

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.

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General Comments This paper describes a coupled observational, modeling and satellite observational study of an estuarine system in the North Sea. Overall, the story and results were well conveyed and the conclusions regarding drivers of spatial and temporal variability in the estuary were supported. The main take away is that there is a Type I phytoplankton distribution and it is mainly driven by benthic grazing pressure in the landward stations. The model supports the importance of grazing pressure on the spatial distribution by numerically removing bivalves in the modeling system. Modeling estuarine primary production and chl-a distribution can be particularly challenging,

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and I think the author's did a pretty good job at capturing overall NPP magnitude and some of the temporal variability, compared to 14-C NPP incubation data. The synthesis at the end is particularly useful, especially related to the discussion of how different mechanisms can lead to similar patterns of chl-a distribution, depending on the system.

The main methodology and results that need to be improved upon, or omitted, relates to the use of the satellite observations. The author's use one image (Fig. 10) and it doesn't really track with the results and conclusions of the rest of the paper. In fact, the chl-a concentration is highest in the landward stations where in most observations showed lower chl-a concentration. I understand the desire to do this coupled methodological approach, but in my opinion if satellite data is to be used, it should be developed a bit more to support the observational and modeling work. There is definitely a lot of value in using these data, but acquiring more spring bloom images from MERIS data that fall within the observational window would offer a bit more support for the other results.

Specific Comments Page 2 Line 10: This sentence with the semi-colons is oddly structured, consider revising because the information is good.

4-20: "Light attenuation was measured . . ." How specifically was light attenuation measured and with what instrument?

4-25: "We used the measured values . . ." I don't quite understand this sentence, consider revising

5-510: What weather forcing was used, specifically, and how was surface irradiance specified?

5-15: I see in the equations, detritus sinking is also calculated, but perhaps mention that here as well

5-30: From what I can tell the bivalve biomass is constant, but perhaps clarify that here. Are the bivalves growing and dying or are they constant in time?

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6-20:25: It would be useful to show some kind of climatology of the measurements with a window or errorbars that show the inter-annual variability. See figs in Testa, J. M., Murphy, R. R., & Brady, D. C. (2018). Nutrient-and climate-induced shifts in the phenology of linked biogeochemical cycles in a temperate estuary. *Frontiers in Marine*. <https://www.frontiersin.org/articles/10.3389/fmars.2018.00114>

9-20: Does the decreasing depth (presumably) also cause the benthic-pelagic coupling to become stronger? Are there bivalves in the more seaward stations but because there is a greater volume of water the grazing pressure just is less, on an areal basis?

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