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# *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



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Discussion paper

described pattern. More specifics are provided below:

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. (b) A figure of DIN concentrations, similar to figure 4, would be beneficial. (c) Why is OS6 not included in figure 2? (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? (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?

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. (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. (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? (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?

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.

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.

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.

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.

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