

Interactive comment on “Reduced growth with increased quotas of particulate organic and inorganic carbon in the coccolithophore *Emiliana huxleyi* under future ocean climate change conditions” by Yong Zhang et al.

Anonymous Referee #1

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General summary:

The Authors explore the effects of future ocean climate change scenarios on *Emiliana huxleyi* including several important environmental drivers in a “environmental clustering” method that allows for co-variation of environmental drivers that have been identified to affect different trait responses. They found that under future ocean climate change scenarios the growth of the coccolithophore is reduced and biogeochemically relevant traits (i.e. PIC and POC quotas) are increased. However, nutrient limitation (expected in future climate change scenarios) affected these traits in different ways

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and interactions. Therefore the Authors conclude, that their study contributes to the understanding of the response of *E. huxleyi* to future climate change.

General comments:

The manuscript is clearly written and structured. The study is well designed and explained considering how complex experiments tend to get when increasing the number of environmental drivers. Also, the rationale behind using a “environmental clustering” approach is clearly stated and the co-variation of the environmental drivers is well deduced from previous studies and literature. It is interesting to see in this study how much variation in response to nutrient availability and light there is, even though there are clear differences in PIC, POC quota and growth between ambient and future scenarios. These differences are put into context and discussed well in the manuscript. There is however, one critical point that I think is not well discussed and needs to be considered in the discussion. In the beginning of the discussion, the Authors make the comparison between ocean observations of Coccolithophores and try to highlight discrepancies to the lab experiment. While these comparisons are nearly impossible, because laboratory conditions are so different from natural conditions, I feel that this point merits further discussion. Environmental variation due to different geographical regions affects the environmental history an organism has experienced and thus how an organism can and will react to changes in the environment. In addition within species and within functional group variation in plastic responses of growth and other traits is well established (other publications by Zhang et al have already shown this as well) and can affect how “a species” responds to environmental change. Since the Authors want to make a conclusion what their study tells us about how a cosmopolitan and biogeochemically relevant group of phytoplankton react to future ocean scenarios and not only conclude something about the combination and co-variation of environmental drivers, I feel that ecological variability and environmental variability should be taken into account in the discussion. One way to approach this “gap” of knowledge could be a discussion about what experimental conditions (based on the given study)

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could now be focused on to further characterize the responses of other coccolithophore strains that i) come from environmentally different regions, ii) are more recently isolated and thus not acclimatized to long times spent in the laboratory.

Specific comments:

Line 120: here the Authors imply, that their study will help understand how biogeochemically relevant phytoplankton change in future climate change scenarios but this is not adequately discussed later on (see general comments)

Line 161: "adding low light" is misleading. Would it be possible to say that light was reduced?

Line 151 and 161: it would be good to have an idea about nutrient and light concentrations here already. The information following in line 190 comes a bit late and could even be combined as later on the pCO₂ manipulation is in focus.

Line 175: I do not fully follow the rationale behind adding the nutrient limitation stepwise

PIC quota Line 434 ff: I stumbled over the way that the effect of future ocean scenarios are increasing PIC quotas followed by the explanation of how PIC is reduced with increasing pCO₂. It would be helpful if there was one more sentence that relates the different results. In addition it could be helpful to highlight in Fig. 2-6, what parts of all of the results are used for the ambient-future comparison. Then the in-between data that are very interesting could become more clear.

Line 531: Please see the general comment: here the discussion should go further because not only oceanic conditions may be different but ecologically within species and functional groups there are many differences that can affect the results.

Line 612 ff.: I see how considering TEP as part of POC quota is important. But then the Authors also say that it is negligible. As it is written currently, the two sentences contradict each other a bit. Consider rephrasing

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Line 620ff: consider moving this part of the discussion about RNA and protein metabolism to where cell cycle is already discussed in line 580. Could fit better together.

Line 643: The conclusion about competitive ability comes a bit "sudden". Consider mentioning the implications of nutrient uptake on competitive ability earlier in the discussion where phosphate and nutrient uptake related changes are discussed.

Technical/language comments:

Line 234: "taken" should be "took"

Line 571: should say nutrient-replete

Line 598: On the other hand to what? Please rephrase

Line 620: type: "a" is missing

Fig. 1: Please indicate in the legend that experimental steps were done in a consecutive manner. Also this might be helpful to mention again in Fig S1. Visually this implies that the steps are done in parallel, but in the methodological description they are explained as being done one after another.

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