Biogeosciences Discuss., 11, C8075–C8085, 2015
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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.

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

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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”.
- 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.
- 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?
- 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.
- 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

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quenching-corrected day profile.
- 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).

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

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were selected.
- 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. . .
- 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).

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. 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

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except for profiler 2.
- 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. 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).
- 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 1μg 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 (<1μg L-1) but lead to over-estimation where chlorophyll was moderate to high (>1μg 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 μg L-1 . Given that our bio-profilers 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.
- 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”.

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 μg L-1) and moderate (0.5 to 3 μg 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.
- 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

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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?

- 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.

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

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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 μg L-1”: To me most chlorophyll profiles show values of 1 μg L-1 with only 2 profiles reaching maxima of 1.5 μg L-1.
- 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).
- 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 μmol 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.
- Importantly, I don’t think you can call “sequestration” a process that occurs above the mesopelagic zone. Carbon “export” would be more appropriate.

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.
- p. 17416 l. 19 “C”: Please define symbol on first use. Although not essential it would

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not hurt to define CO_2 and Fe as well (or to write “iron” in full letters).
-p. 17417 l. 1: What do you mean exactly by “mosaic of blooms”? Patchiness?
- 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.
- The introduction (p. 17417-17418) provides significant background to objective 2. Yet objectives 1 and 3 are not introduced at all.
- 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.
- p. 17421 l.26 “we applied the efficient method of. . .”: Please remove “efficient”. It is inappropriate unless fully supported by statistics.
- p. 17423 l. 22-23 “the drifts of the bio-profilers provided coverage. . . covering territories”: Awkward phrasing. Please reword.
- 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.
- 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.
- p. 17426 l. 5 “as the high chlorophyll levels decreased”: Delete “high” or reformulate.
- 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).
- p. 17428 l. 13 “near to the plateau”: Please remove “to”.
- p. 17429 l. 6 “contribute insignificant bias”: Please reword.
- Table 1: Caption should be relatively self explicit. Please explain what “Hull” and “WMO” stand for.
- 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”?
- Fig. 1: In figure caption specify what kind of satellite image you have used (sensor,

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product level and temporal averaging).
- 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.
- 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".
- 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.
- Fig. 5: The figure caption indicates "water column integrated (0-200 m) biomass" but the units (μg 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 y-axis labels ("Mean 0-200 m chlorophyll" and "Mean 0-200 m backscatter").
- 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?
- 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.
- Fig. 8: The figure caption says "Temporal evolution during eddy entrapment for bio-

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profiles 4”: Temporal evolution of what?
- I haven’t found any reference to the online supplementary material in the text. . .

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