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Interactive comment on "Phytoplankton dynamics in contrasting early stage North Atlantic spring blooms: composition, succession, and potential drivers" *by* C. J. Daniels et al.

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Anonymous Referee #2

We thank the reviewer for their comments and address them below.

Question: Can the differences observed between the ICB and NWB bloom biomass and composition be the result of a different timing of the bloom ? If T °C are different and the bloom progresses northwards, how do you compare both stages ?

While it is possible that the two sites were in different stages of bloom formation, satel-

lite Chl a indicates that the two sites were in the early stages of bloom formation, with the rapid increase in Chl a occurring at approximately the same time later in the season. That the NWB was colder and further north could be factors, both of which are discussed in section 4.2: the small difference in irradiance and temperature is unlikely to have a significant effect. Although blooms generally progress northwards, the timing of bloom formation is far more complex (Henson et al., 2009) and as such it is possible to compare two sites as we have done here, as long as the physiochemical and biological features are considered.

Page 3 line 23 : Correct "nanno" sized diatoms ; what is the dominant genera then ? *Minidiscus* sp. ?

We have corrected this, and added in that *Minidiscus* spp. were the dominant genera.

Page 4 line17 : the authors insist on the importance of the timing and magnitude of the bloom to determine its biogeochemical impact, but in the paper a clear focus is set, rightfully so, on community structure, clearly this is important so I would add this variable to the list at the end of this sentence "and the variability in, bloom timing, magnitude and community structure"

We have added this in as suggested.

Page 5 line 18 : you browse through the 3 bloom development hypotheses, maybe you could state their name : CDH, CTH, you do it with the first two, but not the last one line 18 : DRH (disturbance-recovery hypothesis)

We have now defined the DRH hypothesis explicitly as requested.

Page 7 line 12 : "During the spring bloom: : :would occur early in the growth season (March-April)" this sentence is grammatically awkward, and March-April does not seem very 'early' in the season if you are trying to confront your observations with one of the three bloom development hypotheses, which predict much earlier bloom developments ? Also correct "occurr" with "occur".

We have reworded the sentence as suggested. We consider March-April early in the season as seasonal Chl a from satellites shows that phytoplankton blooms in this region do not peak until later in the season (Henson et al., 2009).We have corrected the typo.

Page 7 line 10 : I am not a native English speaker, but I am not sure that "succeeded" can be used with "being" in "some groups being succeeded due to competition"

We have reworded as suggested.

Page 10 line 9 "particulate biogenic silica samples were collected in the same manner as PIC" is a bit imprecise, did you also rinse the filters with trace ammonium solution (pH 10) ? Also I don't understand the reference chosen for this method (Brown et al. 2003), in this paper the authors only adress the 32Si uptake protocol, and use glass vials for extraction, which has no impact on 32Si concentrations, but will give completely wrong results for particulate silica due to leaching from the glass. A more adequate method reference would be Ragueneau Tréguer 1994, but please indicate more precisely how BSi extractions were carried out (type of tubes used for extraction), and whether a correction for lithogenic silica was at all made.

We apologise for the confusion concerning the protocol for particulate silicate samples and have reworded and expanded this section to provide more detail. The filters were rinsed with trace ammonium solution. Brown et al. (2003) measure biogenic silica as well 32Si, however we have now added the reference to Ragueneau and Tréguer (1994) as suggested. Extractions were carried out in polypropylene tubes and no corrections were made for lithogenic silica.

Page 12 line 24 : figure 5a is cited before any mention of Figure 4 is made, please swap figure order or rename figure numbers accordingly in the text. Page 14 line 2 : first mention of Figure 4

We have swapped the figure order as suggested

Page 16 section 3.4.3. some homogeneity with the previous paragraph for coccolithophores would be best. Either identify all taxa down to the species level (you do it for Guinardia striata but not for the others where only a genera name is given). If not determined, indicate sp. or spp. if one or several species were observed.

Diatoms were only identified to genus level while coccolithophores were identified to species level. We have reworded the text to reflect this.

The next sentence and associated graph (Figure 5) are very confusing. The sampling strategy for diatoms and bSiO2 is not clear to me from this paragraph. I understand two vertical profiles were done at each visit for bSiO2, but only one for diatom cell counts. But you mention a "significant variability observed in bSiO2 between the station visits", don't you mean AT each visit that there was a significant variability between the first and second vertical profile ? Then when was the SEM vertical profile sampled? on the other cast than the lugol samples ?

We apologise for the confusion, we did indeed mean significant variability between each CTD profile as suggested. We have corrected the text accordingly. The SEM samples were collected on the other cast than the Lugol samples, we have now made this clear.

Figure 5 is very hard to follow, and I have several issues with it : - it would be best to convert the "Day of the year" axis into the actual sampling dates, (later on page 17 line 1 you mention "a peak concentration was reached on 10 April", thus it would be clearer to have the same reference to date in both your text and figure. -then to align both graphs vertically, so that one histogram bar from Fig b, corresponds precisely to the bSiO2 concentration given in fig a. –There is no mention anywhere as to which depth these bSiO2 and diatom concentrations

correspond to ? as they are in mmol m^{-3} and cells ml^{-1}, they are not integrated and must reflect one depth ? If you sampled vertical profiles, why not show integrated concentrations in fig a and then the representative community below ? Was just one sample counted at each visit for diatoms ? -What's the justification for bSiO2 double casts sometimes very close in time (two consecutive days) and sometimes several days apart ? -in the legend (b) diatom "species" again should be replaced with "genera", or "sp./spp." should be added to each taxon in the figure. -"*Psuedo-nitzschia*" in the legend has a typo and should be "*Pseudo*" -the two middle grey colors are hard to distinguish one from another, please add some motive to your bars, or make the last one white so you only use 3 shades of grey instead of 4.

We apologise that this figure was hard to follow. To improve consistency we have now referred to day of year in the text and have listed both dates and day of year in all tables, however we have not converted the figures to actual sampling dates as this does not allow for continuous axes. Aligning both graphs vertically would not allow for BSi from both sites to be presented and therefore we have left it as is. The data presented in this figure are from surface samples (5 - 15 m), which we have now made explicit in the methods, the figure caption and in table 3. Diatoms were only counted from surface samples so vertical profiles of diatoms are not available for integrating. Diatoms were only sampled from the pre-dawn cast while bSiO2 was sampled from both the pre-dawn cast and the second cast. We cannot provide a justification for the inconsistency in time between CTDs; this study was part of a large multidisciplinary cruise with many different deployments, as such we could not control the timing of all CTD casts. We have corrected the caption and the typo. We have changed the figure to colour to remove ambiguity.

Since you also measured PIC and counted coccolithophores, I don't understand why you did not put up a similar figure for this group, the direct comparison of PIC vs bSiO2 in the same graph, and of diatom and cocco counts in another

would have been nice.

A direct comparison of integrated PIC and bSiO2 is made in Table 2 and cells counts of diatoms and coccolithophores is made in Table 3. We feel that these comparisons are best represented in these tables rather than in an additional figure.

I could not find anywhere an indication of which depth were sampled and counted for diatoms and cocco? you don't say so in section 2.3 of your methods, nor in your Figure 5, nor in your Table 3. Are the presented counts only surface samples? How do they compare with the vertical profiles? Were bSiO2 and PIC always maximum at surface levels?

We apologise, this was an oversight on our part. Samples for diatoms and coccolithophores were collected from surface samples (5 - 15 m). We have now made this explicit in the methods, the figure and the table. bSiO2 and PIC were not always maximum at surface levels but phytoplankton counts are unavailable from the other depths for a comparison.

Page 22 line 3: correct "occurr" with "occur"

We have corrected this.

Page 23 line 6: correct "and out in situ" with "and our in situ"

We have corrected this.

Page 24 line 3 to line 6 : I am not sure I follow this argument that the species best correlated to bSiO2 is the major exporter of bSiO2, plus you mix 'exporter' and 'producer' of bSiO2 in the same sentence, and these two definitions could be quite different.

We have reworded this sentence to differentiate between producer and exporter, suggesting that the major producer of bSiO2 has the potential to be the major exporter of bSiO2.

Page 24 : can the determination of the genera *Minidiscus* sp. be confirmed with SEM images ? It would be interesting to know for sure. I agree this genera can be important and easily overlooked in lugol samples, since you have SEM samples, it would be nice to confirm identification. Also I find that the paper by Boyd and Newton 1995 for the NABE program should be cited somewhere : they did observe a *Nanoneis* sp. Bloom one year (one of the smallest diatom species known, and probably occupying a similar niche with *Minidiscus* sp.), and *Chaetoceros* sp. bloom the following year at the same site.

We have confirmed using SEM images that the major genus was *Minidiscus*. We have now incorporated the paper by Boyd and Newton 1995 into our discussion.

Page 24 line 17, line 20; page 25 line 4 : correct "nanno" with "nano"

We have corrected this

References

Brown, L., Sanders, R., Savidge, G., and Lucas, C. H.: The uptake of silica during the spring bloom in the Northeast Atlantic Ocean, Limnol. Oceanogr., 48, 1831-1845, doi:10.4319/lo.2003.48.5.1831, 2003.

Henson, S. A., Dunne, J. P., and Sarmiento, J. L.: Decadal variability in North Atlantic phytoplankton blooms, J. Geophys. Res., 114, C04013, doi:10.1029/2008JC005139, 2009.

Ragueneau, O. and Tréguer, P.: Determination of biogenic silica in coastal waters: applicability and limits of the alkaline digestion method, Mar. Chem., 45, 43-51, 1994.

Interactive comment on Biogeosciences Discuss., 12, 93, 2015.