*We would like to thank Dr Sandy Thomalla for the time she granted to this review. We acknowledge her in-depth reading and really appreciated her valuable comments and suggestions to improve the quality of our manuscript. We carefully considered the provided feedbacks and criticisms 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.*

## Reviewer #1 (Dr Sandy Thomalla)

## General Comments

This paper uses an extensive data set from 4 high resolution bio-profiler deployments off the Kerguelen plateau covering 6 months of the growing season (Nov to April) spread over 3 years (2011 /12 and 2014). The study investigates three primary questions each with scientific relevance to Southern Ocean research within the scope of Biogeosciences. The research is encouraging for the expanded use of autonomous platforms and is well presented and articulated. However, in my opinion, there are a number of issues with the scientific results and their interpretation (plus numerous technical corrections) that need to be addressed before this work is suitable for publication.

## Specific comments

### Abstract

You say that your study examines the conditions favoring phytoplankton accumulation (better to use the word growth) in particular the influences in temperature and stratification. Your results and discussion however never investigate the influence of stratification but instead correlate temp, salinity and mixed layer depth with chlorophyll. Best to correct this accordingly, but even better would be to include stratification index into the correlation section.

Authors' response:

*We have kept the term "accumulation" because what was measured was the net budget as reflected by biomass, i.e = growth – loss, and thus we do not have a direct determination of growth.*

*The reviewer is correct that our analysis focused on the mixed layer depth, rather than a more direct measure of stratification. We have added a plot in new Figure 7 (old Figure 6) showing the maximum Brunt-Väisälä frequency squared $N^2$ as a function of chlorophyll inventories. We also added the temporal evolution of this variable in new Figure 10 (old Figure 8), in Section 4.3, for the 3 density layers. Conclusions are that no clear relationship emerged between chlorophyll concentrations and stratification, suggesting that it was not a strong driver of chlorophyll distributions, at least at the time and in the area of our study. In contrast, mixed layer depth was clearly an important parameter, and accordingly, we take care to use this term rather than stratification throughout the revised paper when linking upper water column structure to biological variables.*

You say that your largely linear relationships suggest that dilution of chlorophyll by mixed layer variations (better to say deepening as shoaling will not result in dilution) plays a minor role in spatial distribution of chla. This is incorrect; dilution is not at play here. If the mixed layer deepens it will be expected to dilute the whole mixed layer chla signal that will be evidenced in both surface and deep chla concentrations. What researchers are concerned about and what you are trying to investigate is whether surface signals of chla (seen by satellites) are representative of water column integrated chla.

The big issue here being whether surface satellites are underestimating water column chlorophyll due to unobserved subsurface chlorophyll maxima (this has nothing to do with dilution). As such a linear relationship between surface and water column chla would imply that surface concentrations are representative of water column concentrations (and not that dilution is playing a minor role in distribution).

Authors' response:

*We agree that this text was over-simplified. We have removed it from the abstract, and have expanded our discussion in the text on the role of subsurface chlorophyll maxima and other variations in chlorophyll water column distributions on correlations between surface chlorophyll concentrations and chlorophyll water column inventories. To do this, we replaced the mean [0-200] m chlorophyll concentrations (mg $m^{-3}$) by the chlorophyll inventories (mg $m^{-2}$) over this same depth range (integrated values). We also replaced the mean [0-50] m chlorophyll concentration by the surface (shallowest observation near 10 m depth) chlorophyll concentration. This issue is discussed further below, in our response to Dr Thomalla's comments about Section 4.1.*

I would add your major result from question three of your discussion to the abstract that 40% of surface production was exported.

Authors' response:

*We have done this, using our refined estimate of 35%.*

I feel it is jumping to conclusion to say that a lack of correlation between regions of moderate chlorophyll and MLD suggests a diversity of sources of Fe and or its efficient dispersion across filaments of the plume.

Authors' response:

*We agree and we have removed this statement.*

1. Introduction

The introduction does a good job of introducing the multiple drivers of production in the Southern Ocean and the resultant complexity of chlorophyll distribution evidenced from satellite. As such it introduces the scientific relevance of question 2 well (do regions of high biomass correlate with oceanographic properties) however it does not do a satisfactory job of introducing the scientific relevance of research questions 1 and 3.

Satellite ocean colour products provide high resolution space and time data of the oceans but is limited to surface estimates which do not take into account deep chlorophyll maxima or changes in carbon to chlorophyll ratios with depth. This limits the ability to interpret these products, in particular in regions where surface measurements are not representative of the water column. Your research directly addresses this issue in the Kerguellen region of the Southern Ocean and its importance should be better introduced to support question 1 (do satellite images of chl provide an unbiased guide to spatial distribution of total water column chlorophyll).

Similarly, the percentage of primary production sequestered as organic carbon in the Southern Oceans is of great importance to better understanding the efficiency of the biological carbon pump. I think that this needs to be better articulated in the introduction to highlight the relevance of your addressing question 3 (can the fate of surface biomass be determined). I also think that a primary part of question 3 should include not only 'can it be measured' but should also include "and if so what is it"?

I think that somewhere in your introduction it would also be good to highlight the importance of measuring the system at high spatial and temporal scales (which the bioprofilers are able to do thus enhancing their appropriateness as an effective research platform to address these questions in the Southern ocean) as these fine scales are what link the physical drivers of climate change to the biogeochemistry. These include the temporal scales extending from seasonal to sub-seasonal and the spatial scale ranging from the mesoscale to the sub-mesoscale. (see Lévy et al., 2001; Le Quéré et al., 2007; Klein et al., 2008; Doney et al., 2009; Thomalla et al., 2011; Racault et al., 2012; Joubert et al., 2014; Swart et al., 2014; Carranza and Gille, in press).

Authors' 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, and included quantification of the export as an*

*important aspect of question 3. Our expanded text now refers to many of these suggested citations, as well as to others that illustrate the issues.*

Modified text:

*Assessing influences on productivity, biomass accumulation, carbon export, and carbon dioxide ($CO_2$) 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 that 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., 2015). 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).*

2. Methods

The bbfl2 measures total scattering in raw digital counts and not particle scattering. You need to include in your methods how raw digital counts were converted to particulate.

Authors' response:

*In the method section 2.2, we added a paragraph explaining the retrieval of particulate backscattering from the raw measurements.*

Modified text:

*[…]the retrieval of particulate backscattering, $b_{bp}$ ($m^{-1}$), at 700 nm from the backscatter raw transmitted measurement (counts) was done by applying the manufacturer-provided scaling factor after correction for dark counts (i.e. measured signal output of the backscatterometer in clean water with black tape over the detector), with the additional steps of removal of the pure seawater backscattering contribution (Zhang et al., 2009), and scaling from the limited solid angle sensor measurement to the total backscattered hemisphere based on relations estimated from observations for a wide range of marine particles (Boss and Pegau, 2001; Sullivan et al., 2012).*

Whenever you use backscattering you need to be careful to say particulate backscattering ($b_{bp}$) or simply use $b_{bp}$ but total scattering, backscattering and particulate backscattering are all different measurements and not interchangeable and you need to be sure to always use the right one throughout the text.

Authors' response:

*We did the corrections accordingly throughout the text.*

I am not familiar with uranine solutions and fluorescent and non reflective plastic covers for calibrating

fluorescence sensors. Could you please provide more information on calibrating the instrumentation and how raw fluorescence units were converted to chlorophyll concentrations (so that the methods can be reproduced by future researchers). Were no in situ chla samples collected at the deployment site? Was the instrument factory calibration factor applied to raw fluorescence to convert to chl. Was a dark correction applied to the fluorescence data (i.e. both the factory dark count but also a measured dark count from black plastic covers or from an average of all very deep "dark" data (see swart et al., 2014)).

Authors' response:

*Similarly to the particulate backscattering, we added a sentence to explain the retrieval of chlorophyll concentration from the raw fluorescence measurement in section 2.2. We also gave more information about the calibration of the fluorometer.*

Modified text:

*The bio-optical fluorescence sensors were calibrated (by the manufacturer, Wetlabs, Inc.) against fluorescent uranine solutions as working standards, and cross- referenced to prior measurements of a laboratory culture (25 mg $m^{-3}$ chlorophyll) of the diatom Thallassiosira weissflogii to yield chlorophyll estimates. These calibrations are warranted to yield linear responses with precisions among multiple sensors of better than 10%, and (after one cycle of testing and replacement with the manufacturer) we obtained reproducibility for the set of three floats deployed in 2014 of better than 4% based on measurements with fluorescent and non-reflective plastics (Earp et al., 2011). Accordingly, calculation of the chlorophyll fluorescence from the float data was done by removal of the background dark signals measured prior to deployment and scaling to chlorophyll using the manufacturer's calibrations.*

Authors' response:

*Finally, we added Figure 2c showing that bio-profiler #1 fluorescence profiles compared well to KEOPS2 shipboard observations and associated in-situ chlorophyll data.*

Modified text:

*Bioprofiler #1 was deployed into a semi-permanent meander of the Polar Front, which the KEOP2 program examined as a Lagrangian time series following surface drifters. As shown in Figure 2c, the first and second stations in the meander (E1 CTD-27 on 29 October 2011 at 22:46 local time and E2 CTD-43 on 1 November 2011 at 12:00 local time) bracketed the locations of the first 11 autonomous bio-profiler #1 profiles (Figure 2c.i). The bio-profiler #1 temperature profiles are intermediate between the ship results (Figure 2c.ii), with the variations in temperature profiles mainly driven by vertical motions associated with internal waves (Park et al., 2014b). In Figure 2c.iii, the KEOPS2 shipboard fluorescence results are displayed after linear calibration to high pressure liquid chromatography (HPLC) total chlorophyll-a results from below 40 meters depth (below the depth of non-photochemical quenching). The data reveal two important features: i) good fits achieved below 40 meters do not extend to the surface – where fluorescence/chlorophyll-a ratios were higher than at depth, apparently as a result of community composition variations with depth (see also Lasbleiz et al. 2015), and ii) the bio-profiler #1 fluorescence data displayed similar characteristics and good accord with the shipboard results. In light of the limited available data, a non-linear calibration of fluorescence to chlorophyll-a was not pursued, and no adjustments were made to the laboratory bio-profiler calibration.*

*These variations in fluorescence/chlorophyll-a ratios within individual CTD casts in the shipboard observations serve as a strong reminder that fluorescence is an imperfect proxy for chlorophyll-a concentrations, owing to variations with phytoplankton community structure, physiology, and other effects (e.g. Babin et al., 1996; Cullen, 1982; Suggett et al., 2011). Thus, interpretation of our sensor records, as with any bio-optical sensor results, must keep this in mind and avoid over-interpreting small variations in fluorescence as necessarily resulting from variations in chlorophyll or phytoplankton biomass.*

Your method states no significant temporal drift was observed in the deep values. My understanding is that you cannot use the word significant without doing actual statistically significant tests on the dataset. I think to do some statistical significance tests of variability would add to the confidence in the absence of sensor drift.

Authors' response:

*We agree and we consequently added a new table, Table 2, which summarizes, 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. 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 discussed Table 2 results in the third paragraph of section 2.2.*

Modified text:

*To evaluate the possibility of temporal drifts in bio-optical sensor responses, we examined the variations of the bio-optical variables in mesopelagic (250-300 m) and deep water (950-1000 m) values, i.e. at depths where little signal was anticipated and most profiles reached steady background values (Figure 2a). The particulate backscattering and, to a lesser extent, the Chl-a fluorescence signals showed spikes which presumably reflect larger particles such as aggregates and zooplankton, motivating our examination of average values over 50 m ranges (250-300 m and 950-1000 m depth layers) for the assessment of temporal drifts. As shown in Figure 2a and quantified in Table 2, for most of their deployment periods all four bio-profilers exhibited no significant temporal drift of these deep values except for bio-profiler #1, for which high and erratic values of Chl-a and $b_{bp}$ began to occur after profile #300 both at depth (Figure 2a) and throughout the water column (Figure 3.1c and e). We consider this to be caused by bio-fouling and do not use this data in any subsequent analysis (this loss of signal fidelity was one of the motivations for including periodic deep profiles in the subsequent three bio-profiler deployments, as a means of retarding fouling). In contrast, the high fluorescence chlorophyll values found in mesopelagic waters from profiles ~#100 to ~#170 along the bio-profiler #1 trajectory appear to be real and to reflect the deep extension of high biomass occurrence at this time, as discussed further below (see also Figure 3.1c). Consequently, this range of profile was not taken into account for the drift calculation in Table 2. Overall, except for the bio-profiler #1, most of the bio-optical sensors showed a slight loss of sensitivity with time, as indicated by the negative slopes of the trend of their responses in the two considered depth layers (Table 2). Over the time course of the bio-optical sensor observations, these sensor drifts were small in comparison to the changes observed for surface bio-optical values, contributing less than 7% to either fluorescence or particulate backscattering. The only exception was the drift for the bio-profiler #2 $b_{bp}$ sensor in the 250-300 m layer, where drift appeared to have been larger (though of course changes at this depth range may also be oceanic) and reached up to 19 % of the low surface $b_{bp}$ values for this bio-profiler.*

I think that one sample every 10m is a really problematic bio-optical resolution of the upper water column. I realize that there is naught to be done about it now but I highly recommend that future studies using bio-profilers do not compromise the value of the data set by under sampling the vertical resolution. I think that the implications of the poor sampling resolution of the water column with regards to errors and erroneous measurements needs to be better addressed and discussed further.

On a similar note, I feel a little uncomfortable with the methods of removing bad profiles and the lack of removal of spikes. I appreciate that with such a low 10m vertical resolution data set it was not advisable to apply a box filter to each profile. I think that the method of removing profiles with high mean values in the deeper depth ranges was a really good idea. But this would potentially leave profiles in the data set that had erroneous (not real) spikes in the surface layers. I am not entirely sure what to suggest here but I wonder if one could search for profiles whose mean value in surface depth ranges was above a certain percentile and remove those profiles or at least remove the spikes in those profiles that appeared unrealistic? The problem is that I think a lot of your measured sub surface chlorophyll maxima are not real ecological maxima but merely the result of a spikey data set from instruments that have a high error associated with them (not to mention chlorophyll that is quenching corrected with an even spikier backscattering data set).

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, the 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 were 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 $SD/MEAN_{500\text{-}1000}$ 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 coefficients of variation $SD/MEAN_{Chl\text{-}a(250\text{-}300)} = 22 \pm 10$ % and $SD/MEAN_{Chl\text{-}a(500\text{-}1000)} = 21 \pm 7$ % and the average backscatterometer coefficient of variation $SD/MEAN_{bbp(500\text{-}1000)} = 14 \pm 4$ %. Considering these results, we decided to update the threshold we defined to characterize chlorophyll concentration subsurface maxima, as mentioned below in our first response of section 4.1. We also used the $b_{bp}$ coefficient of variation to less arbitrarily define a threshold $b_{bp}$ surface value above which surface $b_{bp}$ was considered potentially spiked and was not used for the quenching correction (see details just below).*

Modified text

*6th paragraph in Section 2.2:*

*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.*

*7th paragraph in Section 2.2:*

*Without the [quenching] correction, on average, more than 70% of the daytime profiles exhibited a subsurface maximum exceeding 60% of the surface value –defined after assessing the fluorometer error (coefficient of variation of standard deviation/mean ratio of the Chl-a concentration) between 250 and 300 m depth and between 500 and 1000 m depth of 22 ± 10% in average.*

Your method of calculating MLD used a density criteria of 0.2 kg m-3 relative to the density at 10m (according to Park et al., 1998). I am more familiar with the more recent method of de Boyer Montegut et al. 2004 which uses a density criteria of 10 orders of magnitude less 0.03 kg m-3 (and a temperature difference of 0.2 oC). I wonder if this was simply a typo type mistake. If so then I would suggest correcting it and adding the more recent de Boyer Montegut reference. If not, then I think a discussion on why the Park method was chosen and the implications of the different (deeper) MLD it would generate.

Authors' response:

*Yes, it is a mistake that appeared in the proof stage. We corrected the typo type mistake to 0.02 kg $m^{-3}$. We chose to use the mixed layer depth definition of Park et al. (1998) rather than of de Boyer Montégut et al. (2004) to be more consistent with the other studies of the special issue. However, for each bio-profiler, we compared the evolution of the MLD as we defined it with the evolution of the MLD defined following de Boyer Montégut et al. (2004; depth where density increased by 0.03 kg $m^{-3}$ relative to the density at 10 m) and we observed really similar spatial/temporal variations and amplitudes. In Section 4.1, 6th paragraph, we also compared our results with those obtained using a much larger density criterion (Levitus, 1982; seasonal mixed layer = depth where density increased by 0.125 kg $m^{-3}$ relative to the density at 0 m) and found quite consistent results, as described and discussed.*

I don't think it is necessary to show so many examples of the quenching correction. I would suggest having just three images: without quenching, with quenching and backscattering (not four sets of three images). Then on each image have four pro-files one good example from each bio-profiler.

Authors' response:

*We agree and we revised the figure accordingly.*

3. Results

3.1. Coverage of the plume: I really don't see the point of figures 1 d, e and f of the 2014 float trajectories overlaid on 2013 satellite data (the floats were not there at the time so seems odd to overlay a trajectory onto data that was not coincident. I think that it would be better to delete these images. I also think that perhaps a better way to display the coverage of the plume would be to create one composite image of November 2011 to April 2012 (covering the entire time period of the deployment of bio-profiler #1) with the float track overlaid, and one composite image from February 2014 to April 2014 (covering the time period of the deployment of bio-profiler #2, 3 and 4) with the tracks overlaid in different colours (per float) or you could do the different colour dots with time as in fig 3). I also suggest adding the position of the polar front onto the images. If you want to highlight where in the seasonal cycle the floats sampled I would perhaps average the ocean colour in each box to create an annual time series (june to june one for 2011/2012 and one for 2013/2014) with the time periods that the time series was sampled indicated on each graph (this suggestion not necessary to carry out if the authors prefer not to, but may be required if the authors wish to retain the text on seasonal coverage of the bloom).

Authors' response:

*Our motivation explaining our choice to show figures d, e and f is that these figures 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. We think that this is important and have kept these figures. We added a couple of sentences to better explain this aspect.*
*We added the location of the Polar Front as defined in Park et al. (2014), determined from a large dataset of hydrographical data.*
*Concerning the suggestion for construction of an annual time series, we disagree and prefer to keep showing short multiple images to illustrate synoptic features rather than seasonal ones, because we wish to not smooth away the important mesoscale patterns.*

Modified text:

*As shown in these images [Figure 1], the 2011 bio-profiler covered the period of highest biomass accumulation, while the 2014 deployments occurred after this seasonal peak, and thus sampled the system during its senescence (to illustrate these prior conditions, Figure 1 also includes biomass distribution images from late 2013, before the launch of the three bio-profilers in early 2014).*

3.2. Overview of observed oceanographic properties: Please plot the quenching corrected chlorophyll in all sections (not the quenched chlorophyll). The whole way through this section, when you say colder, fresher, more oxygenated etc. please include the values in parenthesis next to each. E.g. colder (<3oC). Also when you describe specific times in the sampling of the float e.g. when it crossed a certain longitude or the PF please always include the approximate profile numbering so one can easily locate the event on the trajectory sections. Please add the PF to all figures a)

Authors' response:

*We plotted the quenching corrected chlorophyll and specified the physical values or ranges or the profile numbers to our description throughout this section.*

*We did not add the Polar Front on each Figure 3.1a, 3.2a, 3.3a and 3.4a because it decreased the clarity of the figures and was not necessary considering that we added the Polar Front location in Figure 1.*

4. Discussion

4.1. Do satellite images of surface chlorophyll reflect total inventories?

I think that this is a really important question that needs addressing but I have some major issues with the approach chosen to do so. Firstly, I think your choice of subsurface chlorophyll that is 30% > than surface chlorophyll to define the presence of a subsurface chlorophyll maxima is within the error of the instrument and as such too small a percentage to be able to say with any certainty that there is a "real" subsurface chlorophyll maxima or not. I would increase this to at least 50% and find a reference about the high errors of chlorophyll measurements using fluorometers particularly in regions of relatively low chlorophyll concentrations. Furthermore there needs to be some kind of investigation to determine how many of these sub surface maxima are simply the result of a noisy spikey data set. Finally, there needs to be an investigation of how any of these

incidences of sub surface chlorophyll maxima coincide with changes in physical water column characteristics. For example how many of the sub surface chlorophyll maxima relate to a changes in density, stratification, MLD etc. This I think will help you to determine the occurrence of real chlorophyll maxima that are representative of ecological phenomena (e.g.: phytoplankton living at preferred depths to promote growth through accessing limiting nutrients, mixing, subduction etc.).

Authors' response:

*We initially chose this 30% threshold to be consistent with the study of Guinet et al. (2012). However, we agree with the reviewer's concern. Therefore, using the coefficient of variation of chlorophyll concentration between 250 m and 300 m depth of 22 ± 10% in average (that we detailed above), for the 4 bio-profilers, we decided to define the "real subsurface maxima" threshold as twice as large as this value and increased it to 60%.*

*We also added a plot in Figure 4 showing the MLD as a function of the subsurface Chl-a maxima, which revealed a more frequent occurrence of elevated subsurface maxima (for 2.5 µg $L^{-1}$ ≤ Chl-a ≤ 5 µg $L^{-1}$) when the mixed layer was deep, for bio-profilers #3 and #4. This would suggest that, in these cases, light limitation may not be a major driver of Chl-a vertical profiles. Contrastingly, the quasi-ubiquitous concomitance of subsurface Chl-a maxima for bio-profiler #1 with shallow mixed layer, lower than 50 m, may suggest that above a certain threshold of Chl-a content, self-shading sets up and limits the phytoplankton development in depth.*

Very importantly, I think your interpretation of the occurrence of subsurface chlorophyll maxima on 46%, 14%, 45% and 41% (average 37%) of the profiles as being "rare" is very misleading. My interpretation of something occurring ~40% of the time would in fact be rather common. Hopefully taking into account my suggestions above, the occurrence of sub surface chla maxima will be reduced. Nonetheless this still needs to be properly addressed in the text to avoid misrepresentation.

Authors' response:

*Changing the threshold from 30% to 60% 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.*

Secondly, you go on to show that even when there is an occurrence of a subsurface chlorophyll maxima (i.e. when satellite measurements will tend to underestimate total inventories) the effect is minor. I agree that it is important to investigate the implications of such underestimations but I do not agree with how you have gone about it. The effect of a subsurface chlorophyll maxima on measurements that only see the surface is that surface measurements will always be biased towards an underestimate of total inventories and as such will misrepresent spatial distribution of total water column chlorophyll. Your relationship of mean surface (1-50m) chlorophyll to mean water column (0-200m) will on the other hand always tend to have higher mean surface values than mean water column values (even in the presence of sub surface chlorophyll maxima). This is because your water column values are averaged over 200m when chlorophyll often only extends to the base of the mixed layer e.g. ~60m. Your water column mean chl is thus averaging over ~140 m of no chlorophyll and reducing the mean value of water column chlorophyll substantially. I don't think the relationship in figure 5 is able to tell you anything substantial about the effect of subsurface maxima on surface measurements. However, I do understand what it is that you are trying to achieve. My suggestion would be the following:

1: Instead of classifying surface measurements as a static average of the top 50 m (2 e-folding depths for satellite observations) I would suggest that you use a dynamic depth range based on the optical depth, which is relative to the surface chlorophyll concentration. Thus there will be a different optical depth for each profile and the 'surface' chlorophyll can be calculated as the mean of chlorophyll from the surface to the optical depth per profile. This way you are generating a dynamic estimate of the surface chlorophyll which equates to what the satellite would see. Now you want to compare this satellite surface chlorophyll value to a value that is representative of the water column. For reasons mentioned above, I don't believe that the mean from the surface to 200m is an accurate representative of the water column (too much inclusion of no chlorophyll depths in your averaging). Instead I would suggest 2 methods of generating a chlorophyll value representing the water column. The first would be a dynamic chlorophyll value calculated per profile averaged from the surface to the Euphotic depth (0.1% and / or 1% surface light depth) this can be calculated from surface chlorophyll concentrations based on a kd attenuation coefficient (see also Morel, 1988 for empirical model). If the Ed is much deeper than

the MLD and much deeper than the high chlorophyll surface waters then this may result in similar issues as averaging 0 to 200m. I think you can get around this by comparing integrated values instead of surface and mean. i.e. for surface values multiply the surface chlorophyll concentration by the Ed and for water column do a trapezoid integration of the chlorophyll profile to the Ed.

The second method would be a mean calculated from the surface to the MLD. This method would however exclude any potential sub surface maxima that were below the MLD. In which case I think it would be a good idea to calculate on how many occasions and for which profiles the sub surface maxima was below the MLD. On these occasions it may be necessary to look at the individual profiles and decide on a depth based on the MLD + a threshold (e.g. 20m) to ensure that all viable chlorophyll deeper than the MLD (but still in the Ed) was included in the number representing the water column. I would also do this for surface and mean as well as MLD integrated values as described above). The comparative results between these estimates of surface and water column chlorophyll will I think do a better job of investigating the effect of sub surface maxima on satellite surface estimates off Kerguellen.

Authors' response:

*We greatly acknowledge Dr Thomalla for these valuable suggestions and we have spent considerable effort refining our analysis in this section of the paper, including:*

1. *Choosing to use only the shallowest measure of chlorophyll to represent a surface value as would be seen by a satellite sensor, based on the following reasoning.*

*We don't have PAR measurements, but we however tried to estimate the euphotic depth $z_{eu}$ from the model of Morel and Maritorena (2001) to estimate the first penetration depth $z_{pd} = z_{eu}/4.6$ (Gordon and McCluney, 1975). From Figure 6 of Morel and Maritorena (2001), considering that in our study, for the total data set, surface chlorophyll concentrations do not go lower than 0.4 mg $m^{-3}$ and 0-200 m integrated chlorophyll inventories do not go lower than 40-50 mg $m^{-2}$ (Figure 5a; excluding flagged profiles), $z_{eu}$ should not exceed much more than 50 m. Therefore, an upper estimation of $z_{pd}$, max($z_{pd}$), would be max($z_{eu}$)/4.6 ~ 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. Thus we replaced the mean 0-50 m chlorophyll concentration by the surface chlorophyll concentration in the revised Figure 5.*

2. *Replacing our previous use of an average chlorophyll concentration for the top 0-200 m by the inventory over this depth range that includes all phytoplankton biomass, following the advice of the reviewer.*
3. *Exploring the correlation between these surface chlorophyll concentrations and water column chlorophyll inventories. This new analysis shows that deep chlorophyll maxima are only one source of variance in the correlation of these metrics, with another important (indeed a more dominant) source being varying depths of chlorophyll distributions (without subsurface maxima). We further advanced this discussion by adding a new Figure 6, which compares the 0-200 m inventory to what would be estimated by extending the surface chlorophyll concentration to the depth of the physical mixed layer (i.e. the product of surface chlorophyll × mixed layer depth (MLD)). This shows that much of the chlorophyll occurs below the mixed layer (though without a subsurface maxima), and that if this is not recognized surface chlorophyll concentrations can strongly underestimate water column inventories. Accordingly, the text of this section has been almost completely rewritten.*

You discuss that your results show that satellite images tend to overestimate the dynamic range of total chlorophyll inventories (which is the opposite of what one would expect from both subsurface chla maxima and physiological poc:chl adjustments with decreasing light depth). You go on to say that this effect is relatively small, less than a factor of 2. Again, this may be subjective but my opinion of an error in measurement that doubles or halves your chlorophyll concentration is rather large, in particular when the seasonal range is generally between 0.1 and 3 mg m-3 (taken from fig 1).

Authors' response:

*This analysis has been reassessed. Following the modifications we did in Figure 5 and the new Figure 6, we revised our interpretation in section 4.1 and modified our conclusions accordingly. Surface Chl-a concentrations seem to reasonably reflect the 0-200 m integrated Chl-a contents, as shown by their relatively linear*

*relationship, and the relative variance (expressed as newly calculated coefficients of variation) of surface Chl-a concentrations and 0-200 m Chl-a inventories are now presented. These data show that surface and inventory dynamic ranges are very similar, and are now quantified and presented. We refrain from making a judgement about whether the quantified relative variance values are large or small, since that depends on the problem under consideration.*

4.2. Do regions of high biomass correlate with oceanographic properties?

Again I think that this is a very relevant investigation that will help inform on the physical controls of enhanced primary production and biomass accumulation but I do not agree with the approach the authors have taken to examine this. The main problem I have is with the selection of the high and moderate chlorophyll boxes which are purely subjective with no concentration thresholds and an overlap of the concentration range of the majority of the data falling into both boxes (i.e the 0-3 ug l-1 chlorophyll range of the moderate box is the same range of concentration providing the majority of the data points in the rich biomass box) as such I don't see how the statistical analysis between moderate and rich biomass boxes can provide robust interpretation. I think that perhaps a better approach would be to use a threshold to characterize high (e.g. >3 ug l-1), low (e.g. <1 ug l-1) and moderate (e.g. 1-3 ug l-1) chlorophyll profiles. For this approach I think it would be better to use water column chlorophyll rather than surface chlorophyll (since you have depth profiles and don't have to rely on restricted surface measurements, as with satellites, I suggest you make the most of the data at hand).

Authors' response:

*We understand your concerns and we adopted your suggested approach: 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).*

In your correlation with physical properties I would include a stratification index and date (to temp, salinity and density).

Authors' response:

*We now include examination of the maximum Brunt-Väisälä frequency squared ($N^2$) to investigate the influence of the stratification. 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.*

I would also consider an additional parameter such as bathymetry or distance from shelf in an attempt to determine the regional proximity of the float as a driver of high chlorophyll. This combination of correlations may assist in determining both regional and seasonal drivers of chlorophyll together with light (MLD) and potential Fe sources? (depending on the results you may want to only present the high and low correlations and leave out the moderate ones)

Authors' response:

*We considered an additional parameter to take into account the regional proximity of the float to the bathymetry as a driver of high chlorophyll. However, instead of considering the geographical distance from the bathymetry, we used a Lagrangian approach. Following d'Ovidio et al. (2014, this volume), we computed the 'water age' and 'origin'. They represent respectively how long before the bio-profiler sampling a water parcel has left the Kerguelen Plateau (defined as the 700 m isobath) and at which latitude the parcel has left the plateau. The Kerguelen Plateau is likely to be a source of iron, because the shallow bathymetry enhances iron-rich sediments resuspension. The main advantage of this approach is that it takes into account of the different pathways water parcels can take to reach a given geographical distance. For example, with this method it is possible to discriminate between water parcels that are close to the plateau, but have spent long time recirculating away from it and the ones that have recently left the shallow bathymetry and are more likely to be iron-enriched. By comparing these diagnostics with the [0-200] m integrated chlorophyll, we found (as shown in new Figure 8) that high chlorophyll data corresponded to water masses that had recently (0-40 days) left the Kerguelen Plateau, with a preference for the Northern part of the Plateau.*

When discussing the relationship between MLD and chlorophyll with respect to light limitation please refer to the paper by Joubert et al., 2014 in Biogeosciences Discussions, which presents similar results (but for NCP) and discusses the complex role of MLD in adjusting both light and Fe in driving the observed relationship between low chl when MLD is deep (light limitation) but both high and low chlorophyll when MLD's are shallow (shallow mixed layers improve light regions but can drive Fe limitation by reducing the size of the accessible reservoir).

Authors' response:

*We agree and added a sentence referring to the study of Joubert et al. (2014) at the end of Section 4.2.*

Modified text:

*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).*

It is not clear to my why the chlorophyll data is correlated with oxygen saturation. Surely oxygen saturation states are driven by the biology and not the other way around, hence an existing correlation can be expected as a result of biological production and not as a driver of enhanced production. I would personally remove the correlation with oxygen saturation from this section.

Authors' response:

*We prefer to keep this subplot. It is true that the oxygen supersaturation observed for the high biomass waters is biologically generated, but for the low biomass waters the presence of oxygen undersaturation could indicate the influence of mixing with subsurface waters. The revised text makes it clear that both issues are addressed by the figure.*

Modified text:

*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). Relatively high biomass was also encountered in waters with extreme T-S properties (the warmest and freshest observed) in the vicinity of the Gallieni Spur by bio-profiler #4 (black symbols in Figure 7). Thus, there was not a unique class of waters with high biomass. This perspective is further reinforced by the lack of any clear relationships between chlorophyll inventories and local water column properties for regions of moderate biomass, including versus mixed layer depth and the intensity of stratification as represented by the Brunt-Väisälä frequency (Figure 7, right column). These low biomass waters also exhibited lower $O_{2\,sat}$ states (95-103%) than those of rich biomass areas. The under-saturated oxygen levels reflect either strong local respiration or the supply of low oxygen waters from below, with these processes difficult to distinguish (except for specific portions of the bio-profiler #4 trajectory where time series within constant physical property layers were obtained, as discussed in section 4.3).*

4.3. Can the fate of surface enrichments in biomass be determined.

I think this heading should be refined to include: And if so what is the percentage of biological production being exported?

Authors' response:

*We modified the heading accordingly.*

I really liked this section, I thought the research presented a novel use of bio-profiling data and was able to

demonstrate a robust method of determining the amount of production sequestrated from the besurface layers.

One area I need to query is figure 7c. In the text it states that surface mixed layer chlorophyll concentrations declined from the start of the lagrangian study at 1.5 ug l-1 to < 1 ug l-1. This is not clear to me from section 3.4c nor fig 7e. In fact in figure 7e, the opposite trend in chlorophyll concentration appears to exist from my analysis of figure 7e. According to figure 7a, the coloured dots mark the time trajectory of the float starting with blue and ending in red. A cursory look at figure 7e appears to me as tough there are more blue profiles with lower surface mixed layer chlorophyll concentrations (at the beginning of the transect (<1 ug l-1) while there are more red profiles with higher mixed layer chlorophyll concentrations (>1 ug l-1) towards the end of the transect. This is opposite to the trend you describe.

Authors' response:

*As suggested by the second reviewer, to improve the clarity of Figures 9e and 9f (old Figures 7e and 7f), we divided the 90 profiles acquired over 28 days in 4 weekly plots (~23 profiles), in an similar approach to Figure 2 of Perry et al. (2008). 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.*

## Technical corrections

Figures

Figure 1: Include the following in the legend: which chlorophyll product, km resolution and period averaged. Units ug l-1 not mg/m3. Units are sometimes touching the numbers on the legend Latitude is touching the numbers on the y axis Delete figures d, e and f. Add PF to figures Refer to text above to alternative suggestions for this figure.

Authors' response:

*We modified the legend, the units and added the Polar Front location (from Park et al., 2014) as suggested. We chose to keep figures d, e and f as they 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. As mentioned before, we would prefer to keep showing short composite images to show synoptic features rather than seasonal*

*ones and avoid to smooth the patterns. Finally, we used 3 different colors to represent the trajectory of each of the 3 bio-profilers deployed in summer 2014 to improve the clarity.*

Figure 2: Refer to text above for suggested reduction of size of figure 2b.

Authors' response:

*We revised this figure accordingly to both reviewers' suggestions.*

Figure 3: Label figure 3.2, 3.2, 3.3, 3.4. Add PF to all figures 1a Plot quenching corrected chlorophyll (not quenched chlorophyll) I really to not think you can use your chosen colorbar for temperature, salinity and dissolved oxygen. I really don't think you cant have the same colours for low and high concentrations. It is confusing and does not justly represent your data. Please adjust to let the colours all scale in one direction only. Labels 1a) and 1b) are not aligned. I personally think that units of chlorophyll should be ug l-1 with a lower case l and not an upper case L. Figure 1e in legend is particulate backscattering (bbp) not backscatter.

Authors' response:

*Most of these changes have been done. We did not add the Polar Front on each Figure 3.1a, 3.2a, 3.3a and 3.4a because we believe that it was decreasing the clarity of the figure and the Polar Front location is already reported in Figure 1. We cannot label figures 3.1, 3.2 etc because it is not an allowed format in Biogeosciences. We also contoured the 700 m isobath with a red line as a support for the discussion of the water mass ages (new Figure 8). Finally, we added some important topographic features: Gallieni Spur (GS), Kerguelen Plateau (KP), Kerguelen Island (KI), and Heard Island (HI).*

Figure 4: coloured dots identifying bio-profilers need to be solid circles not open circles

Authors' response:

*We now distinguish the night, day and flagged day profiles (see the definition of these latter in the Method section and in the Figure 2b caption) by stars, open circles, and open squares, respectively. We increased the size of the symbols to improve the clarity of the figure.*

Figure 6: Rich and moderate biomass region headers need to start with uppercase. Units kg $m^{-3}$ not $kg/m^3$

Authors' response:

*We did these corrections.*

Text

17415 Line 6: refers to 'conditions that favor phytoplankton accumulation'. I think a better word would be favoring phytoplankton growth (accumulation in this context (to me) implies concentration of phytoplankton by hydrography rather than conditions favoring growth rates and increased phytoplankton biomass.

Authors' response:

*As mentioned before, we used the term "accumulation" because what was measured was the net budget, i.e = growth - loss.*

17416 Line 12: I would put (Fe) in brackets after the first time you use the word iron so that they are interchangeable.

Authors' response:

*We did the modification.*

Line 19: carbon (C)

Authors' response:

*We defined the symbol C.*

Line 20: Full stop after references. New sentence: These studies have revealed

Authors' response:

*We divide the sentence in two, as suggested.*

17417 Line 12: factors are also likely Line 21: high winds and strong currents do not preclude the use of alternative profilers (profiling gliders for example are deployed and retrieved in rough southern ocean seas). Maybe rather say that the currents and seas compromise the recovery such that bio-profilers were considered a suitable choice of platform?

Authors' response:

*We modified the sentence*

Modified text:

*"Given the extent of the Kerguelen biomass plume (> 1000 km; Mongin et al., 2009), the remoteness from ports, and the generally rough sea states, the use of autonomous platforms is arguably the only affordable way to survey this region".*

17418 Line 2: you do not measure stratification anywhere in your research. Better to include MLD here and even better to include stratification in your statistical correlation and then leave both stratification and MLD in the question.

Authors' response:

*We agree and included stratification in our statistical correlation (old Figure 6, new Figure 7).*

Line 20 (add wavelength of scattering measurement).

Authors' response:

*We added it (700 nm).*

17419 Line 19: full stop after variations. New sentence After several weeks Line 23: to be evaluated and corrected and thus to avoid

Authors' response:

*We did the corrections accordingly.*

17420 Line 13: profiling float (no s) Line 14: Temperature (T) and salinity (S) in parenthesis Line 15: suggests that Line 17: give time frame of shorter deployment in parenthesis Line 19: and require no further assessment or correction. Line 24: showing (not and showing) Line 27: To evaluate the possibility of temporal sensor drifts in bio-optical variables, we examined...

Authors' response:

*We corrected as suggested.*

17421 Line 2: delete second comma Line 7: don't use the word significant without a significance test Line 20: Flourescence signals were corrected for daytime quenching Line 23: delete second comma

Authors' response:

*We did the necessary corrections.*

17422 Line 1: This method assumes Line 2: replace not stratified with constant. Insert the method of defining MLD here the first time you mention density defined mixed layer rather than in the next sentence. Line 5/6 what do you mean by sub surface portions? Please be more specific

Authors' response:

*We did the necessary corrections. We replaced "subsurface portions" by "deeper half".*

Line 17: there is no figure 6f? delete below.

Authors' response:

*We did the necessary correction, considering the addition of the two new figures.*

17423 Line 6: delete and

Line 13: resolution (no s)

Line 16: delete the

Authors' response:

*As it concerns temporal and spatial resolutions, we think that an "s" is necessary. We did the other corrections as suggested.*

17425 Line 8: replace a huge amount with a large number. (I would delete each one provides more than most oceanographic voyages) so that the sentence reads: Profilers return a large number of water column data making visualization at the scale of individual profiles only (delete is) possible for targeted issues. (full stop delete and) new sentence. The simplest first order....

Authors' response:

*These sentences were modified as suggested.*

Line 11: delete as

17427 Line 8: persistent high Line 19: delete Then, rather As its trajectory...

Line 22: delete the words and correlated

Authors' response:

*We corrected as suggested.*

17428 Line 10: delete as. After cloud cover full stop. New sentence: Instead...

Line 23: delete the last the

Authors' response:

*Suggested corrections were done.*

17429 Line 1: averaged from the surface down to 200m (otherwise not clear if you started at 50m)

Line 20: pick either allow or achieve not both

***References cited in authors' responses***

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