Biogeosciences Discuss., referee comment RC3
https://doi.org/10.5194/bg-2021-59-RC3, 2021

Comment on bg-2021-59

Anonymous Referee #3

Referee 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-RC3, 2021

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 seem to do very well at much higher turbidity. Finally, a such produced CHL-a maximum

displays only a small agreement with the data (Fig. 10b). To conclude, the papers leaves us with a fully unresolved pattern, which is disappointing for a joint data-modeling study. It also remained unclear whether the analysis is restricted to the spring bloom only (Mar-Apr, e.g. Fig. 4, 7-11) or the entire season (Fig. 2,5-6). For example, in the caption of Fig.3 I can only guess whether "in spring" relates to "calanoids and non-calanoids" only - or to "Time-averaged Chl-a concentration" as well. If the focus is on the spring bloom, why are saisonal data displayed and discussed? Data on phytoplankton physiology available for 16 stations have not been shown over the entire transect: here I would expect an important hint on the role of, e.g., photo-acclimation in (non-) producing part of the CHL-a maximum.
The overall low quality of the work is in addition reflected by the lacking discussion of the literature.
Did the authors just forget to do their basic homework, or did they follow a hidden agenda when ignoring previous modeling studies for the same estuary (or the nearby Oosterschelde)? First, from the many estuarine works of Karline Soetaert and her group, only an old one is cited. The very recent and related paper of Jiang, Soetaert et al (Biogeosciences 2020) is not mentioned although it prominently discusses the chlorophyll maximum, which includes a list of further studies on this common pattern in many estuaries, often reported for US estuarine systems (see Fig. 13 in Jiang et al). Similarly, from the model studies of Pierre Regnier and Sandra Arndt only one is cited, but in a false way (as nutrient limitation does play a major role in estuarine phytoplankton dynamics during summer and autumn, at least in the downstream part).
Naithani et al (Hydrobiologia 2016), alike Jiang et al, devised an ecological model including zooplankton dynamics for the Scheldt estuary, hence already accomplished what has been envisioned by Horemans et al in their outlook. That paper already tried to assess the role of trophic cascading for the spatio-temporal estuarine distribution of phytoplankton.
Wirtz (PLOSone 2019) has shown that CHL accumulation in the turbid Wadden Sea indeed is likely the result of intense carnivory by suspension feeders and juvenile fish, which lowers top-down control by herbivores on light-limited autotrophs. A dominant role of trophic links has been backed up by Jiang et al insofar also in their simulations bivalve grazing critically controls phytoplankton distribution. However, here bivalves directly impact phytoplankton concentration in a negative way, while in the study of Wirtz they do so in a positive way by preferably removing zooplankton. These contrasting effects may well be a candidate for explaining the intermediate emergence of the CHL maximum insofar reflecting either changes in the total biomass or the community composition (and thus feeding preference) of estuarine bivalves. However, without a clear presentation of herbivorous biomass along the estuarine transect this route of thinking remains pure speculation. If the single observation within the brackish zone revealing no temporal trend in herbivorous abundance (not identical to biomass!) would be confirmed for the entire zone, the investigation would need to follow new routes. This brings me to the question why the authors have focused on a pattern where the data is incomplete while other interesting features of the spatio-temporal phytoplankton distribution such as the much higher CHL in 2004-2007 for 80km<x<120km were not analyzed. This choice would have eased the constraining of the otherwise poorly documented inverse modeling experiments.

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 ?, ...)