**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

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1 General comments

1.1 Summary

In the study of Bordelon-Katrynski and Schneider the PISCES model (model of **P**elagic **I**teraction **S**cheme for **C**arbon and **E**cosystem **S**tudies) is applied to compute the po-

tential impact of carbon sequestration on global scale, assuming increased dissolved organic carbon (DOC) exudation along with a rise in atmospheric carbon dioxide (CO_2) concentration. Results of this specific model setup are compared with results of other model configurations, so that the computed impact of their assumption can be isolated from other (physical-chemical) CO_2 effects. Their model simulation with CO_2-sensitive exudation yields a positive feedback on atmospheric CO_2. This result is contrary to results of Tagliabue et al. (2011), who simulated enhanced export flux with rising CO_2 concentrations. The study of Bordelon-Katrynski and Schneider not only complements but also questions the generality of the model outcome of Tagliabue et al. (2011). In their conclusion they state that the sign of the CO_2 ocean-atmosphere feedback depends on the pathway of excessive carbon uptake.

1.2 **Impression after review**

I fully agree with the author's notion that magnitude and sign of the feedback depend on the possible pathway chosen for channeling the additional (excessive) carbon uptake. But their conclusive remark does not really help when elucidating global CO_2 flux estimation. It does help, though, to assess alternative model solutions. In their study, the authors intend to provide an alternative solution to Tagliabue et al. (2011), which is desirable and helpful. The question is whether the pathway for excess carbon uptake is plausible or not. The authors have not critically assessed details of their approach and have not discussed the implication of their modified PISCES equations. In the following I will explain why the alternative model setup is implausible. I will also outline the sensitivity to expect from those model equations that describe the dynamics of primary interest: the critical linkage between DOC exudation and export flux of particulate organic carbon (POC) in PISCES. Much to my regret, I must not recommend their study for publication.

2   Specific comments

2.1 CO_2 induced enhancement of DOC exudation, given a constant carbon-to-nitrogen stoichiometry of phytoplankton and particulate organic matter (POM) export

The author's describe DOC exudation as a proportion (5 %) of net primary production (NPP) in the control run and then infer a transfer function to account for an CO_2 induced increase of exudation to match 20 % of NPP after 140 years after spin-up. The reasoning in taking such an approach can only be understood as to maintain NPP similar to the control run, while all excessively fixed carbon (C:N assimilation ratio higher than 122:16) enters the DOC pool directly. It implies that gross primary production (GPP) and exudation increase in equal amounts in order to have similar NPP values; we have $\triangle$GPP = $\triangle$exudation to keep NPP = GPP - maintenance respiration - exudation $\approx$ constant.

So far, it seems to be a smart and fair approach. The additional DOC can then either be respired or is exported via 'aggregation'/adsorption of DOC to detrital and sinking POC (according to equations 4 in Gehlen et al., 2006). In Table 2 we see that after introducing the additional carbon into the system we find an increase (8 %) in NPP together with an increase in surface nitrate concentrations. Ocean mixing and advection are identical. But why do results of NPP change then? The authors wrote: "*The changed DOC cycling also affects nutrient distributions with a tendency of shallow water nitrate accumulation, which reduces the vertical NO_3 gradient. Especially around and directly below the subtropical gyres NO_3 accumulates under higher DOC formation (Table 2, Fig. 2e, f).*" Changes in NPP are attributed to redistribution, sinking and mineralisation of organic matter. Yes, the formation and sinking flux of POC is altered by an increase in DOC exudation, as defined in equations 4 in Gehlen et al. (2006). I see, however, a problem in that "additional" carbon can enter the two sinking pools (small and large

particle aggregates) but the POC pool is then mineralized with a constant N:C ratio of 16:122. The C:N:P composition of dissolved and particulate matter in PISCES is tied to a constant stoichiometry. Therefore, DOC added to the model system introduces an additional source of nitrogen during mineralisation. This is a severe model deficiency, because on the times scales considered here it has an affect on the processes of interest. This situation seems similar to the 'PP-DOC' model run depicted in Tagliabue et al. (2011).

2.2 Distinction of labile and semi-labile DOC and POC

The following model assumptions in this study are arguable:

a) *All DOC exuded by phytoplankton is semi-labile and exclusively consists of surface-active compounds that can adsorb to or aggregate with POC (equations 4 in Gehlen et al., 2006).* It is difficult to specify qualitative characteristics of the bulk (fresh) DOC pool in the field or laboratory. In a data-model synthesis we estimated changes between 34 ± 8 % (bloom period) and 63 ± 20 % (post-bloom period) of all freshly exuded DOC to consist of surface-active compounds (e.g. acidic polysaccharides, Schartau et al, 2007). These acidic polysaccharides may then act as precursors to form larger macro-gels, often measured as transparent exopolymer particles (TEP). Thus a residual fraction of fresh DOC (not refractory!) exists whose fate is undetermined. To impose that all freshly exuded DOC can form TEP is therefore inappropriate.

b) *DOC exudation increases with an increase in NPP*. This assumption is difficult to justify because many observations show maximum DOC exudation during post-bloom periods, at times when NPP (or relative growth rates) converge towards zero. It is rather a function of the imbalance between photosynthesis and cell growth.

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c) *Modelled POC:PON remain near Redfield stoichiometry under all growth conditions so that all excessive carbon uptake enters the DOC pool and can form TEP*. Data from the mesocosm experiment in Riebesell et al. (2007) show nearly unaltered POC:PON ratios. Their findings are extraordinary, but we have to consider that their hypothesis and interpretation of the experimental outcome had not been confirmed with DOC or TEP data explicitly.

2.3 The simulated POC export flux is sensitive to the amount of phytoplankton biomass that can be build up, which in turn depends on relative growth rate

Equations 4 in Gehlen et al. (2006) describe the crucial link between DOC and export of POC, the flux from DOC to POC is defined as $\Phi_1^{DOC} \cdot sh \cdot DOC^2 + \Phi_2^{DOC} \cdot sh \cdot DOC \cdot POC_s$. According to these equations the sensitivity of DOC transformation depends on simulated POC_s concentration. In Tagliabue et al. (2011) the POC within the upper layers must be higher than in this study, as global NPP is much higher (49 PgC yr^{-1}) compared to this study's reference run (30 PgC yr^{-1}). A difference in simulated POC must yield a different impact (sensitivity) on the amount of DOC that can be channeled to the model's pool of sinking POC. Provided that same parameter values for Φ_1^{DOC} and Φ_2^{DOC} were used in Gehlen et al. (2006), Tagliabue et al. (2011) and in this study, differences in DOC transformation to POC export can be attributed to differences in their simulated POC_s concentrations. The build-up of POC_s is one critical key to determine whether DOC is eventually exported or can reside in upper layers. In the second paragraph on page 7994 (discussion section) this issue is address roughly. The lower preconditioned simulated POC_s concentrations the smaller the effect on export and more "additional" DOC will reside in the upper layers, where it can be hydrolized and respired (a shallower turnover of organic matter, discussed in the second paragraph on page 7995).

2.4 **Suggestions for improvements**

Following the discussion section, differences in the sensitivity of DOC entering POC export flux had been sensed but were not analyzed further. I suggest that the authors look into details of this flux, explaining why some regions have an enhanced C-export whereas others reveal higher DOC concentrations within the upper layers. This analysis would give some insight to a critical POC_s mass to enhance DOC transformation and to eventually foster export flux. How large must DOC and POC_s concentrations be to enhance export flux of the "additional" DOC? I also think that the additional carbon uptake should be traced by an increase in the C:N ratio of exported particulate matter. This can possibly be achieved even when primary production is assumed to maintain a constant C:N:P stoichiometry. A sensitivity analysis of the pathways of excessive DOC production should also consider differences in global NPP. At which NPP (e.g. changing maximum growth rates) does the model switch from an overall positive feedback to a negative feedback? Finally, I strongly recommend to compute the nitrogen budgets (combining NO_3, NH_4 with the organic carbon mass converted to nitrogen with the assumed elemental ratio of 122:16) and control mass conservation of the global nitrogen budget.

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