

Biogeosciences Discuss., author comment AC2
<https://doi.org/10.5194/bg-2021-59-AC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Reply on RC1

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-AC2>, 2021

We thank Prof. Dr. de Swart for his time to review our manuscript. His main comments concern 1. the applied methods (to find the light extinction coefficient), 2. the choice of the model and the formulations/assumptions used in that model, 3. the presentation of the results and the discussion. Below, we present a point-to-point response to his main concerns. We would be happy to also address all other minor comments along with a revised manuscript.

1. The applied methods (to find the light extinction coefficient)

We used the data available within the OMES monitoring program. We agree that measuring at different depths may be preferable in case we expect strong vertical stratification of SPM in the euphotic region. However, given the high turbidity in the Scheldt estuary, the euphotic depth is relatively small (\sim dm) compared to the total water depth (\sim m). We thus only expect phytoplankton growth near the water surface where we do not expect strong vertical stratification of SPM. Therefore, it is preferable to have a better estimate at the water surface instead of an estimate covering the full water column. We will clarify this in the manuscript.

2. The choice of the model and the formulations/assumptions used in that model

Why use this model instead of e.g. a state-of-the-art model like Delft3D? Alternatively, the authors could have used the model of Arndt et al. (2011), why was decided otherwise? See also later comment: a discussion about the model limitations is missing.

The main reason why we used an idealized model is that it allows us to apply an extensive sensitivity analysis consistently and efficiently over all uncertain parameters, which is a requirement given the scope of the manuscript. We could have alternatively done this within another framework such as that of Arndt et al (2011), which we would have then needed to simplify more as their model already contains too many parameters for our scope. We decided to use iFlow as we already have a track record for this and verified that sediment and flocculation are sufficiently well represented in this model.

For the revision, we suggest including an extensive motivation for choosing these specific

simplifying assumptions and the framework that we chose in the introduction and method sections. Also, we will discuss model limitations and strengths better in a separate discussion section (see also comment below).

Why is salinity considered to be static and prescribed?

We motivate the choice of using a static longitudinal salinity profile by the fact that, firstly, salinity gradients are of minor importance to SPM transport in the Scheldt estuary and, secondly, that phytoplankton cells are passive and move with the tidal water flow.

This comment also applies to the statement in l177 'to correct for the large temporal variability in the discharges'.

The assumption of using a constant freshwater discharge is validated in our sensitivity analysis, which shows only a minor impact of variability in freshwater discharge on the accumulation of phytoplankton in the brackish region.

3. The presentation of the results and discussion

Section 3.2 (sensitivity analysis) is quite exhaustive and has a bit the style of a logbook. Section 4.2 is not really satisfactory. What would add value to this study is to put the results in a broader context.

We agree with the suggestion to move the Appendix and the results of the sensitivity analysis presented in Section 3.2.1 to an Electronic Supplement. Additionally, we propose a bigger reorganization of the manuscript in which zooplankton is introduced from the start so that no new elements are introduced in the discussion. Furthermore, section 4.2 will be removed and the discussion will focus on discussing the model context, limitations, and applicability to other estuaries.