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Biogeosciences Discussions

Interactive comment on "Population-specific responses in physiological rates of *Emiliania huxleyi* to a broad CO_2 range" *by* Yong Zhang et al.

Anonymous Referee #2

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GENERAL COMMENTS

The paper by Zhang et al. presents results from a large number of experiments on multiple geographically distinct strains of the coccolithophore Emiliania huxleyi. Each strain was exposed to a wide range of pCO2 concentrations and the authors examined differences in growth rates, photosynthetic rates (POC production) and calcification rates (PIC production). The authors conclude that significant variability exists in population-level sensitivity of physiological rates (most clearly growth and POC production) to pCO2. The paper is well written, with the data supporting the conclusions and the authors make some important and insightful conclusions. I have only two minor comments.

The first comment relates to a lack of any discussion or presentation of the variability

in PIC:POC ratios and POC (or PIC) production between the different strains. Further information on the level of inter-strain variability in these parameters would strengthen and support the wider implications and conclusions made in the discussion. The second comment relates to the authors consideration of variability and stability in the different environmental conditions of the strain isolation locations – a large factor in these differences is likely to relate to different seasonal cycles and environmental drivers (ice-melt, riverine input, upwelling, etc). However, the authors only hint at the different factors influencing the relative stability of the different locations. Large-scale environmental differences will directly relate to the stability of the environment, as well as differing potential future perturbations for each of them. Again, making these differences more explicit would support the wider implications of the study.

SPECIFIC COMMENTS

Ln 27: Clarity is needed in the abstract on what the authors mean in terms of population-specific responses.

Ln 28: More information on number of strains per environment would be good in the abstract.

Ln 32: 'expected optimum curve responses' – may be expected by authors but not clear in the abstract. Some further background would be good.

Ln 37: Could the authors elaborate more in terms of the role of seasonality (or lack thereof) in the stability of oceanic conditions.

Lns 91-92: Would the authors consider adding 'geographically-distinct' strains to this line to emphasize both the importance of their own insights and the more general need to consider different strains of other widespread species.

Lns 103-104: A plastic response also allows a strain to acclimate across an environmental gradient and widen its bio-geographical distribution. Rather than focus on just environmental change, what about environmental variability.

Ln 126: How were all strains characterized and confirmed to be morphotype A (i.e. Distal shield length? Central area characteristics?)?

Ln 140-141: Is this statement ('the best compromise') appropriate based on the authors end conclusion that the low experiment temperature relative to optimum growth conditions for the Canary Islands strains led to their low growth (and POC production)? It seems to be a compromise that had a definitive influence on the end outcome of the experiments. Is it not simpler to just delete this section (from the point of 'which ..' to the end) and come back to this in the discussion?

Lns 152-153 (cf Lns 174-175): How were initial cell densities measured/estimated?

Lns 289-290: An important result that should be emphasized in the abstract and conclusions.

Lns 322-324: Suggest deleting 'causes' from this sentence.

Ln 351-352: Another potentially important conclusion, especially given the emphasis on determining time-dependent (or space-dependent) variations in coccolith-specific PIC quotas. However, the current paper lacks any details of the strain-specific variability in PIC quota and to what extent the different trends in pCO2-sensitivity (e.g. Fig. 3e) are driven by changes in growth rate and/or cellular (or coccolith) specific PIC quota. Can strain-specific information on PIC quota be added to the supplementary material to support this point with experimental data?

Ln 374: A two line conclusion seems relatively short based on the significant statements made in the conclusions. Either expand or delete?

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