

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.

Anonymous Referee #1

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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 represented 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

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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.

For example, how many parallel units were used for one treatment? Is duplicate sampling conducted in one carboy or in different carboy?

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.

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.

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.”

Specific comments:

P-11867 L-17 “Klais et al., 2011” is Klais et al., 2013?

P-11868 L-11 How many cells were added to the natural communities?

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

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

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estimates of carbon overconsumption in offshore area where is suggested to be no-diatom dominated is higher than the values of coastal area?

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?

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