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Biogeosciences Discussions

Interactive comment on "Feedbacks of CO_2 dependent dissolved organic carbon production on atmospheric CO_2 in an ocean biogeochemical model" *by* L. A. Bordelon-Katrynski and B. Schneider

Anonymous Referee #2

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1 General comments

The manuscript "Feedback of CO_2-dependent dissolved organic carbon production on atmospheric CO_2 in an ocean biogeochemistry model" by L. Bordelon-Katrynski and B. Schneider is a contribution to the important question how changes in ocean biogeochemical cycling induced by rising atmospheric $p$$CO_2$ feed back on the ocean-atmosphere carbon cycle. The specific feedback that they study here is an increased excretion rate of dissolved organic carbon (DOC) that has been hypothesized to ex-

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plain findings from a mesocosm experiment (Riebesell et al., 2007): Here it was shown that increasing pCO_2 led to an increased rate of carbon drawdown relative to that of nitrogen, while the particulate C:N ratio remained unchanged. The hypothesized explanation was that the additional carbon taken up was routed into DOC. Besides thus changing the overall stoichiometry of organic matter this pathway has the potential to change particle sinking rates in the ocean, as part of the DOC pool, acidic polysaccharides, tend to aggregate and to also enhance aggregation of other particles into larger faster-sinking particles.

The authors study this feedback with a global ocean biogeochemical model with which they perform experiments where the excretion rate of DOC from phytoplankton changes over time following a simple relationship to the expected atmospheric pCO_2 increase. They compare this to a model experiment where the excretion rate stays constant over time; the difference between the two runs can then be discussed as follwing from the DOC feedback. Both runs are also performed with and without a parallel increase in pCO_2, so interactive effects can also be investigated.

The consequences of CO_2 induced changes in C:N stoichiometry have already been investigated before with a similar model (Tagliabue et al., 2011). However, the detailed assumptions how the additional carbon taken up is routed into dissolved or particulate biomass differ between the two studies, as are some key findings (increased vs. decreased particulate export under elevated pCO_2). The authors cite this as evidence that the sign of the feedback depends on the actual pathway the extra carbon is taking.

The general question of the manuscript concerning the magnitude of the DOC feedback as well as the discussion on how it depends on the way that they are implemented in biogeochemical model are worthwile additions to the field. However, I have a problem with the assumptions that the authors made in parameterizing the pCO_2-DOC feedback, a problem that was also shared by the other reviewer, and that — I think — also determines the outcome of the study in such a way that it cannot be interpreted meaningfully. Much to my regret I therefore cannot recommend to publish the study in

Biogeosciences, although, apart from the way of parameterization the whole study is methodologically well done and the manuscript is well written.

My problem is the following. The whole point in the mesocosm experiments by Riebesell et al. (2007) was that increasing the availability of CO_2 to phytoplankton led to an increased photosynthetic production of organic carbon by phytoplankton without an parallel increase in nitrogen uptake; and it seemed as if most of the excess cabon taken up was routed into dissolved organic carbon, thereby increasing the C:N ratio of dissolved organic matter (DOM). The model used in the present study, however, uses a constant C:N ratio both for particulate (phytoplankton/zooplankton/detritus) and dissolved organic model compartments. At least this is what I took from the references that the authors give for their model, I have found no explicit statement on the C:N ratio in DOM in their manuscript. But there are several clear indicators that the C:N ratio in DOM is indeed fixed, e.g. the shallower depth of nitrate remineralization (p. 7991, l. 20-21). In a model with fixed C:N an increased relative excretion of DOC must be accompanied by an excretion of dissolved organic nitrogen, and thus by a reduction in the production of phytoplankton biomass. As the supply of inorganic nitrogen is what is often limiting the possible formation and sinking of biomass (at least in models) it is therefore no wonder that the authors observe that the increased formation of DOC is at the expense of the formation of POC rater than fostering particle aggregation (p. 7991, l. 11-15). So I would argue that

1. the main model result of decreased particle export is at least partly built into their model assumption, namely that the feedback operates through increased relative DOC excretion at fixed C:N ratio, and that
2. this assumption is inconsistent with the findings in the mesocosm experiments cited. Indeed I do not see a physiological reason why an increase in seawater $p\mathrm{CO}_2$ should lead to an increase in organic carbon and nitrogen excretion *at the expense of cell growth*, while there are good physiological reasons why an in-

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creased CO_2 supply could lead to an increased carbon fixation (e.g. the often low affinity of the carboxylating enzyme RuBisCO), and why excess carbon fixed without additional nitrogen uptake would lead to the formation of high C:N biomass, such as sugars.

My summary of the paper is thus that it is a valid sensitivity study for a physiological effect that does not exist. I think the authors could remedy this by allowing the stoichiometry of DOM to vary, while keeping that of the partulate biomass constant. That would, however, require to re-run all model runs, in effect it would be a new study.

I must say that, given the otherwise good methodological quality of the study, I would be happy if the authors could come up with convincing arguments why I am wrong and their study is meaningful, but at the moment I do not see any.

Interactive comment on Biogeosciences Discuss., 9, 7983, 2012.