

Interactive comment on “Growth of the coccolithophore *Emiliana huxleyi* in light- and nutrient-limited reactors: relevance for the BIOSOPE deep ecological niche of coccolithophores batch” by L. Perrin et al.

Anonymous Referee #1

Received and published: 13 July 2016

Review of 'Growth of the coccolithophore *Emiliana huxleyi* in light- and nutrient-limited batch reactors: relevance for the BIOSOPE deep ecological niche of coccolithophores' by Perrin et al..

The manuscript addresses the deep ecological niche of coccolithophores found in nutrient poor oceanic gyres (like in the South Pacific) and the environmental conditions that control coccolithophore growth in this region. The study uses results from laboratory experiments and a model approach to define the growth response of *E. huxleyi* in regard to nitrogen, phosphorus and light limitation. *Emiliana huxleyi* represents one of

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the most studied phytoplankton species and the obtained results from the conducted laboratory experiments are generally confirmed by the existing literature. The model approach, however, is a neat approach to gain a better understanding of the dynamical growth response of *E. huxleyi* when transitioning from nutrient replete exponential growth to nutrient (or light) limited growth and to translate these findings to natural phenomena.

Personally, I was excited to review the presented manuscript, especially as the abstract is well written and clear. This positive first impression, however, was depressed after a complete evaluation of the manuscript. In general I have the impression that this manuscript did not go through co-author review. The manuscript would greatly improve by a complete re-evaluation of the scientific presentation style. Below I will provide comments and suggestions to convince the editor that this manuscript does not meet the scientific standards of Biogeosciences and to encourage the author to consider a new submission of the manuscript after a comprehensive revision.

Main comments:

1) The manuscript has currently 16 figures which is overwhelming and in my opinion not necessary. It distracts the reader from the essential message. I encourage the authors to think about merging certain figures (e.g Fig. 7 - 11). Also, some data are presented as figure and table. Maybe some tables could be moved to supplementary materials.

2) As previously mentioned, *E. huxleyi* is one of the most studied phytoplankton species and I assume that additional literature (besides Langer et al. 2013) data will be available to test the applied model.

3) The language of the manuscript needs to be improved. Some sentences are very long and thus hard to understand. Some expressions are not correct or I cannot follow the line of thought (see some examples below). In general many parts of the text could be condensed. For example, the sections 4.1.1 to 4.1.3 are only repeating the results

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previously mentioned and do not contribute to the discussion of the manuscript.

Additional comments:

- 1) Remove the period between units in the manuscript.
- 2) Use consistent colour and marker coding in all figures.
- 3) Line 18: The expression 'coccolithophore ecosystem' is not appropriate here. Maybe change to 'ecosystem for coccolithophores'.
- 4) L. 22-24: Avoid using 'physiology' and 'physiological' repetitive in one sentence.
- 5) L. 40-41 vs L. 44-45: Repetitive explanation of the connection between coccolithophore productivity and the influence on ocean-atmosphere CO₂ fluxes.
- 6) L. 113: The use of K/2 media in regard to nitrogen limitation studies may provide problems because both NH₄ and NO₃ are present as nitrogen source. Therefore, it is possible that cells switch from NH₄ uptake to NO₃ uptake when NH₄ is exhausted. This should be discussed.
- 7) L. 115: As cells were grown under batch conditions and allowed to enter the stationary phase (nutrient limitation), there is no possibility that cells are acclimated to the experimental conditions (of which nutrient limitation is one of the main factors).
- 8) L. 120: Light intensity is commonly expressed in 'umol quanta m⁻² s⁻¹'.
- 9) L. 140: At what point in time were the samples taken? Was it at the same time point for all treatments? When nutrient limitation starts in batch experiments the cellular physiology is changing in time as cells become nutrient starved. Please specify more clearly at what time point samples for volume, POC, PIC, etc. were taken.
- 10) L. 149: Here it is mentioned that coccolith width was measured but no data is presented.
- 11) L. 151: Please give an error estimate for the determination of the pH.

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- 12) L. 167: The conversion from particulate Ca to particulate organic carbon should be stated.
- 13) L. 193: Why was the C/N ratio after Redfield used when the actual C/N ratio was analysed in the laboratory experiments?
- 14) L. 199: The half saturation constant K_n is sometimes used for K_{nutrient} and sometimes for K_{nitrogen} . This will be very confusing for a non-expert reader. Later it is called ' K_n/p '?
- 15) L. 210: Change ' Q_N^{min} ' to ' Q_N^{max} '.
- 16) L. 277: If the stationary phase was not reached in the low light-Plim treatment, how can you be certain that the population was truly phosphorus limited? Is there the possibility that organic phosphate were available? *E. huxleyi* is known to be able to take up various forms of organic phosphates when inorganic PO_4 is limited.
- 17) L. 294: What is meant by 'were harder to discern due to large error bar in high light and nutrient-replete conditions'?
- 18) L. 379: The expression 'ecologically dominant coccolithophore' is not necessarily correct for *E. huxleyi*. Maybe change to 'numerical dominant coccolithophore'.
- 19) L. 381-382: Leonardos and Geider (2005) used a non-calcifying strain of *E. huxleyi* thus it is quite obvious that they did not measure PIC per cell or the PIC:POC ratio.
- 20) L. 387: Previously a DIC consumption of 12-13 % is mentioned.
- 21) L. 396-399: Not clear, please rephrase.
- 22) L. 452-458: Very long sentence, please split into several.
- 23) L. 469: 'Nutrient limitation as well...' . What is meant by 'as well'?
- 24) L. 527-529: I cannot follow the line of thought.
- 25) L. 529-531: This is contradicting to the general observation of *E. huxleyi* blooms in

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well stratified and sunlight ocean layers.

26) L. 550-554: This sentence is confusing. How is the reported observations of nutrient concentrations related to the phosphate limited experiment?

27) L. 557-559: Needs language check.

28) L. 572-574: Maybe the author want to mention and discuss the possibility of grazing and vertical export.

29) L. 585-590: Very long sentence.

30) L. 610-614: It is not really a new finding that batch experiments can be used to determine physiological parameters such as half-saturation constants and maximum uptake rates.

31) L. 679: Why is the BG Discussion paper of Beaufort et al. cited instead of the final and revised version?

32) Table 2: Why reporting PON:POC instead of the commonly reported POC:PON? This would facilitate a direct comparison to Redfield. Use decimal point instead comma.

There are many more points that need to be addressed. I only gave some examples and it cannot be the reviewers task to invest a considerable amount of time to discuss and mention all. I encourage the authors to invest the time to bring this manuscript in a more presentable format which will certainly facilitate a further review process.

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