

Interactive comment on “Modelling carbon overconsumption and the formation of extracellular particulate organic carbon” by M. Schartau et al.

Anonymous Referee #2

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General comments This manuscript deals with algal carbon over-consumption and its impact, through coagulation of carbon exudates into TEP, on the biological pump. The authors use a model that is fitted to a mesocosm experiment. The fits are impressive and achieved using state-of-the-art techniques. The paper is very well written and illustrated and of general interest; there is little to add, except a few minor comments.

Specific comments

A. The data assimilation technique.

Bootstrapping is a computationally expensive way of generating probability density

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functions of the parameters. Whether you have good estimates of these pdfs based on just 10 minimizations could be questioned. It is much more efficient in general to use markov chain monte carlo methods. They have their own problems, but they lead to a much better sampling of the pdfs, and therefore allow to see co-variation amongst parameter sets.

Page 21, to go from formula (1) to (2), the probability of the parameters is assumed constant. In reality, $\text{prob}(p/H,l)$ is 0 beyond the imposed range, and has a constant value within the range, so it is not really constant.

If higher accuracy can be achieved by gradient optimization techniques (p 25) and if the uncertainties in the optimization are reflected in the bias (p 24), then I do not understand why the genetic algorithm was not followed by one of these gradient-based methods. The GA then takes the model in the vicinity of the minimum and the gradient-based method then exactly locates this minimum.

The notion of 'bias' in the derived parameter values, and what you mean to derive from that is unclear to me. I would expect to find non-zero 'bias' if the resulting pdf is not a normal distribution? (but clearly it is not that simple).

Why was independence of data errors assumed? The experiments consist of several time-series (replicates), so data error covariance could have been estimated; I would expect to find at least temporal autocorrelation \checkmark

B. The algal model.

1. The changing N:C ratio in the algae reflects luxury uptake of DIN, and consequently, the model uses C-specific rates, as carbon represents functional biomass. Another strategy taken by algae to overcome shortages in light or nutrients is the luxury accumulation of C (e.g. as starch), in which case rates are nitrogen based. Were there particular reasons why the luxury uptake approach was chosen?

2. It is questionable whether the minimum and maximum cell quota of algae is indeed

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so well constrained (p 24): the Q_{max} of 0.171 gives a minimal CN ratio of 5.85. The overestimation of C:N values (Fig 4d) at the end of the nutrient replete phase (day 8-16) suggest that this minimal CN ratio may have been too high. 3. It remains unclear to me why the formulation of the maximum nitrate uptake rate was altered. The smoothness reason does not seem to hold, as the maximum nitrate uptake rate may now indeed be smooth at the minimal quota, but it is still abrupt at the maximal quota. Also, other formulations are still non-continuous (e.g. Rphot).

4. In unbalanced growth formulations the term “algal growth” becomes confusing, as algae may increase in C biomass but not in N contents, or vice versa. It is better to use either carbon assimilation or nitrogen assimilation.

C. Other model formulations.

1. Heterotrophic respiration on page 47. This formulation for heterotrophic respiration may work if the CN ratio is larger than Redfield, but as far as I can judge, it will provoke DIC uptake (autotrophy !) otherwise ? (excretion should be triggered in these cases).

2. As you also include an alkalinity model, the nature of DIN becomes important as alkalinity and pH are affected differently by ammonium and nitrate. Clearly you consider only nitrate; you could be explicit about that.

D. Model fits.

The overestimation of DIC at the end of the experiment is attributed to the importance of the heterotrophs, which may be over-estimated. However, it seems to co-occur exactly with the exhaustion of DIN, so an alternative explanation could be that it is caused by the underestimation of carbon over-consumption then? Also, the model fits the net community production and photosynthesis well at that period, suggesting that the heterotrophic compartment was not overestimated then.

And finally

There is one paper that deals with a model of carbon overconsumption in mesocosm,

but that ignores TEP formation, however includes the bacterial loop; perhaps the authors may want to consult it (Van den Meersche, K. et al.(2004). Carbon-nitrogen coupling and algal-bacterial interactions during an experimental bloom: modeling a ^{13}C tracer experiment. *Limnol. Oceanogr.* 49(3): 862-878)

Technical issues

Formula A6, on page 45: \tilde{E} - degradation of TEPC.

Sentences I did not understand 1. p. 20. “ \tilde{E} Additional phytoplankton N and C losses were associated with cell lysis due to bacterial activity “ you mean bacterial activity or viral activity (?) 2. p. 30. “the POC:PON ratio does not reveal any luxury consumption of nitrogen during the growth phase under DIN replete conditions”. So why does the C:N ratio decrease then ?

Tables. Table A1. How were the standard deviations from table A1 estimated? I read $.1^2 + \sigma$ for some, $2^2 + \sigma$ for other, etc..

Table 2. Units of 1 ? (you mean dimensionless, -)

Table 3. Bias estimates depend on the units of parameters and are therefore difficult to compare; I would prefer to see them standardized (e.g. $(p_{\text{best}} - p_{\text{mean}})/p_{\text{mean}}$).

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