

Interactive comment on “Nitrogen and carbon dynamics in the Scheldt estuary at the beginning of the 21st century – a modelling study” by A. F. Hofmann et al.

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In our paper “Nitrogen and carbon dynamics in the Schelde estuary at the beginning of the 21st century - a modelling study” we present a synthesis of the key biogeochemical processes that govern this complex heterotrophic system. To investigate the state of the system in the early first decade of the 21st century, our aim was to develop a model that is as simple as possible, yet reproduces the key features of the system correctly. This rationale implies a restricted set of modelled processes. The fit of the resulting rather simple model to field data from the years 2001 to 2004 gives confidence that the included processes are indeed the main driving forces in the rather complex ecosystem Schelde estuary.

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Nevertheless, there were some valid critical comments by Referee # 1 which inspired us to changes in our model and manuscript:

- As pointed out correctly, our decision not to include primary production into the model was indeed inconsistent with the inclusion of sulfate reduction which operates at even lower levels. Our idea, which we incompletely communicated in the discussion paper, was to model net mineralisation, i.e. mineralisation minus primary production. Considering this, the model underlying the discussion version of our paper remains a valid model. Yet, we took Referee # 1's advice and included primary production explicitly in our model, to be able to characterize the roles of primary production and mineralisation independently.
- Inspired by Referee # 1's comments about the marginal role of amongst others sulfate reduction in our model, we tried to make our model more compliant with our own philosophy: as simple as possible. Therefore, we removed all negligible processes from our model: sulfate reduction, sulfide re-oxidation, and ammonia air-sea exchange. Additionally, considering the fact that our piston velocity wind dependency formulation resulted in almost constant values over the whole estuary (see Figure 14 of the discussion paper), we removed some unnecessary complexity from the model by using a constant piston velocity.
- Furthermore, in the discussion version of our paper we did fail to make a proper point of the fact that the $p\text{CO}_2$ in the upstream Schelde significantly declined from the mid 90ies to our model time period (Middelburg & Borges, unpublished data) - explaining a large portion of the difference in CO_2 degassing estimates in our work and in the work of [Vanderborght et al.(2002)] who work with data from 1996 (and the work of [Hellings et al.(2001)] who also use data from the mid to late 90ies). We also failed to make the point that reduced nitrification rates result in higher pH values, lower $[\text{CO}_2]$ and thus less CO_2 degassing. These mistakes on our behalf are corrected in the revised version of our manuscript.

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However, there are further issues raised by Referee # 1 with which we do not fully agree:

- We do not feel that our work “retrogresses” from [Vanderborght et al.(2002), Vanderborght et al.(2007)]. Both of those models include the physical transport of solutes in the estuary in great detail ([Vanderborght et al.(2002)] describes a tidally resolved 1-D model and [Vanderborght et al.(2007)] a tidally resolved 2-D model). This rather complex description of physical processes leads to severe limitations of the models:
 1. there is a high demand for high resolution data which makes it difficult to run the model for past decades with scarce data coverage or for predictive future scenarios,
 2. the computational demand of the models is rather high, rendering it difficult to run the model for longer model times,
 3. to port these models to other systems detailed bathymetrical maps are needed which might not always be available,
 4. furthermore, the complex representation of physics together with a rather crude representation of biogeochemistry in the models of [Vanderborght et al.(2002)] and [Vanderborght et al.(2007)] implicitly puts emphasis on the role of physics for the estuarine ecosystem functioning, which might not correspond to reality.

Since our model suffers from none of these drawbacks, our model code is public domain (the model codes of [Vanderborght et al.(2002)] and [Vanderborght et al.(2007)] cannot be publicly verified), and our model uses more recent data, we feel that our paper provides a complementary contribution to the

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scientific literature. These issues are mentioned in the revised version of our paper.

- According to Referee # 1 we might overlook some important processes for a model with emphasis on the C cycle.

Our model resolves explicitly the carbon, oxygen and nitrogen cycles and thus allows us to make statements amongst others about cycling and degassing of CO₂. Consequently, we do not feel that our model puts special emphasis on the CO₂ budget.

- Referee # 1 mentions calcium carbonate precipitation as a potentially important process for C cycling in the Schelde estuary. We tried to include biogenic calcification as well as calcite and aragonite precipitation in the model. Although we used unrealistically high reaction rates for these processes, the model results changed only marginally (the amount of CO₂ degassing only changed by $\approx 7\%$). For this reason, to keep the model as simple as possible, and because parameter values are poorly constrained, we decided not to include processes involving the formation of CaCO₃ in our model.
 - [Middelburg et al.(2002)] report methane concentrations on the nanomolar level which is more than three orders of magnitude lower than concentrations relevant to our model (micromolar level). Thus, we do not include methanogenesis and methane oxidation in our model.
- Referee # 1 criticizes that there are not enough predictive scenarios and comparisons to other systems in our manuscript. The main objectives of our manuscript are
 1. to give a description of our model to allow for referencing it in future works
 2. to show the results of diagnostic simulations for the early first decade of the 21st century as an example for an application of the model.

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3. to give a short comparison with earlier studies of the Schelde estuary with respect to N, C, and O₂ to show decadal changes in the biogeochemistry of the Scheldt.

We are aware that

1. applying the model to past decades (i.e. the 70ies, 80ies, and 90ies)
2. applying the model to predictive future scenarios
3. using the model to investigate the influence of freshwater discharge on the biogeochemistry of the Scheldt estuary

certainly are interesting and useful applications of our model. Also comparing the model output to results from other systems, e.g. the Pearl River in China, for sure will be scientifically useful. Some of the mentioned points, along with the application of the pH modelling approach of [Hofmann et al.(2008)] to this model to shed light on the pH steady state in the Scheldt, are already work in progress. However, with only the mentioned main objectives reached, the current manuscript is already rather long. Thus we have decided not to include any of the latter items into the current manuscript. Contrarywise, we decided to shorten the materials and methods section, the results section, and the appendix considerably to make the manuscript more readable. The without any doubt interesting and scientifically useful further model applications and comparisons will be material for future publications.

However we do include predictive scenarios in our publication. We mention a run with all biology toggled off to assess the amount of riverine DIC that is ventilated in the estuary. We furthermore mention runs with scaled freshwater flow values to confirm the relation between a trend in freshwater flow and trends in several state variables of the model.

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