

Interactive comment on “Competition for inorganic and organic forms of nitrogen and phosphorous between phytoplankton and bacteria during an *Emiliana huxleyi* spring bloom (PeECE II)” by T. Løvdal et al.

Anonymous Referee #2

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The study by Løvdal and co-workers measures uptake and turnover of inorganic and organic N and P substrates in two size fractions (considered to represent bacteria and phytoplankton) over the course of a phytoplankton bloom in a mesocosm experiment. The data obtained by Løvdal et al. demonstrate a high degree of temporal variability in nutrient uptake rates and turnover times reflective for the changing nutrient availability during the build-up and decline of the plankton bloom. They also show substantial differences in organic and inorganic nutrient utilization and uptake kinetics between the two size fractions. The results of this investigation corroborate earlier studies that bac-

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teria acquire organic N and P substrates more efficiently than phytoplankton, whereas they are less effective than phytoplankton in inorganic nutrient utilization.

General comments: A difficulty in the interpretation of the data lies in fact that due to the high abundance of particle associated bacterial the two size fractions (0.2-0.8 μm and $>0.8 \mu\text{m}$) do not allow a clear separation between bacteria and phytoplankton. To account for this, the authors correct for the contribution of particle-associated bacteria to the $>0.8 \mu\text{m}$ size fraction. A crucial assumption for this is "that particle-associated bacteria had the same affinity for uptake as free bacteria." (page 3353, lines 4-5). Given the large difference in bacterial size between free and attached bacteria, and given the differences in substrate quality and quantity that the two groups are likely to experience, this assumption is questionable. For the same reasons, it is also unlikely that temporal changes in nutrient uptake affinities were the same for free and attached bacteria throughout the experiment. Obviously, assumptions regarding the uptake affinity of particle-associated bacteria also affect the calculation of phytoplankton uptake rates. This approach greatly compromises the robustness of the results reported here.

To test for the uncertainties associated with the calculations it appears crucial that the authors perform a sensitivity analysis by applying a range of uptake affinities for the particle-associated bacteria. In the light of such a sensitivity analysis, some of the conclusions drawn from this data set may have to be qualified.

I agree with referee #1 that it would be beneficial to put more emphasis on primary data, thereby avoiding uncertainties associated with critical assumptions required in the calculations of derived (secondary) data sets. It would also be helpful to include graphical representations of nutrient concentrations and possible phytoplankton cell counts. Although this manuscript is considered to be included in the special issue on the PeECE experiments, aside from one other study it will be the only paper relating to the PeECE II experiment.

Specific comments (in addition to the ones already listed by referee #1):

1. P. 3347, line 25: Include year of the experiment (4 to 24 May, 2003).
2. P. 3348, line 1: For basic experimental setup also include reference to Engel et al. 2005 (Engel, A., Zondervan, I., Aerts, K., Baufort, L., Benthien, A. Chou, L., Delille, B., Gattuso, J.P., Harley, J., Heeman, C., Hoffmann, L., Jacquet, S., Nejstgaard, J., Pizay, M.D., Rochelle-Newall, E., Schneider, U., Terbrueggen, A., and Riebesell, U.: Testing the direct effect of CO₂ concentration on a bloom of the coccolithophorid *Emiliana huxleyi* in mesocosm experiment, *Limnol. Oceanogr.* 50, 493-507, 2005)
3. P. 3349, lines 12-14: Phytoplankton C, N and P biomass was calculated from Chl. *a* measurements using constant ratios of C:Chl. *a* (30:1) and of C:N:P (106:16:1). It is unlikely that these ratios remained constant over the course of the phytoplankton bloom (with both C:Chl. *a* and C:N:P likely to decline as inorganic nutrients become limiting). To what extent does the assumption of constant ratios affect the results of this study? Could this uncertainty be avoided by making use of measurements of phytoplankton cell abundance, POC, PON and POP, in addition to the Chl. *a* measurements used here? All of these data sets were obtained and are available for the 2003 PeECE experiment.
4. P. 3349, lines 15-19: Are the authors implying that "bacterial + algal C-biomass" to Chl. *a* ratio was constant during the course of the bloom? I think this is highly unlikely (see also comment above).
5. P. 3350, lines 5-7: "When the ambient NH₄⁺ concentrations were below the measurement limit, unlabelled NH₄Cl was added to a final concentration of 0.5 μmol L⁻¹ 1 to 4 h before trace addition." To what extent could this have affected the observed nutrient uptake kinetics (e.g. through acclimation to the enhanced nutrient availability)? Also, because the measurements obtained with nutrient additions represent "potential" rather than "actual" uptake kinetics, it should be clearly indicated in the manuscript which part of the data set was obtained with nutrient additions.
6. P. 3354, lines 22-24: Considering that the mesocosm experiment was started with

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post-bloom water from the fjord which was rich in dissolved organic matter (with DOC concentrations generally $>100 \mu\text{mol L}^{-1}$), wouldn't C-limitation of heterotrophic processes be rather unlikely during this phase?

7. P. 3355, lines 2-3: At a time of high inorganic nutrient concentrations (beginning of the experiment), the ratio of DIN:SRP should not be regarded as a reliable indicator for phosphate limitation.

8. P. 3357, lines 4-5: As above, the DIN:DIP ratio in itself is not sufficient as an indicator for nutrient limitation.

9. P. 3357, lines 13-16: see comments above.

10. P. 3358, lines 23-26: sentence appears incomplete (ending on "...to achieve.").

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