Biogeosciences
Discussions

Interactive comment on "Effects of elevated CO$_2$ and temperature on phytoplankton community biomass, species composition and photosynthesis during an autumn bloom in the Western English Channel" *by* Matthew Keys et al.

Anonymous Referee #1

The manuscript from Keys and collaborators deals with the impacts of ocean acidification and warming on the composition and biomass of the phytoplankton community in Autumn in the Western English Channel. The authors conducted a 36-day experiment in microcosms filled with seawater sampled in the declining phase of the autumn phytoplankton bloom at station L4 on October 7th 2015, where a long-term dataset of nutrient, chlorophyll a and community composition (among many other parameters) is available. This sampled seawater (sieved onto 200 microm) was used to fill 16 borosilicate bottles (2.5 L) corresponding to 4 replicates of 4 treatments. Treatments were

1) control (no modification of pH, T of 14.5 °C), 2) high CO2 (800 microatm, T of 14.5°C), 3) high T (ambient pCO2, T of 18.6 °C), and 4) high CO2 and T. The high T treatment appeared to be applied at once (seawater placed in a temperature regulated outdoor incubation system, while pCO2 was increased gradually to 800 microatm over 8 days. Each bottle was linked to reservoirs filled with filtered seawater in which nutrient concentrations were modified (NOx from 0.24 microM in situ to 8 microM, PO4 from 0.09 microM in situ to 0.5 microM, silicate maintained at in situ concentrations). pCO2 were also controlled in the reservoir for high pCO2 bottles in order to maintain constant pCO2 in the bottles. After 2 days conducted in batch mode, 10-13% of each experimental bottle were replaced with the medium contained in the reservoirs. Various parameters were sampled during the experiment at different frequencies. While chlorophyll a and carbonate parameters were sampled every 2-3 days, phytoplankton community biomass (biomass calculated based on flow cytometry data) was estimated on 5 occasions (T0, T10, T17, T24 and T36), and POC/PON were measured on 3 occasions (T0, T15, T36). Finally, the photosynthetic efficiency was investigated based on samples taken only at the end of the experiment (T36). Based on this experiment, the authors conclude that 1) in all treatments the phytoplankton community shifted from dinoflagellates to nanophytoplankton, 2) large nano-flagellates dominated in the control treatment, while smaller species dominated in the high CO2 and high T treatments, 3) combining these 2 "stressors", led to a different community with a higher proportion of dinoflagellates and especially of the HAB species Prorocentrum cordatum and 4) finally the authors conclude that "future increases in temperature and pCO2 do not appear to influence coastal phytoplankton productivity during autumn in the WEC which would have a negative feedback on atmospheric CO2".

Although this manuscript deals with the very important question being "how coastal phytoplankton will respond to global anthropogenic stressors and will coastal plankton community exert a feedback on atmospheric CO2 increase and global warming", I definitely cannot recommend this manuscript for Biogeosciences in its present form. My main concerns are 1) related to the experimental protocol considered and how it

could be used as a tool for projecting future evolutions of coastal plankton community in this area, 2) the way data have been used and conclusions that have been drawn based on those data, and 3) the way the manuscript is organized mixing results from an experiment and long-term in situ data, with both parts being in my opinion poorly related.

1) Experimental set-up

The authors mention that the effects of pCO2 and temperature on phytoplankton succession in autumn is presently unknown, which is the reason for their experimental investigation. However, I am really concerned about the experimental choices that have been made and I would like the authors to explain much better the rationale behind these choices. First of all, seawater was sampled at the end (let's say the declining phase) of the autumn bloom leading to low nitrate concentrations of 0.2 microM. My question is, is it realistic to force this system again to high levels of nitrate (8 microM, while the long-term average of NOx at station L4 is of 4.1 microM in October/November)? Don't you think your results are biased because of this, and prevent you from extrapolating your experimental results to the "real world"? The second experimental choice that is of concern to me is that, during the whole experiment, while chlorophyll a concentrations vary from 0.1 to 3-7 microg/L, the conditions of the carbonate chemistry have been maintained constant. Again, I do not understand the reason behind this choice. In situ, surface pCO2 is extremely dependent on biological activity (by far the main reason for the ocean being a sink of atmospheric CO2). So I have the same question that really needs to be fairly discussed in the paper: Don't you think your results are biased because of this, and prevent you to extrapolate your experimental results to the "real world"? Related to this, it seems to me that nutrient concentrations have been maintained constant during the experiment, although absolutely no data of nutrient concentrations are shown in the manuscript, which is not acceptable to me. Certainly much more than C availability, N and P (and Si) availability structure the composition of phytoplankton communities and control their productivity. What is

the reason for maintaining these parameters constant? Another experimental choice is to sieve the sampled seawater onto 200 microm, removing mesozooplankton grazers. Grazing is certainly a very important process shaping phytoplankton communities, what are the consequences of this choice, does it hamper your conclusions? Another missing (in my opinion) very important compartment is the heterotrophic prokaryotic community for which no data are shown in the manuscript (bacterial abundance at least could help). Finally, concerning these experimental choices, why did you conduct an experiment over 36 days? I will come back on that later in the second part, but this choice needs to be explained. This is a very important point since you base a lot of your conclusions on the interpretation of results obtained at T36 only.

2) Data analysis

The authors considered an experimental set-up in which they sampled their experimental bottles on a regular (yet variable depending on the parameter) basis. Nonetheless, many of their conclusions are based only on the analysis of data obtained at T36. For instance, they discuss on L456 to 462, that "chlorophyll a was significantly higher in the combination treatment at T36, ..., but Chl a was significantly lower in the high pCO2 treatment...". I apologize but this interpretation does not make sense at all. Actually, T36 seems to be the only sampling point for which these conclusions are valid. At the penultimate sampling point, there is as much Chl a in the combination treatment than in the high CO2 while high T and control lead to lower concentrations. If you choose another time point, you reach another conclusion... and so on... The entire dataset MUST be used instead of single points. This is actually even more problematic for parameters that have been sampled with a lower frequency (especially POC and PON), again conclusions have been drawn based on the last sampling point. At T10, the biomass is the highest in the high T, followed by high T/high COP2, then high CO2 and control with the lowest biomass. At T17, you can draw another conclusion etc... This is also true for community composition, for which you insist a lot on the abrupt increase in dinoflagellate abundance in the high T/high CO2 treatment at T36, while

their abundance is much lower at T24.... Again, what is the rationale in using data obtained after 36 days of incubation in a small volume with all the artefacts associated with this incubation technique...?

3) Structure of the manuscript

I am not convinced about the way this manuscript that combines analyses of in situ long-term data and experimental data, is structured. In my opinion, the analysis of in situ data should be put upfront in the manuscript, before describing and analyzing the experimental results. Furthermore, I am not convinced about the relevance of these observational data in this manuscript. Analyzing the distribution of the abundance of phytoplankton species just based on temperature and pCO2 is a huge simplification.

Minor comments.

Introduction. L36 to 58. Citing 10 years old (at least) papers in this section is not acceptable and suggests (wrongly I suppose) that the authors are not aware of recent literature. L43: please add "surface" pH of 0.3 units... L81: what is the reason of this warming? L83: this sentence makes no sense. If no significant trend, there is no increase....

Mat and Met L131: what were the PAR levels during the experiment?

Results L254: please add: "with a mean concentration over this period, of ... or equivalent Please provide data of nutrients and data of TA and DIC. Regarding TA and DIC, I am extremely surprised by Figure 2. How can you have similar pCO2/pH and CO_3^{2-}/HCO_3^{2-} between control T and high T? With a difference of 4 °C, this appears unreal.... If you keep pCO2 constant as you mention, you should have several hundreds of microM difference between HCO_3^- at 14°C compared to 18°C.... or am I wrong?

On several occasions, please add "atmospheric CO2 increase" when you refer to the potential negative or positive feedbacks on atmospheric CO2.

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