Referee 2
Interactive comment on "Drivers of the spatial phytoplankton gradient in estuarine-coastal systems: generic implications of a case study in a Dutch tidal bay" by Long Jiang et al.
Nicole Millette (Referee)
nmillette@vims.edu
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This is one of the most well-written papers I have reviewed and everything was well laid out and easy to follow. However, I was left with wanting a lot more information and discussion from the authors. This is the first time I have ever said this, but this paper is too short. There are three different approaches used in the paper, which is a plus, but there is a lot of information that could be shared about each approach. The main conclusions for the paper are clear and well supported, but this is a case study, and I think there should be a lot more acknowledgement and discussion of the nuances in the variability of the spring bloom in the Oosterschelde; it has not always followed the described pattern. More specifics are provided below:

Response: The referee's constructive suggestions are much appreciated. We have revised the manuscript and provided the suggested information. Please see the following point-by-point replies.

General Comments
1. More information and discussion of the field data
(a) Page 6, line 17-20: There is some discussion in here about how nutrients, light, and temperature effect the phytoplankton biomass annual cycle, but none of the data presented demonstrate or support these claims. What is this based off of, other people's findings or the authors own analysis? If these are the authors own conclusions, then I would like to see data and analysis to support these claims.

Response (G1a): Thanks for the comment. We have added the model results of temperature, nutrients, and light factors affecting the growth rate (Fig. 8) and explained the seasonal controls of phytoplankton variability in Section 4.2 (Page 7 Lines 19–29 in the "accept-changes" version of the revised manuscript). In winter, the low chl-a is a result of low temperature and light conditions that improve in March and April and initiates the spring bloom. The bloom algae consume nutrients and lead to post-bloom nutrient limitation. In late summer and early fall, light, nutrients, temperature still fuel high growth rate, while the low biomass results from grazing. When temperature and light start constraining primary production, the chl-a decreases and nutrients accumulates, entering the next annual cycle.

(b) A figure of DIN concentrations, similar to figure 4, would be beneficial.

Response (G1b): Thanks for the suggestion. We have plotted the 19-yr timeseries of

DIN. DIN at the offshore station RWS2 is consistently lower than RWS1 and other inner-bay stations, while the stations inside the Oosterschelde show little difference in DIN concentration from each other (Fig. R1). Thus, the DIN spatial heterogeneity in the bay is not as strong as chl-a (Fig. 4). Given that the DIN annual cycle is presented in Fig. 6, Fig. R1 is not included in the manuscript.

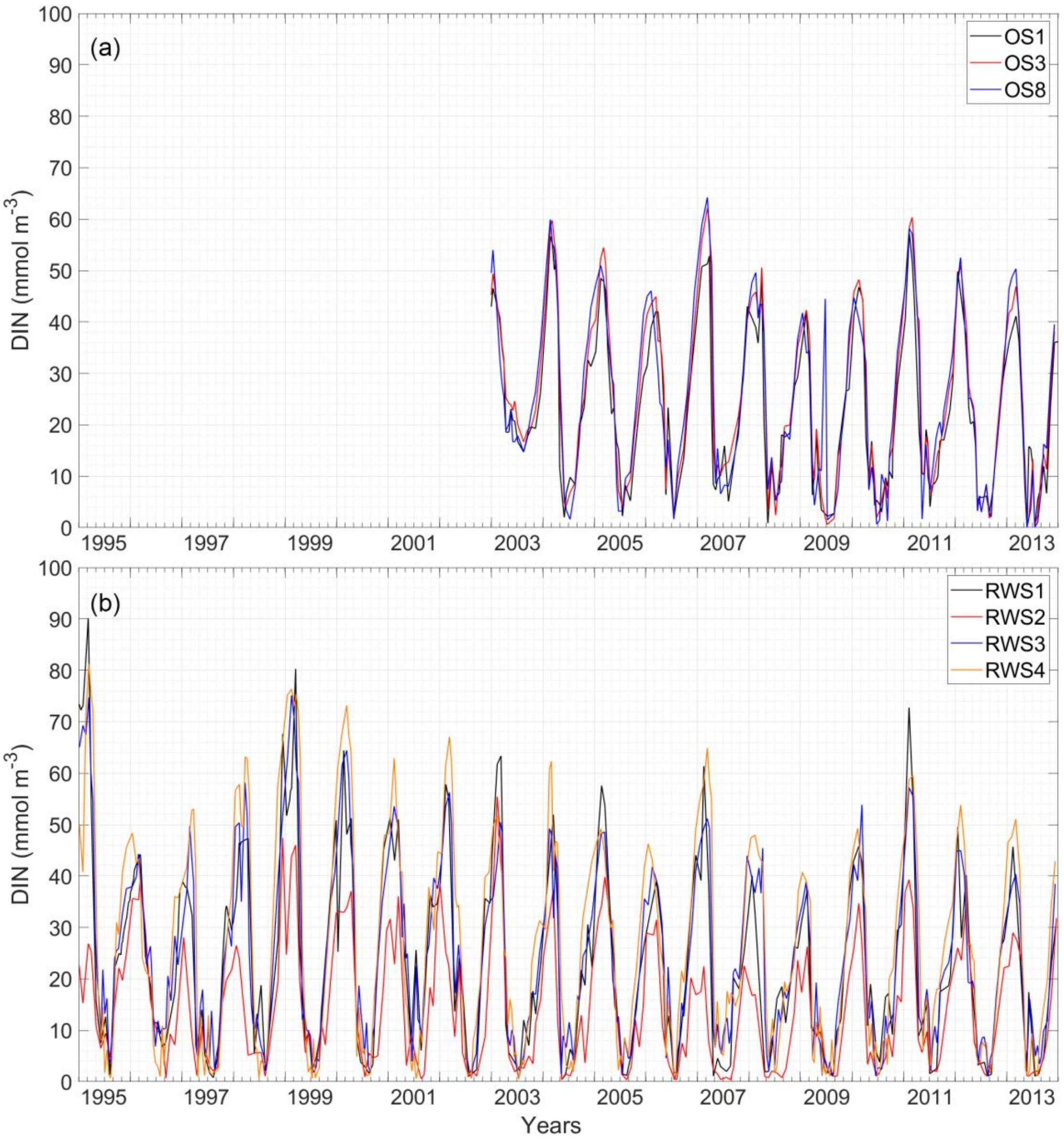

Figure R1: Time series of DIN (dissolved inorganic nitrogen) concentration during 1995–2013 at (a) NIOZ stations OS1, OS3, and OS8 and (b) Rijkswaterstaat stations RWS1–RWS4. Intervals between grid lines indicate two months. See Figure 2 for station locations.

(c) Why is OS6 not included in figure 2?

Response (G1c): OS6 is to the north of OS5 and not in the model domain. Thus, OS6 is not presented in the map.

(d) Page 4, line 16-17: More information on the 14CO2 uptake experiments would be helpful - Who did the experiments? At all stations? For all sample dates in 2010? Is this data published elsewhere?

Response (G1d): One of our co-authors Jacco Kromkamp did the experiments. The measurements were done at stations OS2 and OS8, as shown in Fig. 7, and yes, for all sample dates in 2010. The data is not published before. We have added the suggested information to Section 3.1 and Author contributions. Please see Page 4 Lines 21–22 and Page 13 Lines 8–9 in the "accept-changes" version of the revised manuscript.

(e) The SD bars in figure 2 are large for both gradients, suggesting a wide range of [chla] at all stations during spring between 1995 and 2013. The average values show a tendency towards higher [chla] at the mouth, but the large SD demonstrate a lot of inter-annual variability. Based on figure 4a, it appears that the pattern in spring phytoplankton biomass described in this paper dominated between 2000 and 2009. Pre-2000, [chla] at the head and mid-bay repeatedly matched or surpassed the mouth, 1998 being the clear exception. After 2009, [chla] during spring appears to become less distinct between each location. This does not negate the conclusions of the paper, but the years that do not match the pattern should be acknowledged. It is not expected that every year will always be the same, so what might have happened in the years that didn't follow the pattern?

Response (G1e): Good point. We agree with the referee that the spatial chl-a gradient shown in Fig. 2 is not universal in every year in Fig. 4. The gradient is obvious overall (Fig. 5), especially in some years (such as in 2005 and 2008 in Fig. 4a) and not so much in other years (such as in 1997 and 2012 in Fig. 4a). There are several potential causes.

Firstly, due to shallowness and being surrounded by land, water in the bay head is heated up slightly faster than that in the North Sea, so the spring bloom sometimes occurs several days earlier. If the sampling activity happens during the bloom in the North Sea, the chl-a distribution shows a seaward increasing trend. However, if the measurement is undertaken during the bloom in the bay, the bay head may exhibit higher phytoplankton biomass. For example, the peak biomass at the seaward end occurred later than at the landward end in Years 1996, 1997, and 2005 (Fig. 4b), whereas the spring bloom of the entire basin happens almost at the same time in Years 2002 and 2008 (Figs. 4a and 4b). Moreover, the chl-a concentration tends to be slightly higher in the landward end in February (Fig. 5). Thus, the interannual variability of the spring bloom timing can contribute to changes in the spatial chl-a gradient.

Secondly, the sampling frequency is usually biweekly or even monthly, so one data point in the time series may miss the peak bloom or result in different spatial chl-a distribution. For instance, in 2010, the overall magnitude of the spring bloom is relatively low, and the spatial chl-a gradient is more obvious in the RWS data than in the NIOZ data due to different sampling dates (Figs. 5a and 5b).

Given the above concerns, the seaward increasing chl-a gradient may not be discernible in one year's data. Therefore, we present the gradient with the average biomass in March to May during 19 years (Fig. 2), as well as by month (Fig. 5). The large standard deviations in Figs. 2 and 5 do include the interannual variability as the referee suggests. Note that the phytoplankton biomass before, during, and after the spring bloom also creates large variability in three months, which is another source of the presented standard deviations. We have added a paragraph in Discussion including these effects of temporal variability on the spatial phytoplankton distribution. Please see Page 10 Lines 3–13 in the "accept-changes" version of the revised manuscript.

2. More information and discussion of the model results (a) Figure 6, 7, 9: Are the field observations in these figures averages? If there is any way to calculate the standard deviation for these values, it should be included.

Response (G2a): The mesotidal Oosterschelde is well-mixed vertically (see the CTD casts of temperature and salinity at OS7 in Fig. R2 for example). For DIN and chl-a observations shown in Figs. 6 and 9 (now Fig. 10), the NIOZ and Rijkswaterstaat monitoring programs use the near-surface sample to represent the entire water column. Thus, unfortunately, there is no three measurements or replicates at each station to calculate the standard deviations.

For the *NPP* data in Fig. 8 (now Fig. 9), light attenuation was measured once during the cruise and the overall primary production is an integration of the entire water column. That is, we have only one *NPP* result for each station on each sampling date and are unable to calculate the standard deviations for *NPP*.

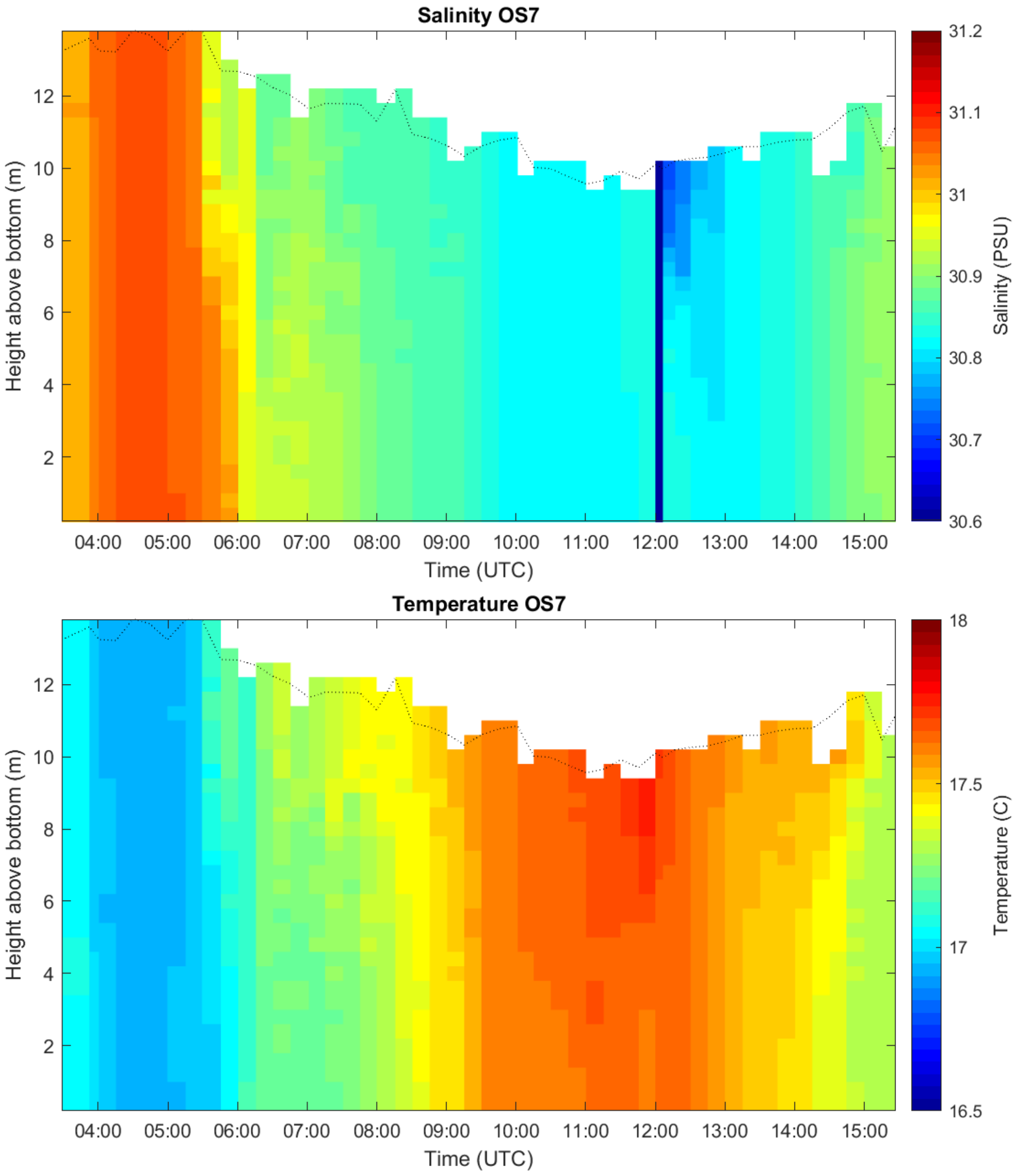

Figure R2: Observed salinity and temperature at OS7 during one full tidal cycle on 4 June 2019. The station location is showed in Figure 2. The water level is shown with the dotted line.

(b) Page 7, line 12: The authors mention that before the bloom, phytoplankton biomass and growth rates were low, but this data is not really presented. I know [chla] = phytoplankton biomass, so maybe keep the terminology consistent in the paper. However, the growth rate data from the model is something that I think should be presented, it sounds like it is interesting.

Response (G2b): Thanks for the comment. Primary production is the product of phytoplankton biomass and growth rate. In our model formulation, growth rate is a function of temperature, nutrient, and light factors, as shown in Equation (2) in Table 1. We have extracted these three factors from the model (Fig. 8). Their product should be proportional to the growth rate and be used to denote the relative changes in the growth rate (Fig. 8). The discussion based on biomass and growth rate has been updated with the new figure included. Please see Page 7 Lines 19–29 in the "accept-changes" version of the revised manuscript.

(c) Page 7, line 17-18: I wanted more detail here, rather than saying NPP is generally higher at OS8 compared to OS2. The authors note that the model overestimates NPP at OS8 in the fall 2010, but that overestimation makes it difficult to compare the two sites in figure 7. What is the average + SD observed NPP at each station? Is it significantly different? On average, how much higher is NPP at OS8 compared to OS2 in observed and modeled data?

Response (G2c): Thanks for the suggestion. The observed *NPP* at OS8 and OS2 is 902.6 ± 928.4 mmol C m^{-2} d^{-1} and 722.5 ± 794.6 mmol C m^{-2} d^{-1}, respectively. Although the observed NPP at OS8 is slightly higher, their difference is not significant according to the t-test ($t = 0.59$, $p > 0.05$, $n = 16$). In contrast, the modeled *NPP* at OS8 (1033.9 ± 1084.3 mmol C m^{-2} d^{-1}) is significantly higher that OS2 (606.0 ± 499.5 mmol C m^{-2} d^{-1}) according to the t-test ($t = 6.85$, $p < 0.05$, $n = 365$). We have added these statistical analyses to the revised manuscript. Please see from Page 7 Line 29 to Page 10 Line 2 in the "accept-changes" version of the revised manuscript.

(d) Figure 7: The authors explain the overestimation at OS8, but why did the model miss the highest peak in NPP at both stations around day 175?

Response (G2d): Good question. The modeled *NPP* is influenced by temperature, nutrients, light, and phytoplankton biomass. Light and temperature is reasonably simulated by the hydrodynamic model (Jiang et al., 2019), and the slight deviation of simulated chl-a (Day 540 in Fig. 6) cannot fully explain the underestimation of *NPP*, which, thus, is related to nutrients. Fig. 7 show that the underestimation of *NPP* actually starts right after the spring bloom (Day 125) and lasts till around Day 175. This is a period that DIN is underestimated (Days 490–540 in Fig. 6). As discussed in the manuscript, this results from our N-based model excluding the effect of phosphorus or silicon limitation. Therefore, DIN is depleted too early and thus limiting *NPP* (Fig. 8), which should decline more slowly or even increase slightly (Fig. 7). We have added these discussions in Sections 4.2 and 5. Please see Page 7 Line 27 and Page 9 Lines 6–10 in the "accept-changes" version of the revised manuscript.

Reference

Jiang, L., Gerkema, T., Wijsman, J. W., and Soetaert, K.: Comparing physical and biological impacts on seston renewal in a tidal bay with extensive shellfish culture, J. Mar. Syst., 194, 102–110, http://doi.org/10.1016/j.jmarsys.2019.03.003, 2019.

3. My personal opinion is the synthesis section (6) does not belong in this paper. Figure 12 and all the work the authors did is very interesting, but does not fit with the rest of the paper. Figure 12 and the synthesis sections should be its own paper. There is a lot of information in figure 12 that deserves more than three paragraphs of explanation. A case study of chlorophyll a spatial pattern in the Oosterschelde is not the place to propose a categorization of chlorophyll a spatial patterns for all estuaries.

Response (G3): Thanks for the comment. Section 6 is originally part of Discussion and aims to put the Oosterschelde in broader context of global estuaries and coastal bays. In comparing the different spatial phytoplankton distribution patterns, main drivers of the phytoplankton gradient are discussed. The general lesson from this section is that the spatial phytoplankton distribution is usually shaped by one or more environmental drivers that differ from system to system. This is the basis of the "general implication" in the title. Therefore, we consider this part important in terms of extending the discussion on key environmental controls of phytoplankton distribution and gaining the interest to a broad audience and tend to retain it. Two other referees are also in support of this section.

Specific Comments

1. Introduction Page 2, line 28 – Page 3, line 1: Include the pros and cons of all three methods. No cons are mentioned for ecological methods.

Response (S1): If we understand this correctly, the referee suggests us to describe the cons of numerical ecological models, not ecological methods. The drawbacks or limitations of numerical models are they are based on simplified assumptions and cannot simulate every detail in nature. Thus, a useful or reliable model has to be calibrated and validated by observed or measured in situ data. We have added the above information to the paragraph. Please see from Page 2 Line 34 to Page 3 Line 1 in the "accept-changes" version of the revised manuscript.

2. Methods Page 5, line 24-26: The model output results went through two conversions to match the observation data (N->C, then C->chla). There should be some mention of the assumptions and limitations of these conversions because they are not perfect and there has recently been growing criticism of the C:chla ratio.

Response (S2): Thank you for the suggestion. Our text may be a bit confusing here. The model is nitrogen-based. Two conversions were involved. (1) The Redfield C:N ratio was used to convert the nitrogen-based modeled NPP to the carbon-based measured NPP as shown in Fig. 7. (2) The Chl:N ratio (Soetaert et al., 2001) was used to convert the modeled nitrogen-based phytoplankton biomass to the chl-a concentration. So the C:Chl-a ratio was not applied here. It is true that these ratios are variable in nature and our simplification in the model is not without limitations. We have also claimed the limitation in the first paragraph of Discussion. Please see Page

9 Lines 11–13 in the “accept-changes” version of the revised manuscript.

Reference

Soetaert, K., Herman, P. M., Middelburg, J. J., Heip, C., Smith, C. L., Tett, P., and Wild-Allen, K.: Numerical modelling of the shelf break ecosystem: reproducing benthic and pelagic measurements, Deep-Sea Res. Pt. II, 48, 3141–3177, http://doi.org/10.1016/S0967-0645(01)00035-2, 2001.

7. Summary Page 11, line 24-28: These last two sentences do not accurately sum up the paper. There is no discussion of temporal variability in the spatial distribution of phytoplankton in Oosterschelde and I did not get an understanding of phytoplankton’s role as an ecological indicator. The paper would be greatly strengthened by a discussion of the temporal variability and what caused it.

Response (S3): Thanks for the comment. We agree that the manuscript focuses on the spatial pattern, which cannot be discussed without temporal variations. These two sentences have been rephrased to emphasize the temporal changes and natural and anthropogenic controls on phytoplankton distribution. Please see Page 12 Lines 22–28 in the “accept-changes” version of the revised manuscript.