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Interactive comment on "Environmental controls on the elemental composition of a Southern Hemisphere strain of the coccolithophore *Emiliania huxleyi*" *by* Yuanyuan Feng et al.

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Review on: "Environmental controls on the elemental composition of a Southern Hemisphere strain of the coccolithophore Emiliania huxleyi " by Feng et al. In this study, Feng et al., investigate the response of E. huxleyi (cell quota, organic, inorganic matter ratios) to different environmental drivers. Their findings are very interesting and relevant in the context of phytoplankton physiology under future ocean conditions. I have, however, one major and several additional concerns that should be addressed before their study is published. I hope this review helps to improve the manuscript.

MAJOR COMMENTS:

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Results: I am concerned about the “cellular content response (POC, PON, POP) to environmental drivers”. Organic matter quotas are strongly determined by the cell cycle. POC/cell, for example, will be much lower directly after cell division than right before. Thus, you can only compare cell quotas among treatments, when you are sure that all treatments were in the same cell cycle stage. The Authors do not indicate if samples were taken at the same time. This information would be a step forward because it could then at least be assumed that cell division was synchronized during night. However, even if sampling times were identical, it remains questionable if this assumption is valid for every treatment. Growth rates are not reported here but I assume that they are below μ=0.69, at least in some treatments (e.g. the low temperature treatment). If the cells divide less than once per day and only divide during night, it means that some cells of the population are packed with POC while others are depleted. Since you only sample once at an unknown cell cycle stage, it may become difficult (if not impossible) to disentangle the cell cycle-specific response from the actual treatment response. I therefore think that the results on cell quotas presented here (but also elsewhere in the literature) could potentially be misleading.

My suggestion would be to show production rates (μ x cell quota) instead of cell quotas. These also have theoretical issues but should more robust.

ADDITIONAL COMMENTS:

Page 2 Line 1: the “each” could probably be removed.

Page 2 Line 1: perhaps remove “cellular” because PIC is extracellular.

Page 2 Line 2: “implications for coccolithophore biogeochemistry”. This is a rather vague formulation. What is coccolithophore biogeochemistry? Do you mean the influence coccolithophores have on biogeochemical cycles? I think a bit more precision would improve the final sentence? Do you mean their influence on the nutrient cycle? Carbon export?

Section 2.1 provides a thorough description of the culturing methodology. One crucial information should be added, however. Were all samples taken at the same time (within an appropriate time window, e.g. $\sim$2 hours)? This is important because cell quotas change over the day and these can only be compared when all treatments were in the same cell cycle state when sampled (see also MAJOR COMMENT).

Page 6 Line 5: Agreed but a reference for this statement would probably be useful.

Page 12 Line 10: "with lowest nitrate and phosphate concentrations of 3.6 and 0.4 μM, respectively". In this case, your results may not really be comparable to Paasche's and others. Your nutrient concentrations were not leading to zero growth whereas those of Paasche et al were.

Page 13 Line 26: check spelling of "cell".

Page 13 Line 26: It is unclear in this sentence whether you measured cell size or you refer to earlier results. Please clarify.

Page 15 Line 1: This final speculation in the temperature section is a bit too extreme. It became clearer during the last couple of years that extrapolations from the (monoclonal) bottle to the global ocean should be avoided since way too many factors (e.g. ecology) are neglected.

Page 15 Line 9: "In general, cell growth of E. huxleyi is less limited by low CO2 concentrations than in other phytoplankton groups (Clark and Flynn, 2000; Paasche et al., 1996; Riebesell et al., 2000a)." This statement implies that E. huxleyi would have a particularly efficient CCM but is this supported by the evidence provided in the cited references? I suggest to check the MIMS-based papers by for example Björn Rost's group because these provide K1/2 values for carbon uptake and they have investigated quite a number of different species that can be compared with E. huxleyi.

Page 15 Line 27: I do not understand where the "both" is referring to.

Page 15 Line 27: "ecological implications"? Do you mean "biogeochemical implica-

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tions”?

Page 16 Line 2: Confusion: The 14C measurement is not referring to your study, or is it? 14C measurements have not been described in the methods or did I miss something?

Page 16 Line 5: Reference missing in the reference list. (Please check the entire list since there some others missing as well).

Page 16 Line 25: Semicolon

Page 17 Line 16: “. . .future research on a full environmental matrix is still necessary.” It would be valuable to add that the goal of such a matrix should not be to simply combine different factors and then use the outcome to extrapolate it to the future. The goal of culture studies should be to understand the underlying mechanisms of synergistic effects. For example: “How does the light intensity modify the temperature response and why?”

Page 18 Lines 14-17: I am not so sure about the final conclusion and the concomitant suggestion. If we design experiments to mimic anticipated physico-chemical conditions of the future as close as possible than the results can in most cases only be used to project findings from a culture experiment to the global ocean in a one to one manner. This, however, is questionable since many factors in that can significantly modify the outcomes are neglected in the experiment. Perhaps it may be more sustainable to suggest that experimentalists should design experiments in such a way that underlying mechanisms for synergistic effects can be understood.

Figure 7: Perhaps rather call it conceptual figure. Furthermore, were abbreviations “Q” defined in the text?

With kind regards, Lennart Bach

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