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Interactive comment on “Carbon cycling and phytoplankton responses within highly-replicated shipboard carbonate chemistry manipulation experiments conducted around Northwest European Shelf Seas” by S. Richier et al.

Anonymous Referee #2

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This study examines the impact of ocean acidification on phytoplankton and net carbon production while taking spatial variability into account. In general, the manuscript is well written and focuses on an important topic. Until now ocean acidification research has often focused on single species or clones, thus, the here presented community approach is timely. However, the weak points in this work are mainly the short experimental duration and that the present experimental design does not allow disentangling if differences in response among the experiments are owing to environmental conditions, community composition or their interaction. It is also not possible to draw any

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definite conclusions about net production, since the carrying capacities of the different communities remain unknown. Moreover, the title claims that the authors conducted thorough carbon budgeting, which is apparently not the case. Some suggestions, questions, corrections, etc. are made in the present comment and might be useful for the authors to revise their manuscript.

General comments and suggestions:

1. The authors traded off short experimental duration against several experiments at different geographical locations. They emphasise that a unique feature of their study is the inclusion of communities as a whole instead of just single species. However, as correctly stated by the authors, the only response that can be measured after the here presented maximum four days is physiological by nature. The short experimental duration does not allow responses to occur on the community (or genotype) level, i.e. in terms of species or genotypic sorting; yet this would exactly be the interesting point in a community approach. Furthermore, it is not possible to draw any definite conclusions about the net production from pure physiological responses. For this, it would be crucial to know the carrying capacities of the communities. The only general conclusion they can draw from this approach is the short-term response of total biomass and changes in particular phytoplankton groups. This, however, is not thoroughly applied in the statistical analysis as currently presented (See specific comment 5 below for suggested amendments). Moreover, do the authors think that the investigated communities harbour any response diversity? More precisely, do they expect that the communities would reorganize in response to ocean acidification? Would this have any effect on net carbon production? Please elaborate.

2. Each experiment was carried out with the community present on the sampling day under in situ environmental conditions. As the authors state themselves it is highly likely that the communities were not all in the same state of growth when they were collected from the water column. Some might have been in a post-bloom phase and consequently depleted nutrients while others have not been. This makes it certainly

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difficult to compare the experiments. Please further justify this approach. With the present design it is also not possible to ascertain if differences in response among the experiments are owing to environmental conditions, community composition or their interaction. Only a full factorial design (pCO₂ x community x environmental (nutrient) condition) would help to mechanistically explain the findings.

3. Throughout the manuscript the authors emphasise the high number of replicates featuring their study. However, it seems that they mix up true replicates with replicated experiments. As I understand, the number of replicates per treatment (pCO₂) is three, which is not exceptionally high. By comparison, Krause et al. had five replicates per treatment. The specific feature of your study is that you consider spatial variability by running several experiments, each at a different geographical location. But it is statistically not sound to throw all the results of the different experiments in a single ANOVA, except when the variable "Experiment" is included as a factor into the analysis. Since a paragraph explaining the statistics performed in this study is completely missing, it is difficult to figure out exactly what the author did (See specific comments below). This needs to be fixed.

Specific comments:

1. The title can be shorter and punchier; what is the main result/message of this study? Since the focus is not the entire 'carbon cycle' this term should be avoided. Also the phrase 'highly replicated' should be rephrased (see my comment above).
2. P 3497, L 14 Does the average natural irradiance vary among sites? What are the values?
3. Were grazers included in the experiment or were they removed before water was transferred into the experimental units? Please elaborate. In case grazers were present, what does it mean for the results; please discuss.
4. At first reading, I found it difficult to understand the experimental design itself. It

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took a while to figure out the number of the true replicates within each of the replicated experiments. This needs to be improved.

5. I miss a paragraph explaining the statistical analysis in the methods section. The focus is on phytoplankton responses, thus it is important to address the inherent biological variability with a proper statistical design. The experiments per se are no true replicates since the authors neither control for environmental conditions nor community composition, and they do not take place at the same time (see general comment). I suggest that the authors run a separate ANOVA for each experiment and the response variables therein they are interested in. Then, in order to draw the general conclusion of CO₂ effects across experiments calculate the log response ratios, and finally run a metaanalysis across all the experiments. Only then it would be possible to draw any general conclusions.

6. Particularly the results but also the discussion on the success of the carbonate chemistry manipulation seem to be excessive, notably because other differences in the set-up (see comments above) were completely neglected. I wonder if those parts can be cut.

7. The authors could further put their study in context with other recent studies investigating the effects of ocean acidification on phytoplankton community responses (e.g. Yoshimura 2013 J Oceanogr doi: 10.1007/s10872-013-0196-2, Eggers 2014 GCB doi: 10.1111/gcb.12421).

8. P 3493, L 7: change 'Egleton' to 'Egleston'

9. P 3502, L 14: correlation coefficient is given as 'r' not 'r squared'. Please change.

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