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Interactive comment on “CO₂-induced seawater acidification affects physiological performance of the marine diatom *Phaeodactylum tricorutum*” by Y. Wu et al.

Anonymous Referee #1

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Review of ms CO₂-induced seawater acidification affects physiological performance of the marine diatom *Phaeodactylum tricorutum*.

The ms addresses the timely topic of effects of increased CO₂ concentrations in seawater on a diatom. The ms is well organised and the discussion is generally supported by the experimental results. Th ms would benefit from more details in the material and methods section, and a less speculative discussion.

Specific remarks: line 2, p. 3855: is there also “non CO₂ induced ocean acidification “?

20 generations acclimation: with the reported growth rates, that would be about 10

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days. Is that really sufficient for acclimation to the changed CO₂ conditions?

The authors use $K_{1/2}$, better would be to use the generally accepted K_m or K_s : half saturation value for uptake or growth.. And why indicate (line 10, p. 3855) as “photosynthetic” affinity, it is simply affinity. In general the ms has a rather high “cliff-hanger” contents: some parameters are stimulated, some reduced, and the result...., could be different (a balance, line 18 p. 3855). So, what will it be ? Also it is confusing that on the one hand it is indicated that growth (net or gross ?) increased, but that the balance could be positive or negative. If growth (net ?) is stimulated, than obviously the balance is positive (stimulating). So, in other words, is it not clear that productivity will increase ?

Lines 15, p 3855, “Increasing... torespiration” (line 18), is a repetition of the previous sentences. Delete or make shorter.

P 3861 line 19: use half saturation constant (K_m) rather not affinity

Why report CO₂ in Pa? And not in μatm or $\mu\text{mol.kg}$?

Why was CCM not measured ?

p.3858 line 9:“automatic system for DIC measurements”, specify.

NBS standards are used for pH measurements. This will give an certain offset in the calculation of CO₂ speciation. How much ? It seems that the nutrients were added as nominal additions. Was nutrient draw down measured during the experiments ? If so, please specify. If not, how did the authors correct for changes in nutrient concentrations (needed in CO₂ speciation calculations) ? Nutrient draw down will affect alkalinity. And it is likely that nutrients were removed: 20000-30000/ml cells are capable of doing that. Please provide details on this.

The discussion is sometimes highly speculative, for example p 3864 lines 1- 12 is full of: “estimates”, roughly’s”, “would allow”, “would lead”, “would increase” . . . etc. Delete or be more specific.

The authors refer to Riebesell paper (and others) (p 3865, line 6) to indicate that negative effects on calcifying *E. huxleyi* under increased CO₂ conditions (contradiction the results with *P. tricornutum*), but leave out for example the Iglesias Rodrigues reference, supporting the present findings of stimulation of growth/productivity of *E. huxleyi* under high CO₂ conditions.

The scale of Fig 1 is inappropriate, better give a smaller range, allowing better insight in the differences.

Fig 2: n (number of analyses) is indicated as 3- 12. Be more specific, what was the exact number for every average ?

Interactive comment on Biogeosciences Discuss., 7, 3855, 2010.

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