

Biogeosciences Discuss., author comment AC1  
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## Reply on RC2

Dante M. L. Horemans et al.

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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-AC1>, 2021

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We thank Anonymous Referee #2 for their time to review our manuscript. In summary, the core argument for Anonymous Referee #2 to reject our work is in the comments that the model 'is an over-simplified model', 'is not able to explain the phenomena observed within the data', and 'does not tackle the phytoplankton dynamics appropriately'. The reviewer is additionally very specific on the elements that need to be in a model, which is a model that 'investigates timeinterval in a transient manner and resolves different phytoplankton and zooplankton groups' and that 'Boundary conditions should be defined temporally variable'.

We do recognize that our conclusions may be too strongly suggested regarding the importance of grazing and are happy to revise this. Otherwise, we do believe our model is well-suited for the scope of our study. Below, we motivate this by a point-to-point response to the main concerns of Anonymous Referee #2.

### Reply to reviewer main comments

#### *Over-simplified model/does not tackle the phytoplankton dynamics appropriately*

The goal of our study is to investigate the hypothesis that the observed rise and fall of a mid-estuary phytoplankton bloom could be explained by sediment concentration. We deliberately kept our model simple and focused on the effects of sediment-induced shading to be able to study exactly this without any other obscuring effects. Hence, we could do an exhaustive sensitivity study over all parameters. This yields a very robust result that rejects the hypothesis.

We do agree with the reviewer (also see several of the comments below) that it might be too premature to point to grazing as the alternative explanation. Our model might indeed be too simple to show this without doubt. We would be happy to revise our discussion in this respect, primarily concluding that phytoplankton mortality is important and stressing that there could be multiple explanations to this, of which grazing is one.

We would agree that the model is too simple to simulate the precise phytoplankton dynamics through the year. However, this is not the goal of this study and we would agree

with the reviewer that there are other models and studies that are much more suitable for that.

*is not able to explain the phenomena observed within the data*

The phenomena that we are looking for are the approximate location and order of magnitude of a phytoplankton bloom. Within this scope, we agree that the model with a system-constant mortality rate (cf. Reference case) is not able to capture the accumulation of phytoplankton in the brackish region in spring in 2008-2014. This is exactly what we conclude in our manuscript and is used to support the main conclusion rejecting the hypothesis that observations can be explained by changes in sediment dynamics only.

However, when using a spatially varying mortality rate related to observed zooplankton concentrations, our model is at least 90 % accurate in all three periods in the region for which we have zooplankton data (beyond km 60). This shows that our model is able to explain the large-scale phenomena provided mortality is chosen in a suitable way.

### **Reply to reviewer suggestions**

*The model should investigate the timeinterval in a transient manner*

We solve the equations in equilibrium state and not in a transient manner. We argue that this assumption is acceptable because, firstly, the accumulation of phytoplankton in the brackish region covers approximately two months, which is large compared to the time scale of a bloom ( $\sim$  weeks). In other words, the system has enough time to evolve towards equilibrium. Secondly, we observed the accumulation of phytoplankton consistently over 7 consecutive years (2008-2014). If the system were to be sensitive to initial conditions (e.g. exact temperature and discharge in winter and early spring), and we would thus have to solve the system in a transient manner, we would expect more variability over these 7 years.

On the contrary, we believe that assuming equilibrium state strengthens the conclusions as it shows that the results do not depend on precise conditions in winter or early spring. This allows us to present a sensitivity analysis consistently and efficiently, which is a requirement given the scope of the manuscript.

*The model should resolve different phytoplankton and zooplankton groups*

Taking one group of phytoplankton which represent the community average is a common approach in literature. Given the scope of this study, we believe this is sufficient as interaction between different species is unlikely to affect the conclusions regarding the effect of sediment on blooms. Furthermore, in the extended model, we do parametrically include effects of two zooplankton groups, based on observations [see Eq. (3)].

*Boundary conditions should be defined temporally variable*

As our model allows for an extensive sensitivity analysis, we validated the assumption of using constant boundary conditions. The results show a minor impact of variability of the boundary conditions on accumulation of phytoplankton in the brackish region (see Figure 9).

### **Reply to the major other comments**

*Gypens et al 2013 (<http://dx.doi.org/10.1016/j.jmarsys.2012.10.006>) studied the Scheldt estuary and came to more detailed and opposing results.*

Assuming that Anonymous Referee #2 means with 'opposing results' that Gypens et al. (2013) conclude that 'grazing pressure plays a negligible role', this indeed seems to contradict our results. However, we read that the corresponding results are not shown in their paper and it is not explained what experiment was done to get to this conclusion. We thus cannot determine how we should compare our results to the results of Gypens et al. (2013) regarding the sensitivity to grazing.

We do however agree that we cannot strictly draw conclusions about grazing, only about mortality and are happy to change this in our manuscript (see reply to comment 1).

On other aspects, the results presented in Gypens et al. (2013) show similar patterns compared to our results.

*Other parameters may play a role too (see McQuatters-Gollop and Vermaat (2011), doi:10.1016/j.seares.2010.12.004)*

We agree that there are potentially many parameters affecting phytoplankton dynamics. Here, we want to focus on the effects of sediment and accept that some other model parameters represent many other processes and parameters. Specifically, our mortality parameter describes multiple processes related to water quality as described by the referenced paper and we would be happy to discuss this better (also see reply to comment 1)

*Winter values of zooplankton may play a key role (Dudeck et al 2021, doi:10.1093/plankt/fbab011)*

The referenced paper does not relate to the Scheldt but to the English Channel. As mentioned above, we observed the accumulation of phytoplankton consistently over 7 consecutive years (2008-2014). If the system were to be sensitive to initial conditions (i.e., winter values), we would expect more variability over these 7 years. Especially because these years included some of the coldest and warmest winters alike in recent history.

*L451 ff: The method to calculate SPM for the early periods is very critical, as deep-water SPM structures are governed rather by benthic-pelagic interactions than by surface variations.*

This may be true for a transparent system but the Scheldt estuary is a turbid system in which phytoplankton growth is only possible near the water surface. The euphotic depth equals 1.15-0.45 m, much smaller than the depth.

*L458: Neglecting background- and phytoplankton-induced light extinction is very critical.*

We do include background- and phytoplankton-induced light extinction in the model runs, but not when estimating  $k_c$ . We agree that neglecting background and phytoplankton-induced light-extinction may be critical in a transparent system. However, the Scheldt estuary is very turbid. Using the Chl-a observations and  $k_p$  coefficient presented in Table 1 (which complies with values found in the literature), we obtain a  $k_c$ -value of  $67 \text{ m}^2 \text{ kg}^{-1}$  instead of  $72 \text{ m}^2 \text{ kg}^{-1}$  for 2015-2018 when we do include phytoplankton-induced light extinction. When we also include background included light-extinction, we find a value of  $66 \text{ m}^2 \text{ kg}^{-1}$ . As shown by our sensitivity analysis (Figure 8b), the impact of this slight difference on the model results is negligible. However, we would be happy to change this in our manuscript.