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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.

Anonymous Referee #2

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Review of the manuscript Âń Phytoplankton dynamics in contrasting early stage North Atlantic spring blooms : composition, succession, and potential drivers Âż. C.J. Daniels and co-authors

This is an interesting study presenting data and hypotheses for the early spring bloom development in the North Atlantic. It presents more classical biogeochemical and biological data with a new focus. The introduction presents a nice overview of the three main hypotheses explaining the spring bloom development in the Northern Atlantic (CDH, CTH, DRH as reviewed in Behrenfled and Boss 2014). Hence the reader maybe expects that the upcoming results will be used to favor one of the three hypotheses, but this is not so clear from the discussion/ conclusion section. I recommend that this

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paper be published with a few minor corrections to both text and figures as detailed 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° are different and the bloom progresses northwards, how do you compare both stages ?

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

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”

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)

Page 7 line 12 : “During the spring bloom. . .would occur early in the growth season (March-April)” this sentence is grammatically ackward, 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".

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”

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

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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.

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

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. 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 ?

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

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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. -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. -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 ?

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

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

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. . .

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

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site.

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

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