

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

**M. Schartau et al.**

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### **1 [Author’s response to comments of referee #1:](#)**

The referee’s comments are vital and helped us to better understand the limitations in our modelling approach. The referee has one central issue that has not been sufficiently discussed in our manuscript: it is questioned whether the assumption of biomass-proportional exudation of polysaccharides is appropriate. Furthermore, the referee worries about variable exudation of polysaccharides that remains unresolved in our model, making our results not fully conclusive.

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In order to gain insight to variations of rate and quality of DOC production during the mesocosm experiment, we decided to follow some of the referee's suggestions and performed additional optimisations. We first assimilated data of the growth period into the model, which was followed by optimisations where solely post-bloom data was regarded. With information from these additional runs, we are able to better interpret our model limitations with respect to DOC loss by phytoplankton. These findings entered our revised manuscript. We greatly appreciate the referee's constructive comments on this issue.

## 2 Response to specific comments of referee #1:

### 2.1 Description of prior information

The description of the prior parameter information in Equation (1) that enters the cost function is incomplete. This arises from the fact that we derive the cost function before the optimisation algorithm is described. As also stated by referee #2, the prior is not truly a flat prior but a uniform prior within prescribed bounds (upper and lower limits of the parameter vector space). The prior beyond these bounds is zero. We thus exclude possible values beyond the prescribed range of variation. The prior, however, remains constant within these bounds and the probability is proportional to the inverse of the number of increments (set by the number of binaries in Table A2) provided to the optimisation algorithm ( $\mu$ GA).

Referee #1 suggests applying a Jeffreys' prior. This prior has been proposed by Jeffreys (1961) for scale parameters that lie anywhere between zero and infinity. The situation is different in our case in that we prescribe intervals of finite size for the model parameters. The interval limits are based on prior knowledge and we feel that

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a constant prior for the interval (and zero outside) is more appropriate than a Jeffreys' prior that puts more weight on smaller parameter values. We must then also expect greater uncertainties with larger parameter estimates. Of course, for many other optimisation problems (in particular in conjunction with gradient search techniques) the use of a Jeffrey's prior does become essential.

We corrected the description of our prior information, stressing that our prior is a uniform prior within prescribed limits.

## 2.2 Model assumption on PCHO release by phytoplankton

The referee questions whether a biomass-proportional production rate of polysaccharides is a good assumption, given the variability (non-linearity) seen in the data. We realised that this issue is insufficiently stressed in our manuscript, even neglected in the discussion section. The revised version of our manuscript now includes this important issue. We agree with the referee in that our assumption is crude, but we preferred simplicity over a more complex parameterisation. On page 7, line 18, we explained that we are not able to distinguish between leakage, cell lysis, and exudation. This has now been stressed in the revised manuscript.

With the data available, we believed that we are not able to constrain a (seemingly) more sophisticated description of active exudation. This we concluded from preliminary model studies, while testing a sigmoidal function for exudation. With the switch we introduced two unknown parameters. The switch turned on polysaccharide exudation once a critical cell quota (first unknown parameter) was reached. The steepness of the sigmoidal function was the second unknown parameter. The model behaved well with the switch, but we had no obvious improvement in model performance (with tiny

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differences in model trajectories). Alternative descriptions for active exudation are proposed in Van den Meersche et al. (2004) and in Anderson and Williams (1998). In both studies, exudation is separately handled from passive leakage of DOC, mainly by assuming a mechanism that regulates exudation through the difference between saturated and actual growth conditions. This assumption is reasonable. However, we compared our switch formulation with the simplest approach with biomass-proportional polysaccharide exudation and learned that we have actually no data in hand to constrain the separations between leakage, lysis, and exudation. For this we would have needed either explicit polysaccharide measurements or information about the residual DOC. We therefore decided to consider the simplest version and rather focus on the fraction of all DOC loss by phytoplankton that can be identified as PCHO.

We split up the data-assimilation experiment into two parts, into a growth phase and a post-bloom period respectively. We varied four parameters relevant for TEPC formation: carbon loss rate (leakage/exudation) by phytoplankton, fraction of polysaccharides of DOC loss, and the parameters associated with TEP coagulation. Variations of the entire parameter set seem meaningless to us, because growth parameters, e.g. that are strongly linked to nitrogen mass flux, will remain unconstrained when assimilation data are taken solely from the post-bloom period. We expect to learn something from separated data-assimilation experiments only when we can rely on a best fit, provided by the assimilation of all data (as it is done in sensitivity analyses). We present our results in a new paragraph added to the sensitivity analyses section (page...). From the additional experiments we learned that the overall loss rate of DOC by phytoplankton remains constant whereas the fraction of polysaccharides increases from 34% during the bloom phase to 63% during the post-bloom period. This indicates a qualitative change in organic carbon loss by phytoplankton, with twofold higher polysaccharide exudation at the end of the bloom than during bloom conditions. These results are now in support of applying a distinguished parameterisation for exudation (something we did not identify before because we could not rely on the prior information that we obtained

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from the optimisation procedure with all data). In the revised version of the manuscript, we stress that a mechanistic understanding of the qualitative changes in DOM exudation by phytoplankton is lacking but is needed in order to refine model descriptions for TEP formation, and the attachment probability of phytoplankton aggregation.

### 2.3 Parameterisation for TEP formation and aggregation between phytoplankton and detritus

Referee #1 is unconvinced that a division into two particle size classes can be sufficient to describe complex aggregation dynamics. Indeed, with our results we are not able to assert that the aggregation between phytoplankton and detritus can be described with a two classes approach. This is mentioned in our manuscript and discussed. On the other hand, our results do foster a two classes approach when it comes to parameterising extra-cellular TEP formation. Our estimates of the product between collision kernels with attachment probabilities are in fair agreement with values applied in another study (Engel et al., 2003), where a different, independent data set was used. Thus, our modelling study is conclusive in terms of the applicability of the two-class parameterisation in conjunction with a dynamical phytoplankton growth model, but also in terms of first estimates of the corresponding parameters.

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