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Biogeosciences Discussions

Interactive comment on "Optical community index to assess spatial patchiness during the 2008 North Atlantic Bloom" *by* I. Cetinić et al.

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We thank the reviewer for the time devoted to this manuscript, and for the comments and suggestions. Below we answer the reviewer's comments with references to appropriate parts of the text (in quotation marks).

The authors propose that the ratio between chlorophyll fluorescence (Chl F) and the particulate backscattering coefficient (bbp) is a proxy of relative contribution of diatom in plankton biomass and it can distinguish diatom community from pico- and nanophytoplankton community. This proposal is based on in situ measurements of a series of variables related to phytoplankton, taken by glider, float and ship. The authors discuss mechanisms of the co-variability between ChlF/bbp and diatom, to conclude that variability in ChlF/bbp is caused by the taxa-specific chlorophyll-to-carbon ratio of phyto-

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plankton and that the observed highest values of ChlF/bbp are indicative of Si-limitation to diatom.

Main comments The manuscript summarizes extensive measurements of phytoplankton and associated variables relatively well. Especially, the authors' findings that the optical index can be a useful proxy of relative carbon biomass of diatom has a great potential to advance understanding of phytoplankton ecology of their study region because the optical index can be determined from in situ measurements taken by commercially-available instruments and therefore a load of measurements would easily be taken. As a result, the paper has a good potential to be published in Biogeosciences. The authors discuss mechanisms controlling variability in ChlF/bbp, to conclude that the variability is due to (1) taxa specific differences in the cellular Chl-to-autotrophic carbon ratios (2) a fraction of the planktonic carbon due to diatom (Section 4.1, L 6, P12849) and (3) Si–limitation to diatom is responsible for the highest values of ChlF/bbp (Section 4.2, L8, P12852).

Firstly, I am surprised that (1) and (3) (as well as (2)) are rather explicitly concluded in the main text, but not mentioned in both Abstract and Conclusion. These conclusions should be mentioned there.

Answer: Changes have been made in Title, Abstract and Conclusion to point out to these results more clearly.

In the Abstract, we added the following sentence: "Observed changes in optical index were driven by taxa-specific chlorophyll-to-autotrophic carbon ratios and by physiological changes in Chl F driven by the silica limitation."

In the Conclusions we have added and modified text to the following: The observed shift in the optical index was primarily driven by the change in phytoplankton composition and distribution of biomass, reflecting differences in taxa-specific chlorophyll-to-autotrophic carbon ratios. Furthermore, the optical index allowed us to observe changes in the physiological status of the community as well, clearly isolating the senescent, Si-limited, termination stage of the diatom bloom from surrounding patches of diatoms not yet in senescence.

Secondly, although the conclusion (1) is exciting, it was drawn from their observation that Chl-to-autotrophic carbon ratio is higher by factor of two for diatom-dominated samples (L4, P12850). I am not really convinced as to how the authors were able to conclude the above (1) just because of that. No detailed discussion was given as to how difference in Chl-to-carbon ratio among different plankton community can be translated to variability in ChlF/bbp (only discussion on high Chl-to-carbon ratio for diatom was given). Please explain/clarify this, since it is crucial for readers to understand how the optical index proposed by the authors works.

Answer: Section 4.1 asks the question "Why does the Chl F/bbp ratio vary?" and systematically eliminates potential competing explanations for the variability in the ratio: 1) We reject fluorescence non-photochemical quenching as a source of the variability, as all data from depths shallower than 10 meters are rejected. Figure 2C shows the relationship between fluorescence and light; quenched data are excluded. Nutrient limitation, apart from Si limitation, is not a potential source of variability; nitrate is in great excess (> 8 μM) at this time of year. Although phosphate was not measured, no report from the subpolar North Atlantic has ever implicated this nutrient as a limiting nutrient. Hence we reject physiology as the explanation for the difference in the Chl F/bbp ratio. 2) Other field studies show higher Chl-to-carbon ratios for diatom dominated communities in contrast to communities dominated by small phytoplankton (Llewellyn et al., 2005; Putland and Iverson, 2007; Li et al., 2010). 3) While it could be possible that the ratio changed because heterotrophic protists became more abundant, Fig. 5D shows that changes in the percentage of heterotrophic carbon to total carbon is not driving the optical index.

We conclude – if chlorophyll fluorescence is a proxy for chlorophyll concentration (please see Fig. 2) and that if optical backscatter if a proxy for particulate organic carbon (Cetinić et al, 2012) – that higher diatom chlorophyll-to-carbon ratios are responsible for the observed higher Chl F/bbp ratios.

Thirdly, the significant scientific finding in this manuscript is, as concluded by the authors, that ChlF/bbp is an optical index of a relative abundance of diatom community. Meanwhile, the authors also introduce rather immature analysis on patchiness of the optical index, but failing to draw a significant scientific finding, as the authors themselves admit it by stating "analysis on patchiness did not manage to resolve the primary drivers of the observed patchiness in community composition in the Conclusion section (see L19,P12854)". As a result, description of patchiness does not add a value on this paper, and the section describing "patchiness" distracts the overall story of this paper. The main story and points of this paper (i.e. the optical index is a proxy of %diatom) would be much clearer without the discussions of the patchiness.

Answer: We have, following the suggestion of the reviewer, modified the Abstract and Conclusions to put stronger emphasis on the optical index. However, we do not agree with the reviewer that our analysis of patchiness is not important and distracting. This is, to our knowledge, the first analysis of this kind, where the distribution of the phytoplankton community was assessed for a two-month period with such high spatial and temporal resolution. Without the patchiness study, this is just a methods paper. In the manuscript, we have alluded that spatial patchiness is associated with physical processes that were happening on the larger scale, and pointed to models that have demonstrated that such patchiness, in these types of systems, originates from mostly physical drivers. Our analysis did not manage to resolve the primary drivers of the patchiness primarily due to the sampling schema; hence, we could not resolve temporal from spatial variability. Secondly, due to the lack of instruments for measuring higher trophic levels (zooplankton) on the same scales, we cannot say anything about potential top-down control of this spatial patchiness. Here, we are presenting a methodology that could, in the future, in combination with different sampling schemes and a new generation of imaging/acoustic instruments, offer a better view into the forcing functions that lead to heterogeneity of oceanic biodiversity and associated biogeochemistry.

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Other comments Title The optical index is developed here, actually to estimate a relative carbon biomass of diatom. The title needs to be revised to be more precise.

Answer: This optical index does not estimate a relative carbon biomass of diatom; rather it tells us something about plankton community type and how it changes over space and time. The variability in that ratio is driven by the taxa-specific chlorophyll-to-carbon ratio. We have changed the title to following: “A simple optical index shows spatial and temporal heterogeneity in plankton community composition during the 2008 North Atlantic Bloom”

Introduction Please re-consider the phrase “in situ remote optical sensing...as well as from space”, since Reader can easily confuse it with “optical remote sensing”. (Is “autonomous optical sensing” better in the present context?)

Answer: Changed to read: “Autonomous observations of phytoplankton are becoming increasingly ubiquitous, including in situ optical sensing from Argo-type and Lagrangian floats, gliders, and moorings, as well remote sensing as from space.”

Section 3 There are quite many variables and observation platforms appear in this section. Also different instruments, platforms, and/or data processing were applied even for a same variable (e.g. Chl, diatom carbon etc.). All measurement items, instruments, platforms and data processing methods could be better summarized, for example, in a table.

Answer: We appreciate the reviewer’s suggestion, and now include a table with measured parameters, associated acronyms/symbols and instrument/methods for specific platforms (Table 1).

Subsection 3.2 Please plot Si concentration, in one of the plots in Fig. 3, since it would be useful information for Reader to understand a latter discussion about Si-limitation in Section 4.2 (as well as it is an evidence for your statement L20 in P12844).

Answer: Silicic acid is plotted in Fig. 6B on the same plot as shown in Fig. 3. We have

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now added: Also see Fig. 6.

Subsection 3.3 Scatter plots (otherwise a table showing correlation statistics) between (1) %diatom and ChlF and (2) %diatom and bbp would be much simpler than Fig.5c and 5d to latter discussions in subsection 4.1.

Answer: Following the reviewers suggestions, we have calculated the suggested statistics. Calculated values are now part of the text in the same paragraph where we discuss figure 5: “Changes in Chl F or bbp were not strongly correlated with variability of % diatomC (respective r2 of 0.21 and 0.16, and p of <0.01 and p<0.1).” Opposite to reviewer, we believe that figure 5 is of grave importance to the Reader’s understanding of the paper. The point of Fig. 5C is to show that, while there is considerable scatter, that the chlorophyll-to-cell carbon ratio is greater when diatoms dominate. The point of Fig. 5D is to show that changes in the percentage of heterotrophic carbon to total carbon is not driving the optical index. The ratio is driven by the changes in the phytoplankton composition.

Subsection 3.4 L4-8 in P12847: Do you mean score instead of loading here? Answer: To make the figure caption clearer, we have changed the figure caption to read: “The length of a single parameter vector (black line with arrow) describes its contribution to the PC,. . .”

L9: I can’t see, from the information in Fig. 7, (1) that PC 2 shows no significant difference in the % diatom_c product among stations for the two types of diatom communities, i.e. Group 2 and 3” and (2) that PC1 can separate them as a function of nutrient concentrations, since which data points (stations) correspond to what group is not shown in the figure.

Answer: It seems that the misunderstanding here arises from the fact that it was not clearly defined that color coding represents the association of certain stations with a specific group (as defined by the optical index). We have rewritten the figure caption, so it clearly states which color is associated with which group: “PCA biplot for R/V

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Knorr CTD stations (n = 38), color coded by median Chl F/ bbp for 10-50 m, where blue to Group 1, yellow to Group 2 and red to Group 3."

Subsection 4.1 L9-12: Would the author please explain, step by step, why they refer to "relationship between POC and bbp as a function of plankton community composition" here, rather than a relationship between bbp and the community composition? I guess the latter makes more sense in the present context here. Since the authors have measurements of bbp and %diatom, they should be able to check by their own measurements whether or not bbp varies with plankton community composition (see also my comment for subsection 3.3). If bbp does NOT have correlation with community structure, the authors may want to look at a relationship between ChlF and %diatom (e.g. scatter plot) to check if ChlF alone is correlated to %diatom (i.e. ChlF alone is sufficient to explain %diatom), especially when the authors believe that effects of solar quenching and nutrient limitations on ChlF are minor or minimized in their dataset (L4-L6, P12849). The authors may also want to explain (i) what is an advantage(s) to normalize ChlF by bbp as an optical index for %diatom and (ii) whether the normalization actually enhance a signal of community composition, or weaken the signal, especially if bbp have correlation with community structure. I made comments above, because a comparison between ratios is sometimes not straightforward since numerator (or denominator) of a ratio is not a direct translation of that of another ratio, even though they may have a certain degree of correlation.

Answer: Figure 4A shows that values of chlorophyll fluorescence alone cannot explain the difference in community composition; for the same values of Chl F, bbp varies by a factor of 2.

We discuss this above, but repeat here for convenience. Section 4.1 asks the question "Why does the Chl F/bbp ratio vary?" and systematically eliminates potential competing explanations for the variability in the ratio: 1) We reject fluorescence non-photochemical quenching as a source of the variability, as all data from depths shallower than 10 meters are rejected. Figure 2C shows the relationship between fluo-

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rescence and light; quenched data are excluded. Nutrient limitation, apart from Si limitation, is not a potential source of variability; nitrate is in great excess (> 8 μM) at this time of year. Although phosphate was not measured, no report from the subpolar North Atlantic has ever implicated this nutrient as a limiting nutrient. Hence we reject physiology as the explanation for the difference in the Chl F/bbp ratio. 2) Other field studies show higher Chl-to-carbon ratios for diatom dominated communities in contrast to communities dominated by small phytoplankton (Llewellyn et al., 2005; Putland and Iverson, 2007; Li et al., 2010). 3) While it could be possible that the ratio changed because heterotrophic protists became more abundant, Fig. 5D shows that changes in the percentage of heterotrophic carbon to total carbon is not driving the optical index.

We conclude – if chlorophyll fluorescence is a proxy for chlorophyll concentration (please see Fig. 2) and that if optical backscatter if a proxy for particulate organic carbon (Cetinić et al, 2012) – that higher diatom chlorophyll-to-carbon ratios are responsible for the observed higher Chl F/bbp ratios.

Following the reviewers suggestions, we have calculated the suggested statistics. Calculated values are now part of the text in the same paragraph where we discuss figure 5: "Changes in Chl F or bbp were not strongly correlated with variability of % diatomC (respective r2 of 0.21 and 0.16, and p of <0.01 and p<0.1)."

In our Conclusions we make a very important cautionary statement: "The interpretation of these ratios must be based on in situ validation and used within a limited set of conditions, at least until a better mechanistic understanding is developed." We believe that we have done a very thorough job of in situ validation, which then allows us to make a statement about the spatial and temporal variability of the phytoplankton communities.

L13: "making a change in particle optics an unlikely explanations" Don't particle size and refractive index vary with particle concentration in natural environment? (In other words, there is no correlation among them?)

Answer: We are trying to say that regardless of the community composition, relation-

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ships between bbp and POC remained constant in this environment. Cetinić et al. (2012) discusses in detail how the change in community composition and associated changes in morphology, size and refractive index did not affect the dominant drivers of POC/bbp relationships. Later in the season, when coccolithophores bloom in the system, the POC/bbp relationship again changes as a function of plankton composition, due to the increased coccolithophorid backscattering (Alkire et al., 2014). In our Conclusions we now clearly state: “The interpretation of these ratios must be based on in situ validation and used within a limited set of conditions, at least until a better mechanistic understanding is developed.”

Subsection 4.2 The author found that Si-limitation is associated to “highest values” of the optical index. While this is a good finding, can the authors give a quantitative guidance on how “high” the values should be to imply Si-limitation, because I am currently unsure how this finding can actually be useful for users of the authors’ science.

Answer: In subsection 3.2, last paragraph (in original discussion paper page 12845, L6 –16) quantitative definitions of each of the groups were provided. Unfortunately, there is no hard and fast rule to diagnose Si-limitation vs. non-limitation. Figure 4B shows a continuum for Group 3 (Si-limited diatoms), with excess fluorescence likely increasing as Si limitation became more pronounced. Since relatively few studies report chlorophyll fluorescence as a function of Si limitation, please see Cleveland and Perry (1987) for a simple but useful discussion of excess fluorescence for nitrogen limitation.

Subsection 4.3 I am not sure if this section (hence, Fig.9 also) is needed, since no significant conclusion was drawn from here, as the authors admit it by stating “our analysis did not manage to resolve the primary drivers of the observed spatial patchiness” in Conclusion section. If patchiness were to be discussed, more extensive analysis would be needed to draw a conclusion. In any case, discussions of patchiness without a significant conclusion distract a story of the manuscript.

Answer: See answer above, third major point. Repeated here for convenience:

However, we do not agree with the reviewer that our analysis of patchiness is not important and distracting. This is, to our knowledge, first analysis of this kind, where distribution of the phytoplankton community was accessed on such high spatial and temporal resolution. Without the patchiness study, this is just a methods paper. In the manuscript, we have alluded that spatial patchiness is associated with physical processes that were happening on the larger scale, and pointed to models that have demonstrated that such patchiness, in these types of systems, originates from mostly physical drivers. Our analysis did not manage to resolve the primary drivers of the patchiness primarily due to the sampling scheme; hence, we could nott resolve temporal from spatial variability. Secondly, due to the lack of instruments for measuring higher trophic levels (zooplankton) on the same scales, we cannot say anything about potential top-down control of this spatial patchiness. Here, we are presenting a methodology that could, in the future, in combination with different sampling schemes and a new generation of imaging/acoustic instruments, offer a better view into the forcing functions that lead to heterogeneity of oceanic biodiversity and associated biogeochemistry.

Section 5 The authors should include their conclusion such as (1) Chl/C ratio is responsible for ChlF/bbp and (2) the highest values of ChlF/bbp is an indicative of Si-Limitation to diatom, since they are keys to interpret how the optical index the authors propose works.

Answer: See answer above, first major point. Repeated here for convenience:

Changes have been made in Title, Abstract and Conclusion to point out to these results more clearly. In the Abstract, we added the following sentence: "Observed changes in optical index were driven by taxa-specific chlorophyll-to-autotrophic carbon ratios and by physiological changes in Chl F driven by the silica limitation." In the Conclusions we have added and modified text to the following: The observed shift in the optical index was primarily driven by the change in phytoplankton composition and distribution of biomass, reflecting differences in taxa-specific chlorophyll-to-autotrophic carbon ratios Furthermore, the optical index allowed us to observe changes in the physiological status of the community as well, clearly isolating the senescent, Si-limited, termination stage of the diatom bloom from surrounding patches of diatoms not yet in senescence.

Fig. 3 Please increase a font size. Please describe what DM, E, S, M, Ed and P means in figure caption, too.

Answer: This figure will, in final version of the paper, be such that the font size is the same as in all other figures; currently, due to the format of the BGD, font seems smaller. Definition of the symbols is now part of the caption; Section 3.1 references the origin of these symbols (i.e., Alkire et al., 2012).

Fig.6 Please consider merging Fig. 6 into Fig. 3

Answer: We believe the current order of presentation of material aligns with the text. However, we have now added a reference to the end of the Fig. 3 caption: "Also see Fig. 6."

References:

Alkire, M. B., et al. (2014). Net community production and export from Seaglider measurements in the North Atlantic after the spring bloom, J. Geophys. Res., 119, 6121-6139.

Cetinić, I., Perry, M. J., Briggs, N. T., Kallin, E., D'Asaro, E. A., and Lee, C. M. (2012). Particulate organic carbon and inherent optical properties during 2008 North Atlantic Bloom Experiment, J. Geophys. Res., 117, C06028, 10.1029/2011jc007771.

Cleveland, J., and Perry, M. Quantum yield, relative specific absorption and fluorescence in nitrogen-limited Chaetoceros gracilis, Marine Biology, 94, 489-497.

Interactive comment on Biogeosciences Discuss., 11, 12833, 2014.