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Biogeosciences Discussions

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

Anonymous Referee #1

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GENERAL COMMENTS

Cox et al. report during the last 40 years the intriguing trend of an increase of phytoplankton biomass paralleled with decreasing inorganic nutrients in the freshwater Schelde. The mechanisms behind this trend are investigated using a simple mathematical model.

MAJOR COMMENTS

The authors use the definition of autotrophy and heterotrophy from Garnier and Billen (2007), whereby P/R = GPP/(autotrophic R + Nitrification). However, this definition is different from the widely accepted and commonly used definition of ecosystem or community autotrophy and heterotrophy whereby P/R = GPP/(autotrophic R + heterotrophic R) (Odum and Hoskin, 1958; Odum and Wilson, 1962; Smith and Hollibaugh 1993; Heip et al., 1995; Kemp et al., 1997; Duarte & Agusti 1998; Gattuso et al., 1998; Gazeau et al., 2004; 2005a;b;c; Battin et al. 2008). Further, nitrifiers are chemoautotrophs, hence nitrification needs to be accounted in GPP and not in the R term of P/R (e.g., in the Schelde (Gazeau et al. 2005b)).

This will mislead a reader (that misses the information that the present paper uses the definition of Garnier and Billen (2007)) and assumes that autotrophy and heterotrophy refer to the widely accepted definition of P/R = GPP/(autotrophic R + heterotrophic R). Also, in page 5445 lines 24-29, the authors discuss autotrophy and heterotrophy while mixing apples and oranges, since P/R either refers to GPP/(autotrophic R + Nitrification) or to GPP/(autotrophic R + heterotrophic R). I very strongly suggest that the authors replace the terms autotrophy and heterotrophy by something like O2-deficit status and O2-surplus status, respectively.

The authors state that the Schelde evolved to a situation where P/R = GPP/(autotrophic R + Nitrification) > 1. This means surface waters of the Shelde should be over-saturated in O2, or close to atmospheric equilibrium (if the term Raer is very strong and brings waters close to equilibrium). Assuming an annual average water temperature of 14°C, the O2 saturation level (atmospheric equilibrium) is about 320 μM, while annual averages of O2 concentration in the late 2000's are well below, at about 200 μM as shown in figure 2. This means that either there is an over-statement in text, and the system evolves towards P/R = GPP/(autotrophic R + Nitrification) > 1 but does not reach it, or that the model missed an important term in the O2 dynamics.

And indeed there is an important O2 consuming process missing in the model: respiration by heterotrophic bacteria. I agree that in a situation where NH4 is extremely high, the major O2 consumption will be by nitrification. But as NH4 and nitrification decrease, degradation of organic matter by heterotrophic bacteria will become an increasingly important O2 consumption term. I think that this could be (easily) explored with the model, and would probably require some sort of parameterization of heterotrophic respiration as a function of phytoplankton biomass (B), since the authors assume there is no advection of matter from upstream in the model, and external organic carbon inputs are not included in the model. This does not seem to a very complicated parameterization to implement in the model.

Throughout the ms the authors show O2 concentrations. However, during the time-series there have been strong inter-annual variations in temperature that affect O2 concentration by changes in solubility. It would be useful to show the degree of O2 saturation (O2_in_situ/O2_at_equilibrium*100) instead (or in addition) to O2 concentration.

MINOR COMMENTS

Somewhere in text it could be mentioned that silicate is not discussed because assumed not limiting in the Schelde, and limitation is assumed to be related to N or P.

The authors mention the paradox of decreasing nutrients and increasing phytoplankton biomass. However, they only show inorganic nutrients, while it is established that the majority of rivers inputs of N and P are in the organic form. This should anyway be mentioned. Further, do they have some idea of the trend of N and P organic nutrients for the time period ?

Abstract : the first two sentences are general (introductory) statements that should be removed. An abstract should only provide information on methods and major findings of the paper.

Abstract : it should specified that the model only looks at O2 limitation of primary production, and that O2 dynamics are mainly due to nitrification and NH4 loads. Among the hypotheses «either by elevated ammonium concentrations, severe hypoxia or the production of harmful substances in such a reduced environment» only limitation by hypoxia was tested in the model.

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Introduction : It might be useful to mention that changes in C,N,P cycling in an estuary such as the Schelde will also have an impact on food-web structure (e.g. Lancelot et al. 2005; 2007) and overall carbon cycling (e.g. Gypens et al. 2009) in the adjacent coastal zone (in the present case the Southern Bight of the North Sea).

Introduction : In the Black Sea very complex and fairly well documented regime shifts have been reported (Oguz T & D. Gilbert 2007; among many others) in part related to nutrient delivery.

Introduction : page 5433 Lines 14-16 : This sentence already describes the results and should not appear at the end of the introduction.

Page 5433 Lines 18-19 : the direct effect of the toxicity of ammonium was not tested in the model.

Page 5434 Lines 22-27 : these statements are difficult to understand. Please clarify and add more information.

Page 5435 Lines 16-17 : TDIN are discussed but not shown. These data could be added to figure 2.

Page 5436 Lines 1-4 : It could be useful be add to Figure 2 a plot with number of observations per year.

Page 5436 Line 16 : I assume that «metabolism» refers to physiology.

Page 5436 Lines 18-19 : During the time period of investigation, is there evidence for absolute anoxia (O_2 = 0) ? If not then this hypothesis does not make sense, since sulfides cannot co-exist with O_2 (even at low O_2 levels) and are immediately oxydized to sulfate.

Page 5436 Lines 25-26 : this statement is not clear please clarify

Page 5436 Line 27 : «not change much»; change of what ?

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Page 5436 Lines 25-27 + Page 5437 lines 1-10 : It might be useful to add a conceptual idealized plot of the temporal evolution of NH_4, O_2 and phytoplankton biomass to clarify the expected conceptual evolution of regime shift related to ammonium changes. This text is a bit difficult to follow, but presents the conceptual frame of model and of the paper.

Page 5437 Line 15 : I assume authors refer to state «variables» ?

Page 5437 Line 18 : I assume authors refer to gross primary production ?

Page 5437 Line 18 : the conventional abbreviation for gross primary production is GPP and not Rpp

Page 5437 Line 18 : I assume authors refer to autotrophic respiration ?

Page 5437 Lines 11-19 : the term $F(O_2)$ needs to be defined here, before the equations appear. It is difficult to understand equations if $F(O_2)$ is defined in the paragraph after the equations.

Page 5439 Line 5 : Please justify the choice of time period for comparison

Page 5445 Lines 14-19 : Do the authors have an idea how important zooplankton grazing is as a removal term of phytoplankton biomass in the Schelde or in freshwater tidal rivers in general ? Even if zooplankton biomass increase would this change something in phytoplankton biomass ? Although not directly comparable (since different phytoplankton species and different intensities of C flows), in the Southern North Sea grazing only represents 5 % to 10% of GPP (Lancelot et al. 2005).

Page 5455 : Fig. 2 : I'm very surprised by the lack of Chlorophyll-a data prior to 1995. This is a standard monitoring variable that should have been sampled in conjunction with O_2 and nutrients. Why was this not the case ?

Page 5455 : Fig. 2 : It could be useful to add suspended matter to this plot. The authors state that it did not change but it could be useful to show the data, anyway.

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Page 5459 : Fig. 6 : Is there an explanation why SPM values seem to be higher after 2002, while discharge does not seem to show a trend ?

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