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***Interactive comment on* “From heterotrophy to autotrophy: a freshwater estuarine ecosystem recovering from hypereutrophication” by T. J. S. Cox et al.**

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Received and published: 21 August 2009

General answer to the reviewers

Both reviews of our manuscript made us realize that we have to be more precise and complete in depicting the context, uniqueness and novelty of our study. There are three main reasons why our study is unique.

First, the hypereutrophied freshwater Scheldt of the past, marked by extreme hypoxia/anoxia caused by organic pollution and high ammonia inputs and potential algal growth inhibition, represented a far more degraded situation when compared to other systems. Most other studies document recovery starting from the point where the

Full Screen / Esc

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Interactive Discussion

Discussion Paper



Scheldt is to date. To illustrate this, we have attached a conceptual plot (to be included in the revised manuscript), in which we discern four main stages in the recovery from hypereutrophication. Stage 1 is the hypereutrophied state, characterized by extreme high ammonia inputs with associated algal growth inhibition and hypoxia/anoxia. Stage 2, a transitional stage, represents the recovery from this hypereutrophied state towards the classical eutrophied state. The third stage represents the classical, often studied eutrophied state, with intense algal blooms due to excess inorganic nutrient availability. The final stage refers to the recovery of aquatic systems from eutrophication. Our study describes biogeochemical mechanisms and dynamics in the first two stages. Almost all studies on eutrophication, trend reversal and biomanipulation, deal with stages 3 and 4. This makes the freshwater Scheldt a unique case study area, although e.g. in China certain aquatic systems might be similar, now or in the future (e.g. Harrison, 2008).

Second, this is the first account of the existence of multiple steady states in a flow-through system. Research on non-linear dynamics has focused on enclosed systems in which the dynamics is governed primarily by internal processes and less by physical forcing. In such systems, the manifestations of multiple steady states are more obvious, whereas in flow-through systems that are strongly influenced by external forcings, shifts may be masked by (predictable) variability. In this case, we demonstrate that under the given hypothesis, the system will display (mathematically defined) alternative stable states, and we show how a corresponding shift in the estuarine functioning is partly masked by the seasonality of the drivers of the system.

Finally, we did not intend to perform a model study with a complex, mechanistic model. Rather we tried to capture the system dynamics with the simplest possible model. While we do have access to complex models (e.g. Soetaert and Herman 1995, Hofmann et al. 2008), a simple model is more instructive to understand the processes at play. The fact that this simple models' output corresponds remarkably well with observed data, without any parameter fine-tuning, represents a strong argument in favor

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of the proposed hypothesis and contributes to the novelty of this paper.

Specific answers to reviewer #1

We sincerely acknowledge referee #1 for her/his valuable comments and suggestions that made us refine some conceptual aspects of the text, and will enable us to revise and improve the manuscript.

[Major Comments, paragraph 1&2]

Referee #1 brings to our attention that more care has to be taken to use properly defined terminology concerning the status of our system. We are the first to admit that definitions of the metabolic status of estuaria are rather sloppy. We want to note, however, that most of the literature dealing with the trophic status of ecosystems is concerned with systems with two co-existing energy-sources: light and allochthonous organic matter. In that case, 'respiration' and 'production' can be defined equivalently in terms of carbon or in terms of oxygen: CO₂ fixation (production) always comes with oxygen production (consumption) and vice versa. Moreover, the carbon source (CO₂ vs. OM) is directly related to the energy source (light vs. OM). As such, a lot of sloppy wording can be found in the literature (e.g. conflating 'respiration' with 'oxygen consumption'). In the case where nitrification is important, these relations cease to hold: chemo-autotrophy is a sink of oxygen and carbon dioxide, consequently carbon fixation is no more synonym to oxygen production, neither to phototrophy. Therefore, incompatible definitions are found in the literature.

Although not the most common definition, we had deliberately chosen the definition of autotrophy/heterotrophy in terms of oxygen production/consumption from Garnier and Billen, 2007, to characterize the status of our system. This choice resulted in a light and compact formulation throughout our manuscript of the ecosystem status. We admit, however, that this might not be the best choice, all the more so because many readers will indeed be misled, as referee #1 mentions. We will change the wording throughout the text to make it more correct and in line with common definitions, and

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6, C1631–C1637, 2009

Interactive
Comment

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Interactive Discussion

Discussion Paper



avoid naming the different states 'autotrophic' and 'heterotrophic'.

[Major Comments, paragraph 3]

The referee further remarks that we 'state that the Schelde evolved to a situation where $P/R > 1$ '. This is, in fact, not the case: we systematically separate the model, that indeed display a clear autotrophic and a clear heterotrophic state (in the sloppy sense), from the Schelde which we claim to be 'increasingly autotrophic'. First the 'real' Schelde evolves toward steady state along the estuarine axis, and we do believe that it will be the closest to steady state at the downward boundary (figure 3). Due to seasonality, P/R in the real Schelde will only be larger than one at the moments of highest production (in summer). Finally, the situation is still changing, and nowadays it is common to measure for many consecutive months at different stations oversaturated oxygen concentrations. We will give these considerations more prominence in the revised manuscript.

[Major Comments, paragraph 4]

This also addresses the issue raised in paragraph 4 of the reviewer comments. The inclusion of OM degradation will not make the model significantly better than how it stands now. Organic matter inputs are decreasing, along with ammonia inputs. The inclusion of organic matter degradation will allow for a quantification of the differential contribution to the oxygen dynamics of OM degradation and nitrification, but will not change the qualitative behavior of the model, nor make it close to a realistic ecosystem model.

Unpublished oxygen uptake experiments with and without nitrification inhibitors on the freshwater Scheldt for the 1998-2003 period, show that on average nitrification in this part of the estuary accounts for about 50% of total oxygen consumption, with a maximum of 75%. It also shows a decreasing trend in time and along the estuarine axis. This is in line with similar data from Gazeau et al (2005) on the brackish and saline parts of the estuary, showing that nitrification accounted for about 25% of annually

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Discussion Paper



averaged oxygen consumption in 2003. They also show that the relative importance of nitrification increases towards the upstream reaches and shows a high intra annual variability. It is therefore plausible to assume that neglecting organic matter degradation (in comparison to nitrification) is a satisfactory approximation until the early 1990s. Probably more important later on, the additional oxygen consumption by OM degradation is implicitly taken into account by our model parameterization.

[Major Comments, paragraph 5]

We have chosen to show oxygen concentrations in stead of O₂ saturation. We believe both have their pros and cons, with concentrations being more easily interpretable by a diversity of aquatic scientists (e.g. fishery researchers), and with saturation values making abstraction of differential solubility at different salinities and water temperatures. In fact, the interannual variability in averaged temperature is rather small, particularly when compared to the intra-annual temperature variability.

[Minor Comments]

We specifically thank referee #1 for the detailed reading of our manuscript and the numerous minor comments and improvement that were suggested. We will take these into account during revision and will clarify and correct the manuscript.

We will in particular add two conceptual plots, making the description on pages 5436 and 5437 easier to follow.

Attachment

Figure 1. Conceptual plot of the recovery from hypereutrophication of the freshwater Schelde, and the potential subsequent recovery from eutrophication on a decadal time scale. Progressive reductions in organic waste and excess nutrient loads are the main drivers of this recovery. As explained in the text, the existence of two steady states and the associated regime shift, appear as a gradual evolution in annual averaged quantities

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6, C1631–C1637, 2009

Interactive
Comment

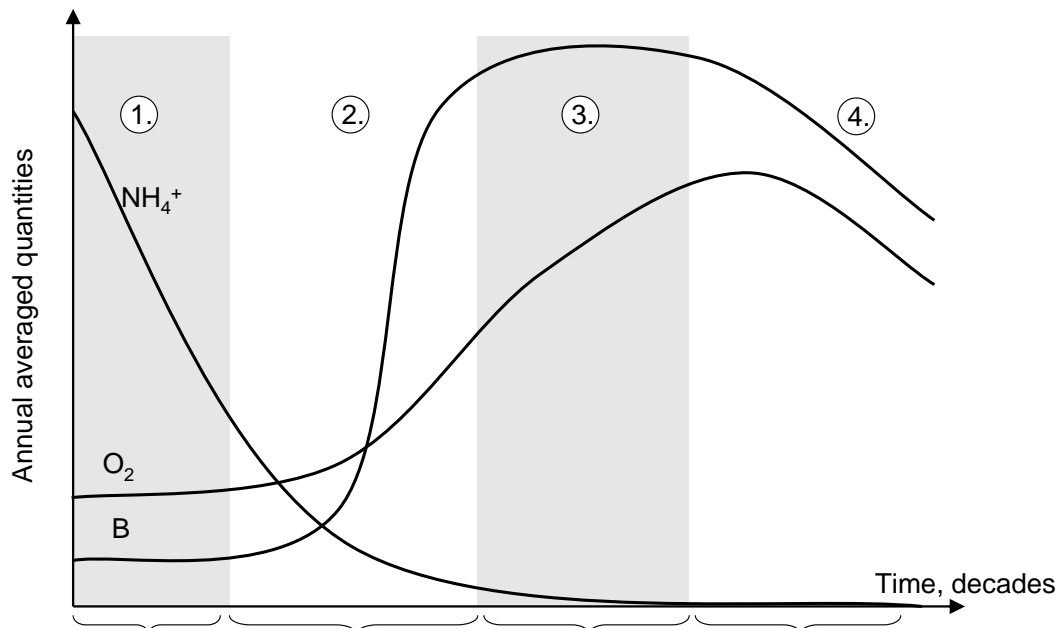
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Discussion Paper





1. Hypereutrophied state; a decrease in organic waste input leads to a decrease in ammonia concentrations, but prevailing summer anoxia inhibits algal growth

2. Transition between hypereutrophied and classical eutrophied state, marked by the two co-existing steady states, and large swings in oxygen concentrations.

3. Classical eutrophied situation

4. Further decrease of excess nutrient inputs, increase in zooplankton grazing and progressive restoration of other ecosystem functions might result in **future decrease of algal blooms.**

Fig. 1. Conceptual plot of the recovery from hypereutrophication of the freshwater Schelde, and the potential subsequent recovery from eutrophication on a decadal time scale. Progressive reductions in organic

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