest biomass regions encountered by bio-profiler #1 in 2011 and bio-profiler #3 in 2014 were associated with waters with very similar properties, specifically moderate temperatures (3.5-5 °C), high salinities (33.82-33.85), and thus relatively high densities (sigma-theta values of 26.7-26.9 kg m^{-3}). The bio-profiler #1 distributions of chlorophyll with these properties showed linear decreases on either side of these values, suggestive of mixing with

surrounding waters much poorer in Chl-a. This characteristic is also observed between integrated Chl-a and mean surface oxygen saturation ($O_{2\ sat}$, Figure 7f), for which the high $O_{2\ sat}$ states (reaching 10%) indicate oxygen production in these high biomass waters (since these values exceeding expected from processes such as warming or bubble injection; Shadwick et al., 2014).

- p. 17430 l. 8-10 “For the moderate biomass observations, no clear relationships with mixed layer depth emerged (Fig. 6), suggesting a limited influence on production by light limitation, i.e. deep mixing was insufficient to lower light levels to limiting levels”: This cannot be concluded from the present analysis.
- p. 17430 l. 10-13 “But for the high biomass observations, there is a tendency for the highest chlorophyll concentrations to occur preferentially in shallow mixed layers, suggesting self-shading may become a limiting factor on production as biomass levels become very high (Fig. 6)": I am not sure which observations lead to this comment. Fig. 6d does not show much trend in chlorophyll concentration vs MLD.

Authors’ response:

We agree with the reviewer and we revised our interpretation of the relationship between mixed layer depth and stratification and chlorophyll contents, as described below.

Modified text (last paragraph of Section 4.2):

[…] the bio-profiler #1 profiles with integrated Chl-a greater than 600 mg m^{-2} were mainly characterized by a shallow mixed layer, lower than 60 m (Figure 7d), and a low stratification (-0.01 s^{-2} < max N^2 < 0 s^{-2}; Figure 7e). Below this Chla-a inventory threshold, no clear relationships emerged between MLD or N^2 and 0-200 m integrated chlorophyll (Figures 7d and 7e). In a steady state perspective, this lack of correlation could arise because mixed layers were shallow enough that light limitation was not sufficient to halt phytoplankton accumulation, yet not so shallow that mean mixed layer light levels allowed light promoted growth to reach accumulations that became self-shading (viewpoints that have been developed previously, based on relationships between fluorescence and mixed layer depth observations in this region using sensors on elephant seals; Blain et al., 2013). Importantly, our observations emphasize that chlorophyll distributions do not track the shoaling of mixed layer depth on seasonal or weather timescales, and thus that MLD variability is unlikely to show simple relationships to biomass accumulation. This point has also been emphasized in terms of competing effects of light and Fe limitation responses to MLD variability (Joubert et al., 2014), for waters where vertical Fe supply is dominant (rather than the horizontal dominance of supply studied here).

Fate of surface enrichment (section 4.3):

- Vertical distribution and time evolution of chlorophyll biomass (p. 17431 and Fig. 7): Fig. 7e may not be ideal to examine the temporal evolution of the vertical distribution of chlorophyll, identify subsurface maxima and characterize their origin. It is quite difficult to read the chronology of the chlorophyll profiles (despite the color code) and determine the depth of the maxima. It would be nice to have additional cross sections similar to those in Fig. 3-4a but with a zoom on profiles 150 to 250 over a shallower layer (e.g. 0-200 m). Another option would be to have a succession of chlorophyll vs. depth plots similar to, e.g., those in Perry et al. (2008) LO figure 2. It is possible that in a different graphical representation the subsurface maxima appear as relatively minor features. I also recommend plotting a cross section (or some equivalent graphic representation) of the particulate backscattering to chlorophyll fluorescence ratio. This would help to interpret possible changes in the composition of the particulate assemblage.

- p. 17431 l. 4-5 “hlorophyll profiles show elevated surface mixed layer levels, near 1.5 ug L-1”: To me most chlorophyll profiles show values of 1 _g L-1 with only 2 profiles reaching maxima of 1.5 _g L-1.

Authors’ response:

As suggested by the reviewer, following Figure 2 of Perry et al. (2008), we modified the representation of our old Figure 7e (new Figure 9e), and we added the representation of the chlorophyll fluorescence to particulate backscattering ratio. We detailed more precisely the evolution of the 3 variables shown in Figures 9e, 9f and 9g, as written below.

Modified text:

At the start of this period (blue lines subset in Figure 9e), chlorophyll profiles showed moderate to high surface and subsurface layer levels, well above HNLC background values, with some profiles exhibiting subsurface maxima reaching up to 1.5 µg L^{-1} between 50-70 m depth and up to 1 µg L^{-1} around 120 m depth. Both the surface constant Chl-a layer and the subsurface "chlorocline" layer (by analogy to thermocline or halocline, "chlorocline" is defined here as the depth range with the highest chlorophyll concentration gradient) were thick, equal to ~ 80 m and ~50 m, respectively. The origin of the smaller and variable subsurface maxima seen in some profiles in Figure 9e is uncertain. One possibility is that they are remnants of the high surface chlorophyll biomass observed just prior to the eddy entrapment (visible in Figure 3.4c and the "bloom 2013" animation in the supplementary material), that had been carried to depth by particle settling or by subduction of the denser, saltier, and slightly cooler water associated with that high biomass. Associated b_{bp} profiles showed similar large variations with strong local maxima correlated to local Chl-a maxima (blue lines subset in Figure 9f). The strong variability of the Chl-a/b_{bp} profiles over the first 100 m suggests possible changes in the composition of the particulate assemblage (blue lines subset in Figure 9g).

During the Lagrangian eddy entrapment period, the surface mixed layer chlorophyll levels declined further from 1.5 µg L^{-1} to ~1 µg L^{-1} (Figure 3.4c and 9e). Since the constant chlorophyll surface layer shallowed progressively with time, this Chl-a decrease did not result from the possible effect of dilution by mixed layer deepening (i.e. entrainment). Furthermore, the chlorocline content decreased briefly before re-increasing progressively in its upper part, and then its deeper part. In parallel, b_{bp} and Chl-a/b_{bp} profiles became tighter and tighter (light blue to orange profiles in Figures 9f and 9g) before re-exhibiting larger variations (red profiles). These results suggest the possibility of some chlorophyll conversion to non-fluorescent material, or its removal by export to depth or by local respiration or both, throughout the eddy entrapment. They may also of course partly reflect small spatial variations in the structure of the biomass distributions.

- p. 17432 l. 27-29 "the rate of chlorophyll loss is too small (by factors of 2–3, assuming a moderately high C/Chl a ratio of 50) to explain all the oxygen decrease": Please detail the reasoning (and calculation) that led you to this conclusion. I am assuming that at some point you have to use an average organic carbon to oxygen ratio or make a guess on the oxygen demand for respiration? Also note that "assuming a moderately high C/Chla ratio of 50" should be moved, maybe at the end of the sentence, as it is currently misleading (gives the impression that the change in the chlorophyll concentration by a factor of 2-3 depends on the carbon to chlorophyll ratio).

Authors' response:

The calculation is very simple:
"decrease in Chl-a" × "C/Chl-a of biomass" × "O_2/C ratio for respiration" = "expected decrease in O_2", which yields a smaller change in O_2 than observed.
Because other readers have found this to be straightforward, we have made no modifications.

- Not being familiar with oxygen data I may have missed something. Yet it is unclear to me how identical oxygen consumption rates in layers 2 and 3 ("4 _mol m-3 d-1", p. 17433 l. 15) lead to different percent estimates of carbon sequestration ("25 percent within layer 2 and 15 percent within layer 3" p. 17433 l. 19-20). Again please detail your reasoning here.

Authors' response:

Identical oxygen consumption rates lead to different percent estimates of carbon sequestration because the thickness and the average density of each layer is different (see Figures 10b and 10c). We calculate the O_2 consumption of each layer by multiplying the O_2 consumption rate by the thickness and the average density of the layer, which leads to 4 µmol kg^{-1} × 35 m × 26.70 kg m^{-3}= 3738 µmol

m^{-2} for layer 2 and 4 µmol kg^{-1} × 25 m × 26.86 kg m^{-3}= 2686 µmol m^{-2} for layer 3. O_2 consumption in layer 1 is equal to 5 µmol kg^{-1} × 80 m ×26.46 kg m^{-3}= 10584 µmol m^{-2}. Among the 3 layers, the total mean consumption equals ~17000 µmol m^{-2}. So the oxygen consumption of layer 2 equals 22% of the total consumption of the 3 layers and layer 3 equals 16%.

Modified text:

Comparing O_2 consumption of layers 2 and 3 (by multiplying the O_2 consumption rate by the thickness and the average density of the layer) relative to the total mean consumption among the three layers, we estimate that 40% of the CO_2 produced during this autumn period of bloom decline was exported (20% within layer 2 and 15% within layer 3).

- Importantly, I don't think you can call "sequestration" a process that occurs above the mesopelagic zone. Carbon "export" would be more appropriate.

Authors' response:

We agree and did the suggested correction everywhere it was needed.

Minor corrections and typos

- p. 17416 Introduction: I recommend the authors use the term "primary production" instead of "productivity" which is not appropriate in this context.

Authors' response:

We corrected as suggested.

- p. 17416 l. 19 "C": Please define symbol on first use. Although not essential it would not hurt to define CO2 and Fe as well (or to write "iron" in full letters).

Authors' response:

We defined the different symbols.

-p. 17417 l. 1: What do you mean exactly by "mosaic of blooms"? Patchiness?

Authors' response:

Yes, we refer to the "patchiness pattern" of the surface chlorophyll distribution. We added it in the text, to be clearer.

- p. 17417 l. 19: Regarding the deployment of floats 2, 3 and 4, please replace "in January 2014" by "between late January and early February 2014" as indicated in Table 1.

Authors' response:

We modified as suggested.

- The introduction (p. 17417-17418) provides significant background to objective 2. Yet objectives 1 and 3 are not introduced at all.

Author's response:

We thank the reviewer for encouraging us to expand on the importance of these 3 issues. We added new paragraphs dedicated to both question 1 and question 3.

Modified text:

Assessing influences on productivity, biomass accumulation, carbon export, and carbon dioxide (CO2) uptake in the Southern Ocean is challenging because of variations across many scales, including weather, seasonal, and interannual time-scales, and sub-mesoscale, mesoscale, and circumpolar frontal space scales (Joubert et al., 2014; Le Quéré et al., 2010; Lenton et al., 2013; Levy, 2003; Nicol et al., 2000; Shadwick et al., 2015; Sokolov and Rintoul, 2007; Swart et al., 2014; Thomalla et al., 2011; Weeding and Trull, 2014). Satellite observations offer extensive space-time

coverage [Martinez et al., 2009; Moore and Abbott, 2000], but may provide a biased view if surface distributions are not representative of water column inventories. Important ways bias could arise include correlations of surface values with their vertical extents (e.g. high surface chlorophyll values might be predominantly associated with shallow accumulations, through the promotion of production by higher light levels in shallow mixed layers; Sverdrup, 1953), the presence of unobserved subsurface chlorophyll maxima (Carranza et al., 2014; Schlitzer, 2002), or the variation of phytoplankton to chlorophyll ratios with growth conditions (Cloern et al., 1995; Fennel and Boss, 2003; Goericke and Montoya, 1998).

These difficulties of observation become even more acute for carbon export estimates, which require either flux measurements (e.g. from moored or free-drifting sediment traps or radionuclide activities; Planchon et al., 2014; Savoye et al., 2008) or the partitioning of changes in state variables across biogeochemical versus oceanographic causes (e.g. nitrate depletions in surface waters or oxygen consumption at mesopelagic depth; Matear et al., 2000; Trull et al., 2014). Obtaining estimates of carbon export and the depth of its penetration into the ocean interior are important to determining impacts on the climate system, because variations in these two factors have similar influence to variations in total primary production in terms of the sequestration of CO_2 from the atmosphere (Boyd and Trull, 2007). Notably, export estimates expressed as 'e-ratio' fractions of primary production (Maiti et al., 2013), or as 'f-ratio' fractions of production derived from 'new' nitrate supply (Savoye et al., 2004) vary widely in the Southern Ocean, with the possibility that these efficiencies are increased by natural iron fertilisation (Jouandet et al., 2011; Trull et al., 2008).

- p. 17420 l. 5-7 "As discussed in the Results section below, the bio-profilers…, but what is their level of fidelity": This type of comment is very unnecessary here. Please go straight to the methodology.

Authors' response:

We removed these sentences and start directly with the methodology.

- p. 17421 l.26 "we applied the efficient method of…": Please remove "efficient". It is inappropriate unless fully supported by statistics.

Authors' response:

We removed it.

- p. 17423 l. 22-23 "the drifts of the bio-profilers provided coverage… covering territories": Awkward phrasing. Please reword.

Authors' response:

We deleted "covering territories".

- p. 17424 l. 27-28 "breadth of spatial coverage of the plume did not extend to full temporal seasonal coverage": I don't understand what you mean here.

Authors' response:

We removed it.

- p. 17424 l. 6 "biomass accumulation": Please avoid the systematic use of the word "accumulation" throughout the text. I think that "biomass" is enough in the present context.

Authors' response:

We used the term "accumulation" because we are investigating the biomass evolution along the bio-profiler trajectory and because the chlorophyll concentration we estimate is a net budget between biomass growth and biomass loss.

- p. 17426 l. 5 "as the high chlorophyll levels decreased": Delete "high" or reformulate.

Authors' response:

We deleted "high".

- p. 17428 title of section 4.1: Please be more accurate, “total inventories” does not mean much (say, e.g., “water-column integrated content” or something equivalent).

Authors’ response:

We replaced “total inventories” by “water column contents”.

- p. 17428 l. 13 “near to the plateau”: Please remove “to”.

Authors’ response:

We removed “to”.

- p. 17429 l. 6 “contribute insignificant bias”: Please reword.

Authors’ response:

This was revised.

- Table 1: Caption should be relatively self explicit. Please explain what “Hull” and “WMO” stand for.

Authors’ response:

We specified the meaning of the terms “Hull” and “WMO”.

- Table 2 could be simplified. Some information is unnecessary. There are also two rows with identical labels and different numbers: “Day time profiles with subsurface maxima before correction”?

Authors’ response:

We kept all the information because the reviewer did not detail what was considered unnecessary.

The identical labels were a mistake. We corrected the second row label by “Day time profiles with subsurface maxima after correction”.

- Fig. 1: In figure caption specify what kind of satellite image you have used (sensor, product level and temporal averaging).

Authors’ response:

We added these specifications in the caption.

- Fig. 2: Could be split into two different figures (for drifting and quenching correction). Thus you could add letter (a, b, c etc.) for convenient reference to each individual panel of the figures.

Authors’ response:

As we already have a large number of figures, we chose to not split Figure 2 into different figures. But as we lightened Figure 2b, we believe that the titles and axes of the different plots are sufficiently explicit to find easily the plot we are referring to in the text.

- Fig. 3: I recommend splitting Figs. 3a, 3b, 3c and 3d into four different figures. This would facilitate referring to the different panels (e.g., references such as Fig. 3-a1 are not so convenient). Why using a symmetric color scale for temperature, salinity and oxygen properties (i.e. max and min values have similar colors)? For Fig. 3-1 I wouldn’t show the data that are not used due to sensor drift and stop the graph at profile 300. For all panels of Fig. 3 it would be nice to focus only on the first 200 or 250 m of the water column so small features are more visible (at least for the biological variables). Please say something about salinity units, e.g. “no unit” or “psu”.

Authors’ response:

Similarly, we chose to not split this figure in 4 different figures.
We chose a more relevant and appropriate color scale for the properties.
We think that it is still interesting to show all the data acquired by the bio-profiler #1, even after profile #300, as well as to show the larger depth range documented by most of the bio-profiler profiles, because this is the only figure where the whole data set is shown, and these data may be

useful for other studies.
Until recently the recommendation of the SCOR working group on salinity is that salinity be unitless, as the measurement is now based on conductivity and is not precisely related to the mass of dissolved material. We specified that it was unitless in the text and in the captions of the figures.

- Fig. 4: The figure caption gives the impression that the titles a) and b) are for the right-column plots only. My understanding is that a) is for the top plots whereas b) is for the bottom plots.

Authors' response:

We improved the clarity between the caption, the figure and the text by labelling each subplot, from a) to f).

- Fig. 5: The figure caption indicates "water column integrated (0-200 m) biomass" but the units (_g L-1) and the text (p. 17430 l. 1) suggest it is mean biomass (instead of integrated biomass). The word "distributions" is not necessary here. "Left column: fluorescence phytoplankton biomass estimates. Right column: backscatter total biomass estimates": both expressions are incorrect and inconsistent with the yaxis labels ("Mean 0-200 m chlorophyll" and "Mean 0-200 m backscatter").

Authors' response:

This figure has been revised as we modified the approach to answer to the question "Do the satellite images of surface chlorophyll reflect total water column contents?". In the revised version, the caption consistently refers to the parameters represented in the figure and discussed in the text.

- Fig. 6: The caption "Chlorophyll relationships with surface water properties" sounds a bit odd. I suggest replacing by, for example, "Relationship between chlorophyll a concentration and various properties in surface waters: (a) temperature, (b) salinity etc." Why have you labeled only the left-column profiles?

Authors' response:

We replaced the caption "Chlorophyll relationships with water properties" by "Relationship between 0-200 m integrated chlorophyll a concentration and various water properties…" and labeled all the subplots, for the sake of clarity.

- Fig. 7: In caption "coloured by time" is probably not what you mean. Maybe "with color code indicating the date of acquisition" would be more appropriate. "relative to profile 177 (red square)": Please recall what the red square is/where it is.

Authors' response:

We clarified "coloured by time" by "with the colour of the points changing, from blue to red over time, from profile 150 to profile 240". We added some details to better explain the signification of the red square.

Modified text:

b) Overlay of bio-profiler trajectory (white line) and eddy retention indices, showing the portion of the trajectory within a long-lasting (more than 30 days) retentive structure. The red square marks the temporal reference (profile 177) from which the Lagrangian trajectories were computed for the retention statistic, as described in Methods section 2.3.

- Fig. 8: The figure caption says "Temporal evolution during eddy entrapment for bio- profiles 4": Temporal evolution of what?

Authors' response:

We clarified the caption: "Temporal evolution of physical and biological properties during the eddy entrapment".

- I haven't found any reference to the online supplementary material in the text…

Authors' response:

We added references, mainly in Section 3.2 (Overview of observed oceanographic properties), to the animations constituting the supplementary material.

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