Biogeosciences Discuss., author comment AC3
https://doi.org/10.5194/bg-2021-59-AC3, 2021

Reply on CC1

Dante M. L. Horemans et al.

Author comment on "Evolution of the long-term and estuary-scale phytoplankton patterns in the Scheldt estuary: the disappearance of net growth in the brackish region" by Dante M. L. Horemans et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-59-AC3, 2021

We thank the Referees for their time to review the manuscript. Although the Referees see the high potential in studying the (combined model approach and) multi-annual observations in the Scheldt estuary, they point out that the reason for using the presented model approach requires further clarification. We believe that we can satisfy all Referees' comments by (1) a thorough revision of the text, focusing much more on the reasons for our assumptions, embedding in literature, and using the discussion to reflect on the validity of our work rather than to speculate. Furthermore, (2) the Referees' suggestions inspired us to extend the model by distinguishing between the two dominant phytoplankton groups (i.e., freshwater and marine diatoms). We hope that our response and suggestions allow us to resubmit the manuscript after deep revision. We thank you for your continued interest in our research.

1. General reply and outline for the revised manuscript

In the following, we briefly motivate our model approach, discuss the main weaknesses of its presentation in the manuscript, and propose solutions to tackle these concerns. Next, we respond to the individual remarks of Josette Garnier and Anonymous Referee #3.

1.1 Explanation of our method

The goal of our study is to describe the appearance and disappearance of Chl-a accumulation in the brackish region in spring in 2004-2018 and analyze whether this is due to changes in physical or biological characteristics. Here, the observations are the core. Our model approach is a complementary tool to interpret the data. This is our motivation to construct the model as such it is mainly data-driven and most of the parameters directly follow from the observations. We aim to minimize the number of variables and calibration parameters that we cannot validate using data.

This approach is different compared to state-of-the-art modeling studies such as Arndt et al. (2011), Gypens et al. (2013), and Naithani et al. (2016). In the latter modeling studies, the phytoplankton-zooplankton(-nutrient) dynamics are explicitly resolved over one year, assuming multiple phytoplankton and zooplankton groups. Such models require quite a few calibration parameters that are poorly constrained (e.g., maximum grazing rate, mortality rate per species). These parameters are generally calibrated by fitting to data and then assumed to be fixed in time. Although assuming fixed parameters may be acceptable when focusing on one year, we study an observed trend change, suggesting that (some of these) parameters must have changed over time.

Before our work, the observed trend change in Chl-a was poorly described and it was unclear whether this trend change is related to changes in physical characteristics (e.g., sediment, discharge, temperature) or changes in biological characteristics. In our study, we can constrain this to a change in biological characteristics related to phytoplankton mortality that seems to have some correlation with zooplankton grazing. However, we can at this moment not constrain this further as detailed data is lacking.

We see this as the rejection of the hypothesis that changes in the physics explain the observations and opening a new research question motivating to investigate the biological changes in more detail.

1.2 Suggested outline for the revised manuscript

We see that our approach suffers from chronological reasoning; we start with a light-limited model for phytoplankton growth and then add the grazing functions to the model only in the discussion and speculate about other aspects that affected grazing. Hence, grazing seems an afterthought, which is strengthened by the fact that no data can constrain the final result stating that some change to the functions g1 and g2 must have occurred.

Therefore, we propose to thoroughly restructure the manuscript:

- In the introduction, we will more explicitly state the goal as done above and set the scope of this study (comments by Referee #3). Furthermore, we will more elaborately present the state-of-the-art models and applications to the Scheldt (comments by Referee #3) and motivate our model approach.
- In the methodology section, we will start from the state-of-the-art descriptions and explicitly explain why our simplifications are justified within our scope and how the remaining parameters should be interpreted (comments by all Referees). We will present only one model and will not add new aspects later in the discussion (comments by all Referees). This model will directly include the effect of grazing and distinguish between the two dominant phytoplankton groups (i.e., freshwater and marine diatoms) (comments Josette Garnier).
- We will shorten the results, focusing on the main results and removing the lengthy sensitivity study (comments by Huib de Swart)
- We will thoroughly revise the discussion, focusing on model interpretation in the context of the literature, model limitations, implications for other estuaries, and further work.

Please, find below our response to the main concerns of Josette Garnier and Anonymous Referee #3.

Josette Garnier

Overall, we see that we should better motivate the choices and assumptions made in our model. We can support each of our assumptions based on our data, thereby resolving your concerns. Additionally, we should include the two zooplankton classes from the start, as opposed to introducing them as a model variation at the end. Both points would be fairly easily addressed. We explain this in more detail below.

It is difficult to understand why the authors did not take silica into account, and

hence the diatom and non-diatom compartments

We believe that the Referee refers to Si-limitation which sometimes occurs at the downstream boundary of the Scheldt, depending on the season. This is not the focus area of our study. The assumption of a minor impact of N-P nutrient limitation in spring follows from observations (LINES 64-67). Similarly, the Si concentrations are at least one order of magnitude larger than the corresponding half-saturation constants in spring. We will add this to the manuscript.

As the phytoplankton abundance dominantly consists of diatoms (Maris and Meire, 2007; Muylaert et al., 2009; Maris and Meire, 2009, 2013, 2017), we do not distinguish between non-diatoms and diatoms. However, we do realize based on your concerns that it may be useful to distinguish between freshwater and marine diatoms [following, for example, Vanderborcht et al. (2002), Naithani et al. (2016)]. We will add this distinction to the manuscript and present and discuss the corresponding results.

Zooplankton is treated by magic parameters that arrive at the end

We agree that we should present only one model to avoid chronological reasoning (see general response above). We included two zooplankton classes using a data-driven approach. A phytoplankton mortality rate that is linear to the zooplankton abundance is an accepted model set-up (Steele and Henderson, 1992). If we were to resolve the zooplankton dynamics explicitly, we would end up with more calibration parameters besides g1 and g2, such as the mortality rate of zooplankton, which is a parameter that is notoriously hard to constrain.

Anonymous Referee #3

We agree that our approach needs clarification, motivation, and restructuring, including a discussion of state-of-the-art models (see our general response above). In the following, we present a point-to-point response to the other concerns of Anonymous Referee #3.

The title is misleading

We acknowledge the title would benefit from a more concise formulation; with 'long-term', we mean 'multi-annual', and 'net growth' should be replaced by 'accumulation of Chl-a'.

The claim that decreased herbivory is responsible for the intermediate CHL-a maximum is poorly supported by any kind of evidence

We agree that Section 4.2 contains hypotheses and should be removed. A discussion on the evolution of grazing parameters is the best possible given the available data, which cannot constrain this further. Still, we think we have made significant progress constraining the observed phenomenon to some change in biological characteristics as (direct effects) of changes in physical characteristics (sediment, discharge etc) are insufficient (see also general response above).

As "deus ex machina" the authors impose a 7-fold decrease in "calanoids grazing efficiency"

We discuss the importance of mortality/grazing in the discussion Section 4.1. The required 7-fold decrease in the mortality rate/grazing parameter is indeed not explained within our model context and the data does not provide conclusive evidence here as well. Nonetheless, such variations comply with grazing experiments (LINES 364-366) and are also found in other modeling studies.

The pretty central functions Z1 and Z2 are not given at all

The functions Z1 and Z2 and the corresponding extrapolation are defined in the results section (LINES 309-314). However, we agree that it would be better to mention this already in the methodology section when we introduce the mortality rate [Eq. (3)].

The analysis is restricted to the spring bloom only

This statement by the Referee is correct and should be mentioned more explicitly in the manuscript.

Why are seasonal data displayed and discussed?

We see that this may be confusing as we focus on spring blooms. We will change this and only present the values for the relevant months in a table.

Much higher CHL in 2004-2007 for 80km<x<120km were not analyzed

This can be explained by the lower mortality rate in 2004-2007, resulting in a slower decrease of Chl-a in the downstream direction. We will mention this in the manuscript.

Lacking discussion of the literature

We agree that our model approach is lacking a clear motivation/discussion, including similar state-of-the-art modeling studied in the Scheldt estuary. We will more elaborately present the state-of-the-art models and applications to the Scheldt. We will thoroughly revise the discussion, focusing on model interpretation in the context of the literature, model limitations, implications for other estuaries, and further work (see the general response above).