Comment on bg-2021-59
Anonymous Referee #3


The paper of Horemans et al ("Evolution of the long-term and estuary-scale phytoplankton patterns in the Scheldt estuary: the disappearance of net growth in the brackish region") documents a joint data and modeling study for explaining time-variable accumulation of Chl-a in the Scheldt estuary. I see the merit of the study in presenting valuable observations on physiological changes in phytoplankton over the season or on the abundance of herbivorous grazers along the estuary. Also the modeling puts appropriate emphasis on SPM dynamics as a major driver of primary production in a highly turbid environment. For such systems, so far not many studies sought to combine both new observations and new modeling. As a consequence, I see a high potential value in the paper, which the authors unfortunately fail to exploit properly.

The major problem lays in the approach and its presentation. Already the title is misleading. The issue of "long-term evolution" is nowhere really addressed, nor is the "disappearance of net growth in the brackish region". Apart of the intermediate appearance of a CHL-a maximum in the brackish zone, the study does not discuss any long-term dynamics. After reading the paper two times, I could not find any hint on temporal patterns in net growth rate. It took the second read to grasp the goal of the study, which is understanding the intermediate appearance of a CHL-a maximum. The storyline is blurred by many weaknesses in the structuring and selection of material, of the language, and of graphical presentation. Most importantly, the claim that decreased herbivory is responsible for the intermediate CHL-a maximum is poorly supported by any kind of evidence. And it would take little to convince me, due to similar findings of preceding literature (see below). Observations of zooplankton abundance are shown for x (=distance from the mouth) larger than 70km, thus having little overlap with the zone of interest (40km<x<80km). How were those values extrapolated? The pretty central functions Z_1(x) and Z_2(x) are not given at all! As "deus ex machina" the authors impose a 7-fold decrease in "calanoids grazing efficiency" for the interim period (2008-2014, Table 1) without a clear justification, which process might cause such a drastic inactivation of herbivory. By the way, the notion "grazing efficiency" is simply wrong - as are many biological notions used in the paper (see below). The authors suggest that a roughly 20-25% increase in turbidity (Fig. 4) may have hampered predation rates. This explanation clearly suffers from the mismatch of numbers and most of all ignores that in the upstream Scheldt (x>80km) a lot of calanoids and non-calanoids
To conclude, I cannot recommend publication of this paper in BG.

From the numerous minor comments I focus on language, where three types of issues are most apparent:
* semantic errors (e.g., L16, L19, L24, L46-47, L49, ...)
* repetitive wordings (e.g., 2 x "estuary" in title, 14 x "observe" and 6 x "shows" p.10-11, 4 x "captures" L255-257, ...)
* wrong biological notions ("phytoplankton grazing" L21, "grazing efficiency" typically denotes the energetic yield of ingestion, "mu_max" usually denotes maximal growth rate
but is here used for maximal photosynthesis rate, "phytoplankton-induced exponential light extinction coefficient" = self-shading coefficient, "zooplankton grazes .. primary production" L358, "volume-weighted phytoplankton concentration"?, "volume-weighted zooplankton abundance" = biomass ?, ...