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Biogeosciences Discussions
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Interactive comment on "Spring bloom community change modifies carbon pathways and C : N : P : Chl a stoichiometry of coastal material fluxes" *by* K. Spilling et al.

K. Spilling et al.

kristian.spilling@environment.fi

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Anonymous Referee #1 Received and published: 29 August 2014

Referee #1, General comments: Spilling et al. report on the effects of phytoplankton community composition on stoichiometry of planktonic biogeochemical processes, in a coastal model system. This paper presents a nice description of this point and uses a lot of previous literature to put the study in context and also to support the results. I am in favor of some of the points about stoichiometry and C budget in the discussing section. More importantly, their results originate from a total of 40 coastal mesocosm experiments in 5 years, rather than in monocultures. The mesocosm experiments rep-

resented an ideal semi-natural experimental system for community-level comparisons. Therefore, this is potentially a very useful paper providing important information on an understudied of consequences of phytoplankton community dominance patterns for marine biogeochemical cycles. However there is quite a bit of detail missing in the materials and method sections which can lead to some uncertainty in the results section as to which results have been presented. In addition, some important points are never discussed in the manuscript.

Referee #1, Comment 1: For example, how many parallel unites were used for one treatment? Is duplicate sampling conducted in one carboy or in different carboy?

Author reply: The duplicate sampling (e.g. filtration) was done from one carboy, and we have added the words: "from each carboy" to make this clear.

Referee #1, Comment 2: How about the silicate results? Although experiments have added silicate, there are no silicate results and discussion in the manuscript. When discussing the competition between diatom and dinoflagellate, results of silicate is very important.

Author reply: A very good point. The reason why it is not included was that silicate did not have any effect on the outcome of the experiment. The treatments with N and P addition behaved very similar to the treatment with N, P and DSi addition. Also the control and the treatment with only DSi addition were similar (Fig 1).

We have included this as a paragraph in the results chapter of the paper:

Results: "The N : Si ratio was lowest during diatom dominance ($p < 0.001$), and increased with dinoflagellate dominance, especially during exponential growth phase (Fig 6). In general, the drawdown of N and P and build-up of biomass (e.g. POC) were very similar in the N, P and N, P & Si treatment and in the control and Si addition. "

Discussion:

"The N : Si ratio of the seston was, as expected, affected by the dominance pattern

(up to 4-fold difference) as only the diatoms are utilizing silicate. The fact that the DSi addition made little to no effect on the outcome suggests that the initial DSi concentration was sufficient for the diatom community and did not affect the competition with dinoflagellates. “

Referee #1 Comment 3: And, the effect of bacteria cannot be ignored as well, in particular in stationary growth phase. I understand that, if taken into account the effects of the bacteria the system will become more complicated, but the relevant data should be presented.

Author reply: We agree, during exponential growth phase at these temperatures bacterial production most likely remains low (Lignell et al 1993, Mar Ecol Prog Ser. 94: 239-252), but increasing during the stationary growth phase. Unfortunately we do not have the data on the bacterial community or production in our experiments.

Referee #1, Comment 4: In addition, readers need to flip the manuscript because the graphics and text are not linked. For example, it is better to add (Fig. 3) after “This was supported by the difference in C: Chl a ratio between diatom and dinoflagellate-dominated communities.”

Author reply: We made the suggested change

Specific comments: Referee #1: P-11867 L-17 “Klais et al., 2011” is Klais et al., 2013?

Author reply: Yes there is a point although Klais et al 2011 was the initial paper dealing with ongoing change in the phytoplankton community. We added the 2013 references as well.

Referee #1: P-11868 L-11 How many cells were added to the natural communities?

Author reply: Here we discovered a mistake, during 2005 we did not have proper cell counts for the diatom addition, and these 4 mesocosm had already been removed from the analysis, but this was not reflected in e.g. Table 1. We have now made appropriate changes to Table 1 and the materials and methods chapter. We added the cell numbers

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for the 2007 experiment.

Referee #1: P-11869 L-28 and thereafter the “duplicate” Is duplicate sampling conducted in one carboy or in different carboy?

Author reply: The duplicate sampling (e.g. filtration) was done from one carboy, and we have added the words: “from each carboy” to make this clear.

Referee #1: P-11882-11883 In the stationary phase, strongest diatom-dominated communities had up to 3.6 times higher seston C:N content (regression in Fig. 6) than anticipated from Redfield ratios, thus support the “overconsumption” carbon flow pattern. Based on this result, whether it can be considered that diatom-dominated communities had higher carbon overconsumption than dinoflagellate-dominated communities or not? And why estimates of carbon overconsumption in offshore area where is suggested to be no diatom dominated is higher than the values of coastal area?

Author reply: Yes the diatom dominated communities had a higher carbon overconsumption than dinoflagellate dominated communities. The offshore areas discussed were values from literature and are from other sea areas and cannot be directly compared with the numbers we present.

Referee #1: Fig 7 High respiration rates for dinoflagellate-dominated communities induced low carbon assimilation efficiencies in late bloom stages (Fig. 5). And therefore, a significantly lower level of corresponding CO2 drawdown enhancement to POC in the stationary phase of dinoflagellate-dominated communities exhibited (Fig. 6). But why significantly higher C : Chl a ratio of dinoflagellate-dominated communities was observed in Fig.7?

Author reply: Any ratio is made up of the numerator and denominator, and will of course be affected by changes in both. The reviewer points out that the increase in POC was lower in dinoflagellate than diatom dominated communities. The fact that C : Chl a ratio was higher during dinoflagellate dominance suggests that Chl a was reduced to a

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greater extent for dinoflagellate- compared with a diatom-dominated community. Added some information about this in the Discussion:

“An interesting difference between the diatoms and dinoflagellates, dominating the spring bloom in the Baltic Sea, is their different response to the onset of inorganic N depletion. Diatoms continued to run photosynthesis building up the internal C storage, and also releasing C as DOC, probably as a way of dissipating excess light energy (Myklestad et al., 1989; Staats et al., 2000). Dinoflagellates, in contrast, seem to shut down the photosynthetic machinery earlier as a way to acclimate to a condition with reduced need for inorganic carbon fixation. The observed increase in C : Chl a in the two groups could be caused by different reasons, for the diatoms primarily an increase in POC, while for the dinoflagellates the decrease in Chl a was relatively more important. “

Anonymous Referee #2

Received and published: 25 September 2014

Referee #2: This is an interesting manuscript, describing the carbon flow and community stoichiometry over the full duration of natural, mixed community Baltic Sea spring bloom events. The authors performed several mesocosm experiments with different seed communities and nutrient enrichments and followed changes in community composition, biomass production and nutrient fractions for several weeks. Data are mostly novel and interesting and the subject area is clearly appropriate for publication in Biogeosciences. The experiments were correctly planned, described and carried out. The manuscript is straightforward and clear and the discussion section covers all relevant aspects. For these reasons I think that the manuscript deserves publication in Biogeosciences.

In addition to comments of Referee #1, there are a couple of points of relatively minor nature – especially in the materials & methods and results section - that the authors should take into account and add in a revised version of the manuscript:

Referee #2: p.11866 l.1 Please be more precise. How are thermal stratification patters and freshwater runoff connected to phytoplankton community changes? Which groups appear under which conditions?

Author reply: We have added some more detail to the text:

"for example diatoms typically dominate during times with high turbulence whereas dinoflagellates are more common after firm stratification has been established"

Referee #2: p.11866 l.2-3 This references to the Arctic environment is confusing here as your study deals with temperate coastal waters of the Baltic Sea and not the marine Arctic region. I suggest removing this sentence.

Author reply: The Baltic Sea is considered a Sub-Arctic region and with several similarities to the Arctic such as an annual ice-cover. The point is that these areas will be most affected by the ongoing global warming. To make it clearer we have added temperate and arctic:

"The extensive temperate and arctic shelf seas. . ."

Referee #2: p. 11866 l.15 Please be more precise. Which functional aspects do you refer to?

Author reply: They are mentioned right above, but to make this clear we have added "of these":

"Several of these functional aspects of algal physiology are. . ."

Referee #2: p.11867 l.5ff Decomposition does not only occur in the sediment but also in the water column. Please include pelagic bacterial degradation here.

Author reply: We agree and have added:

". . .this should also affect the decomposition by pelagic bacteria."

Referee #2: p. 11868 l.17-19 Can you briefly discuss potential effects of the culturing

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vessel? I imagine that there was more light available for photosynthesis in the transparent vessels compared to the white plastic barrels. Did you measure light intensities in the water?

Author reply: The lights were placed on top, so the light intensities should not be very different. The 80 L barrels were higher and there was a bit lower light in the bottom of the tank compared with the 20 L tank. However, the difference was negligible as for example the growth between years was similar.

Referee #2: Can you estimate the amount of wall growth in your experimental units (or did you determine wall growth)? Did you clean the walls regularly?

Author reply: We could not see any wall growth and we do not think this was a problem. For this reason we did not clean the walls.

Referee #2: p.11868 l.25 Did you sample surface waters or at a certain depth?

Author reply: It was taken from the surface. This information was mentioned above in the text (L 14):

"For each experiment, containers were filled with natural surface water and. . ."

Referee #2: p.11869 l.11 Light intensities of 90 mol photons m-2 s-1 seem quite low to me. What are mean light intensities in this area during this time of the year at the sea surface?

Author reply: The light intensity at a clear day is of course much higher, but the phytoplankton are not all at the surface. They are mixed around and it is the total light exposure that determines the growth, not the light intensity at the surface. Our aim was to have the total light exposure similar to the natural environment (but it will of course never be the same in a constant light), and we were relatively close judging from the community growth rate.

Referee #2: p.11869 l.18 What was the sampling volume? How much volume did you

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remove over the whole experimental period?

Author reply: Sampling volume was relatively small, a total of 500 ml per sampling and we never sampled more than half the volume, which was added to the text:

“The total sampling volume never exceeded half the total volume.”

Referee #2: p. 11869 l.21 Add reference to Table 1 here.

Author reply: Added.

Referee #2: p. 11870 l.11 How much volume did you filter for POC/PON analysis?

Author reply: 50-100 ml depending on the biomass concentration, we added this information to the text.

Referee #2: p. 11871 l. 17 Give typical background concentrations for refractory DOC in the Baltic Sea.

Author reply: In the open Gulf of Finland it is 350-400 μmol C L-1 as DOC (Hoikala 2012), and we added this to the sentence

Referee #2: p. 11871 l.20-23 Usually growth phases are defines according to cell numbers or growth rate. Does your phase definition agree with your calculated growth rates (μPOC, μChla)?

Author reply: Yes

Referee #2: p. 11873 l.21 I found the definition for “mixed community” in the legend of figure 7. Please add this information to the method section or early in the results section.

Author reply: Yes we added this to the beginning of the results chapter where Fig 2 appear:

“The phytoplankton community was divided into three categories: diatom dominance (>80%), mixed community (20-70% dinoflagellates) and dinoflagellate dominance

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(>70%). The rationale behind setting the group boundaries was from the apparent difference in species evenness (Fig. 2)."

Referee #2: p. 11874 l.5-7 Figure 3b shows CAE (the ratio between POC and TGP) plotted against POC but the correlation is quite low (R2 = 0.12 and p=0.04) indicating a high variation in TGP. Even if the correlation can be argued as statistically significant, due to high variation in the data, I don't agree with the authors' conclusion of lower loss rates with increasing growth rates. I recommend interpreting this data more carefully.

Author reply: We removed the interpretation

Deleted text: ". . .indicating lower loss rates with increasing growth rate."

Referee #2: p. 11875 l.11 Please give R2 and p-values for the significant change in C:Chla ratio in the dinoflagellate dominated group.

Author reply: Added to the text:

(R2 = 0.53, p<0.0001)

Referee #2: p. 11877 l.8 Please include heterotrophic remineralization in the water column here.

Author reply: Added to the text:

"This increases the heterotrophic remineralization in the water column."

Referee #2: p.11877 l.16 Did you check for grazers and bacterial/viral abundances? Can you please discuss the role they might have played in the development of the phytoplankton bloom during your studies?

Author reply: No. During exponential growth phase bacterial production is low, but increasing during the stationary growth phase. Unfortunately we do not have the data on the bacterial community or production. There were very few grazers present, and the grazing pressure is generally low in the area during spring, something we have

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pointed out in several places throughout the manuscript.

Referee #2: p. 11878 l.24 Bacterial or grazer biomass may have contributed significantly to total biomass masking differences due to phytoplankton community composition.

Author reply: Yes this is of course possible in theory, but not very likely, at least not during the period with active growth. Please see also the answer to the previous comment.

Referee #2: p. 11880 l.5 Later on (p. 11881) you discuss also the aspect of aggregation of dissolved organic matter to larger particles such as TEP and a coating of phytoplankton cells by mucus layer. These carbon compounds contribute to the POC pool influencing C:Chla ratios and μPOC. Please include this aspect also here into the discussion.

Author reply: We agree and we added:

"... potentially affecting e.g. C : Chl a and μPOC..."

Referee #2: p.11897 Figure 2 Please add here information about your division into the three categories (in the legend and in the figure). "The phytoplankton community was divided into three categories: diatom dominance (>80 %), mixed community (20–70% dinoflagellates) and dinoflagellate dominance (>70 %). The rationale behind setting the group boundaries was from the apparent difference in species evenness between these groups." It is not clear to me, why you chose 70% dinoflagellate dominance as a threshold. Please explain in more detail.

Author reply: Added as suggested. The 70% was chosen as the species evenness started to bend downwards at this point.

Referee #2: p. 11899 Figure 4 You refer to Fig. 1 for species evenness but I guess it is Fig. 2?

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Author reply: Yes, and we corrected this.

Referee #2: p. 11902 Figure 7 You refer to Fig. 1 for species evenness but I guess it is Fig. 2?

Author reply: Yes, and we corrected this.

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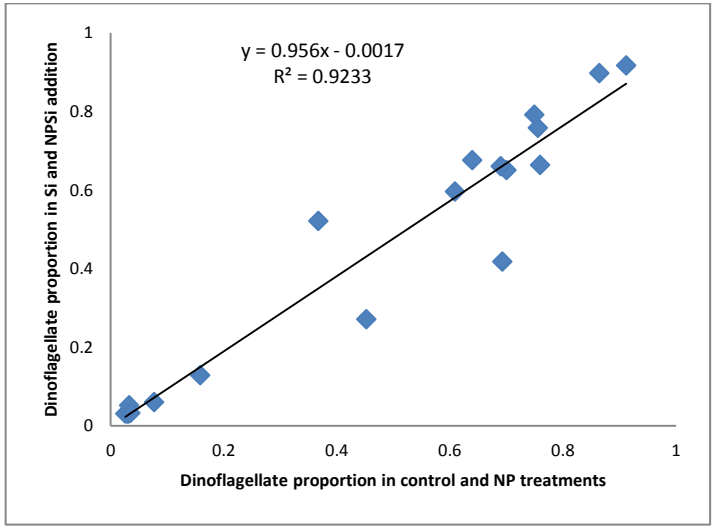

Fig 1. The dinoflagellate proportion of the total diatom and dinoflagellate community in the control (no addition of nutrients) and NP (nitrate and phosphate added) treatments plotted against the dinoflagellate proportion in the Si (silicate added) and NPSi treatments. The relationship is close to 1:1 and with a relative high R^2 value (0.92), indicating that the addition of silicate made little to no difference in the outcome of the competition between diatoms and dinoflagellates.

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