**Biogeosciences
Discussions**

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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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

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 episode represents the classical, often studied eutrophied state, with intense algal blooms due to excess inorganic nutrient availability. The final episode 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

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

Specific answers to reviewer #2

We sincerely acknowledge referee #2 for her/his critical comments on our manuscript which stimulated us to rethink and helped us to further improve our text.

This referee raises two main concerns. First, he/she expressed the feeling that not enough context was provided, and that readers are assumed to have more background on estuarine systems than can be expected from a broad audience. We admit that we have, deliberately, written a short and concise paper, and maximally referred to the existing, and abundant, literature for the interested reader. Indeed, the site description was very short, and this will be extended to address of the questions raised by this reviewer. In the revised version we will include more background and references on the importance of nitrification, the dominance of phytoplankton in autotrophic production, the unimportance of benthic processes in pelagic biogeochemistry in this part of the system, etc. See below for detailed answers to the questions raised.

Second, referee #2 is critical to our combined approach of long term data analysis, literature review and numerical modeling to investigate and explain long term evolution of observations. According to him/her, laboratory or field experiments would be more appropriate tools. However, the spatial scale of our system and the temporal scale of the recovery are beyond the reach of experimental manipulation. For instance it is not possible to perform experiments to generate long-term trends extending over several decades. However, our hypothesis of algal growth inhibition is potentially testable in laboratory settings, and this is the logical next step after this study.

In fact, we can perceive the intense eutrophication of the Scheldt estuary as a huge, undeliberate experiment with a whole ecosystem on a large temporal scale. This experiment can only be performed once, it lies in the past, and we have to infer what happened, based on the incomplete monitoring data. Thus we report the data of the experiment, we analyze available data using a numerical model, and put forward sug-

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gestions for future research and future (small scale) experiments. This is very similar to any classical paper on a classical laboratory experiment.

Answers to specific comments

§1

"The intro doesn't effectively summarize the literature and current state-of-the-art of eutrophication reversal in estuaries"

We admit that our proposed title might read erroneously as though our manuscript would be describing the general dynamics of estuarine recovery from eutrophication. We will propose a new title that makes it clear that we are performing a case study (A macro-tidal freshwater ecosystem recovering from hypereutrophication: the Schelde case study.).

"... but rather than conduct any field/lab experiments rely on a modeling exercise to support their speculation"

We'd like to note that our speculation relies more on literature review of potential processes, and the analysis of the long term data set, than on the numerical modeling results. The reason that the numerical modeling provides a strong extra argument for the advanced hypothesis of algal growth inhibition, is that although we did not intend to build a 'realistic' model (the model is really simple and parameter values are taken from literature rather than tuned to fit the data) the numerical output is in remarkable, quantitative agreement with the observations. Since the only special feature that is responsible for this simple model to fit the long term trends, is the algal growth inhibition, this agreement gives a strong argument in favor of this hypothesis.

"Non-linearities and thresholds and regime shifts are not a strength of this manuscript"

For several reasons we do want to make a contribution to the regime shift literature. First, this is to our knowledge the first account of a (potential but mathematically well defined) regime shift in a flow-through system. See also the general comments above.

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Second, the 'classical' regime shift literature either focuses on conceptual, theoretical, dimensionless models, or use advanced statistical tools to test observed data on the existence of jumps as an indication for regime shifts, or on indicators to predict potential future shifts or collapses. Although simple, our model is more realistic and produces interpretable, quantitative output that can be linked with observed data. The parameterization of the proposed mechanisms that produces the shift, is realistic, and the theoretical jumps have amplitudes that correspond with observations.

"…these authors don't even comment about plants other than those measured by suspended chlorophyll concentrations"

We admit that in writing a concise paper, many important facts were not explicitly mentioned but only implicitly through literature references. We will significantly extend the context and system information as mentioned in the general reply above.

The importance of benthic plants to nutrient cycling in general and oxygen concentrations in particular, is indeed very limited in the freshwater Scheldt. Light intrusion is restricted to the upper meter of the water column due to high turbidity, excluding benthic primary production in the river bed (minimal water column depth is 3m). Due to embankments, the area of mudflats has been decimated (Meire et al 2005), excluding also a significant contribution from there. Note that the situation is different from the Westerschelde (downstream part of the estuary) where there exist large tidal flats. But even there, microphytobenthos production has been estimated to account for less than 17% of total algal production (De Jong & De Jonge, 1995; Heip et al 1995; Kromkamp et al 1995).

§2

We will expand the site description. In doing so, we will consider different wording.

§3

"Low oxygen is a human-induced impact or a consequence of nutrient enrichment due

to human activities in the watershed?"

In our system the major source of oxygen depletion is organic and chemical pollution, driving the water body to hypoxia through aerobic respiration and chemical oxidation of imported reduced substances. In other systems (mainly lakes and small streams), algal blooms, induced by nutrient enrichment, are the source of (nightly) hypoxia events due to autotrophic respiration. In coastal system, bottom water hypoxia results from the enhanced flux of organic matter from surface waters to deeper water, a consequence of enhanced production in the surface waters due to nutrient enrichment. We perceive all these causes of hypoxia as 'human-induced'.

§4,5

We'll consider these suggestions.

§6

"what's the difference between annual and FW averaged concentrations"

The data are both annually averaged and averaged over the ecosystem, and this produces the freshwater ecosystem (FW), annual average. We will define this explicitly in the revised version.

"what's the breakdown between TDN, NH4+, DN and NO3"

See Figure 2: black circles represent NH4+, triangles represent TDIN, the difference is NO3+NO2. DON has not been measured

"…where were the 2 pre-1995 DO samples collected from?..."

We don't understand this question. As is clear from table 1, and figure 2, there are 22 pre 1995 DO sample averages (each average consisting of more than one sample).

§6

"You should show NO3- concentrations"

We implicitly show NO3- concentrations, by showing TDIN concentrations

§6, 7, 8, 9

We admit that in writing a concise paper, many important facts are not explicitly mentioned but only implicitly through literature references. We mention here the literature that answers the questions raised by referee #2, and we will extend the site description along these lines in the revised version of the paper.

The nitrogen cycle in the water column and in the sediments of the Scheldt estuary has been studied for more than 3 decades, with studies from the macro scale to the micro scale, with all available methodologies (Billen (1975), Vanderborght en Billen (1975), Billen et al (1985, 2005), Soetaert and Herman (1995), Middelburg and Nieuwenhuize (2000, 2001), de Bie et al. (2002), Gazeau et al (2005), Gribsholt et al (2005,2006,2009), Andersson et al. (2006a,2006b), Brion et al. (2008), Hofmann et al. (2008), Vanderborght et al. (2008)). The fact that nitrification has been the major oxygen consuming process is well established and confirmed in different ways, from in-situ measuring to ecosystem modeling exercises (e.g. Gazeau et al (2005), Soetaert and Herman (1995), Vanderborght et al (2008)), and it is generally quoted as the major process leading to suboxic conditions in eutrophic estuaries (e.g. Heip et al. 1995). Based on the observation that maximum nitrification rates are found in the zones of maximum turbidity, it has been hypothesized that the attachment of nitrifiers to particles could be responsible for their long retention times in estuaries. As such, nitrifiers can attain high biomass, resulting in high overall nitrification rates (Owens, 1986; Heip et al 1995).

The fact that chlorophyll a is an adequate proxy for autotrophic production, is well known for the Scheldt, and in fact for temperate, tidal estuaries in general (Heip et al 1995). Macro-algae don't play a significant role in turbid, tidal estuaries (Heip et al. 1995). Vascular plants can be important for nutrient cycling in (the vicinity of) certain tidal marshes. In the freshwater tidal estuary however, the area of tidal flats and marshes has been decimated (Meire et al 2005) and they don't influence the pelagic

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oxygen concentrations. There are no vascular plants growing in the main channel of the highly dynamical European estuaries.

Similarly, water biogeochemistry in this part of the estuary is governed by pelagic processes. As explained above, no light reaches the sediments of the main channel, and the total area of tidal flats in the freshwater part is too low to play a significant role in pelagic biogeochemistry. Even in the Westerschelde, with large intertidal and shallow water zones, benthic processes might be tractable, but are much less important than pelagic processes (e.g. Soetaert and Herman, 1995; Vanderborght et al 2007). Overall, although benthic processes do play a role at certain scales, in temperate tidal estuaries their role in pelagic biogeochemistry at ecosystem scale is of minor importance when compared to pelagic processes (Heip et al 1995).

Finally, estuaries are generally classified as heterotrophic systems (Heip et al 1995), with the degree of heterotrophy of the Westerschelde estuary one of the highest yet reported. On average 380g carbon per square metre is net lost per year (range 200-1200 gCm-1y-1) (Soetaert and Herman 1995b). This conclusion has always been extrapolated to the freshwater part of the estuary, which was probably correct until recently but, based on our findings, might need review.

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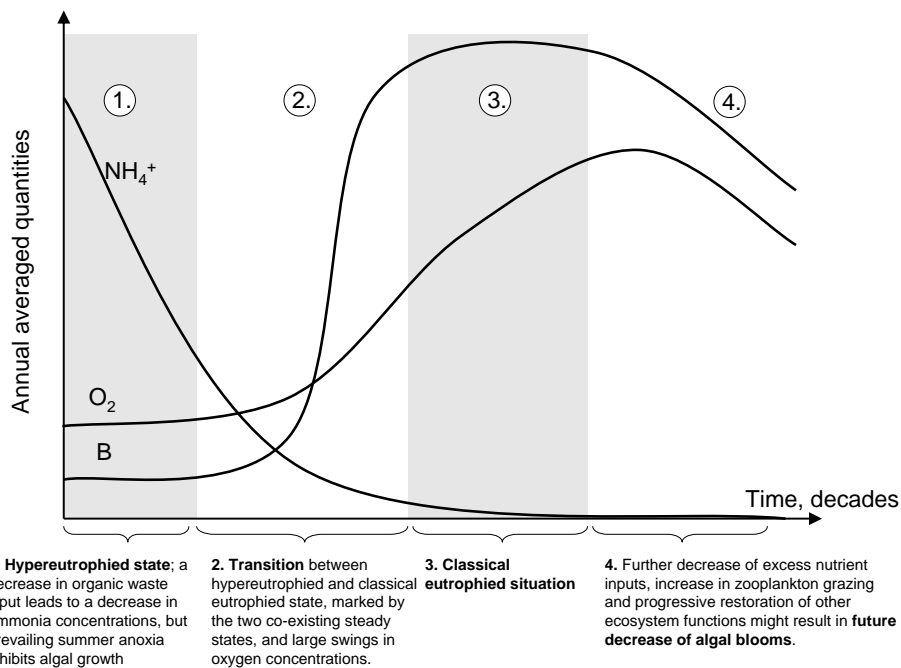
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1. Hypereutrophied state; a decrease in organic waste input leads to a decrease in ammonia concentrations, but prevailing summer anoxia inhibits algal growth

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