

## ***Interactive comment on “Autonomous profiling float observations of the high biomass plume downstream of the Kerguelen plateau in the Southern Ocean” by M. Grenier et al.***

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### 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

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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.

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 chl<sub>a</sub>. This is incorrect; dilution is not at play here. If the mixed layer deepens it will be expected to dilute the whole mixed layer chl<sub>a</sub> signal that will be evidenced in both surface and deep chl<sub>a</sub> concentrations. What researchers are concerned about and what you are trying to investigate is whether surface signals of chl<sub>a</sub> (seen by satellites) are representative of water column integrated chl<sub>a</sub>. 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 chl<sub>a</sub> would imply that surface concentrations are representative of water column concentrations (and not that dilution is playing a minor role in distribution).

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

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

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dispersion across filaments of the plume.

## 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 Kerguelen 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 bio-profilers 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

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ranging from the mesoscale to the sub-mesoscale. (see Lévy et al., 2001; Le Quééré 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).

## 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 backscattering.

Whenever you use backscattering you need to be careful to say particulate backscattering (bbp) or simply use bbp 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.

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 chl<sub>a</sub> 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)).

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.

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

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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).

Your method of calculating MLD used a density criteria of  $0.2 \text{ kg m}^{-3}$  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  $0.03 \text{ kg m}^{-3}$  (and a temperature difference of  $0.2 \text{ }^{\circ}\text{C}$ ). 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.

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-

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files one good example from each bio-profiler.

### 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).

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 ( $<3^{\circ}\text{C}$ ). 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)

### 4. Discussion

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

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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.).

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 chl<sub>a</sub> maxima will be reduced. Nonetheless this still needs to be properly addressed in the text to avoid misrepresentation.

Secondly, you go on to show that even when there is an occurrence of a sub surface 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

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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  $k_d$  attenuation coefficient (see also Morel, 1988 for empirical model). If the  $E_d$  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

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surface and mean. i.e. for surface values multiply the surface chlorophyll concentration by the  $E_d$  and for water column do a trapezoid integration of the chlorophyll profile to the  $E_d$ .

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  $E_d$ ) 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 Kerguelen.

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 chl<sub>a</sub> 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<sup>-3</sup> (taken from fig 1).

#### 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

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of the majority of the data falling into both boxes (i.e the 0-3 ug l<sup>-1</sup> 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<sup>-1</sup>), low (e.g. <1 ug l<sup>-1</sup>) and moderate (e.g. 1-3 ug l<sup>-1</sup>) 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).

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

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).

It is not clear to me 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.

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#### 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?

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 sequestered from the surface 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 though 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.

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.

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

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

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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.

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

Figure 6: Rich and moderate biomass region headers need to start with uppercase. Units kg m-3 not kg/m3

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.

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

Line 19: carbon (C)

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

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?

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.

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Line 20 (add wavelength of scattering measurement).

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

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 . . . .

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

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

17423 Line 6: delete and Line 13: resolution (no s) Line 16: delete the

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 . . . . Line 11: delete as

17427 Line 8: persistent high Line 19: delete Then, rather As its trajectory. . . . Line 22: delete the words and correlated

17428 Line 10: delete as. After cloud cover full stop. New sentence. . . Instead. . . Line 23: delete the last the

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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

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Interactive comment on Biogeosciences Discuss., 11, 17413, 2014.

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