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Interactive Comment

Interactive comment on "Control of phytoplankton production by physical forcing in a strongly tidal, well-mixed estuary" *by* X. Desmit et al.

Anonymous Referee #3

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Referee Review of "CONTROL OF PHYTOPLANKTON PRODUCTION BY PHYSICAL FORCING IN A STRONGLY TIDAL, WELL-MIXED

ESTUARY" by X. Desmit, J.P. Vanderborght, P. Regnier, and R. Wollast

General comments ————-

This paper describes a study in which a numerical model of estuarine phytoplankton dynamics is used to explore the high-frequency relationships between turbidity and incident solar irradiance controlling phytoplankton growth in a tidal water column. The authors use the model both under idealized conditions (e.g. prescribing forcings with simple sine functions) and under realistic conditions based on measurements from a real estuary. I find this research area of high-frequency interactions a very interesting and valuable one that deserves increased attention in estuarine and coastal science. I see the authors' use of a model to perform this exploration as an excellent approach

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and a perfect and logical way to make use of models. I also believe they generally did a good job of addressing their assumptions (e.g. as regards photoacclimation, sedimentation and resuspension). I generally found this paper very well written and the figures useful and informative.

However, I do not think the authors explicitly demonstrated some of the things they concluded. For example, the authors conclude that the ratio of euphotic depth to mixed depth could control net phytoplankton growth. Although this certainly makes intuitive sense, I did not see anywhere that this ratio was explicitly calculated. It was also unclear to me where the authors were speaking of the mean value of this ratio versus a ratio that fluctuates due to high frequency variations in light attenuation coefficient. This ambiguity of timescale was something I found generally confusing.

I really like what the authors have done so far with this study, but recommend some extensions of their simulations and additional calculations to allow them to conclusively say something about the overall importance of high-frequency variations in turbidity and the ratio of euphotic depth to mixed depth.

Scientific issues ——————

1) The authors speak repeatedly of the significance of the ratio of the euphotic depth to the mixing depth in determining net phytoplankton production. In fact, the stated "main purpose" of the study (p. 40, l. 11-14) was to investigate whether positive growth can be sustained...where mixing depth is larger than euphotic depth. My understanding is that a) net integrated phytoplankton production in the water column is determined by the ratio of the CRITICAL depth to the mixing depth, and b) that critical depth (defined as the depth at which vertically integrated photosynthesis equals vertically integrated respiration) is different from the euphotic depth (commonly taken as equivalent to the compensation depth, the depth at which local photosynthesis equals respiration). I therefore believe it is well known that net water column production of phytoplankton may be positive if mixing depth is greater than the compensation (or euphotic) depth

but less than the critical depth. Of course, the critical and euphotic depths are related and perhaps euphotic can be taken as an indicator of critical. The authors obviously know these terms and definitions. Nonetheless, I wonder whether the authors mean to emphasize the ratio critical depth:mixing depth instead of euphotic depth:mixing depth. If I am incorrect, I suggest the authors explain in more detail why the euphotic:mixing depth ratio is the important quantity here and why we should be surprised if positive growth occurs where that ratio is less than one. Furthermore, I do not believe the authors ever presented actual values of this ratio; without such a quantitative basis, some of their conclusions, though intuitive, are not substantiated.

2) The modeling approach appeared sound and appropriate for this study. Well done.

3) I have not before seen a description of in situ measurement of Kd, but am interested in learning more about it. Can the authors include a citation for this?

4) Is self-shading by phytoplankton accounted for in the model? Why or why not? (Or why might it not matter?)

5) It may be useful to the authors to see Lucas & Cloern 2002 (Estuaries, V25, No.4A, pp. 497-507), which describes a very similar modeling approach for examining issues extremely closely related to the Desmit et al. study. Lucas & Cloern also devised a zero-dimensional model to look at high frequency interactions between the light field and long-term net phytoplankton growth in a tidal water column that oscillates in depth. Those authors did not examine high frequency fluctuations in Kd but instead focused on the effects of differences in the light field (and, incidentally, in benthic consumption) driven by tidal fluctuations in water column depth (Zmax). Desmit et al. and Lucas and Cloern appear to have conducted 2 nicely complementary studies in that they address closely related yet distinct issues in a similar way and draw very similar panoramic conclusions (e.g. the high-frequency stuff really matters and if you don't consider/model it, your answers may be substantially wrong). This reviewer wonders whether Desmit et al. have considered the effect of fluctuating Zmax in their results as it relates to

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the fluctuating depth-averaged light field. The Desmit et al. paper is focused more on turbidity-driven high frequency variability in the light field as opposed to fluctuating Zmax, but their model appears to have incorporated a fluctuating Zmax; therefore, this effect could be explored with the model they have. It is certainly possible that the large fluctuations in Kd considered here dwarf any fluctuating Zmax effect, or that the estuarine regimes modeled here are not subject to the fluctuating Zmax effect. Nonetheless, I suggest the authors address this somewhere.

One way to start this exploration, and to concurrently explore more deeply the euphotic:mixing ratio, might be to actually calculate Z_euphotic:Zmax as part of Fig. 3. The data is all there. If Z_euphotic:Zmax is in fact the important indicator ratio here, then seeing how it varies versus Kd and versus Zmax would be enlightening.

6) Figures 11-12 are really nice and very demonstrative—a great way to use a model. They show very clearly why we need to look at the high frequency details of the underwater light field. Why did the authors choose to show PAR at 20 cm depth? Obviously, GPPz(t) looks to follow closely PAR_20cm(t), which is just a function of Eo and Kd. But I wonder if a more useful quantity would be total or average PAR in the water column. This quantity could show the effects of fluctuating Zmax, if there were any. Because of the apparent close relationship between GPPz and PAR_20cm, I suspect the fluctuating Zmax effect may be negligible here. Why?

7) For what average Zmax was Fig. 12 calculated?

8) What I think is the most important point from the first set of simulations is somewhat buried in the text on p. 49. That point is that the LONG-TERM effect of fluctuating Kd and therefore fluctuating GPPz is significant. I suggest combining Figs. 11-12 and adding a third panel showing the long-term trajectories of computed phytoplankton biomass for the varying and constant Kd cases. This would demonstrate why we should care that Kd and consequently GPPz actually vary in the short-term. The authors could also do this for a different mean Zmax, to compare/contrast the importance of varying

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Kd across a range of mean depths (I suspect the impact of varying Kd might be less for deeper water columns). If the authors did this, then Fig. 13 may be unnecessary. This comparison across depths would allow the authors to guide readers in terms of where the fluctuating Kd is important in determining long-term dynamics and where it is not. Under what conditions might the entirely wrong trend direction (positive vs. negative) in net growth be predicted if varying Kd is not considered?

9) It may be useful for the authors to see May et al. 2003 (MEPS 254:111-128). Those authors did an analysis similar to that suggested above in #8, but that study focused more on the effects of variable wind on turbidity and, ultimately, on phytoplankton growth.

10) I really like the application of the model to the Scheldt estuary and the fact that the model results are consistent with the observations of higher biomass at the more turbid site and lower biomass at the clearer site (Figs. 14-16). I agree that these results are consistent with the idea that the "euphotic to mixing depth ratio is the principal controlling factor of phytoplankton dynamics in this type of estuary." However, I do not believe the authors have explicitly demonstrated this. Also, I am not sure whether the authors are speaking of the mean or fluctuating ratio as being the controlling factor– from what I see, either could be important. I suggest the authors first compare the 2 (shallow and deep) varying-Kd simulations with new simulations using constant mean Kd. If the constant Kd cases produce the opposite of the trends in Fig. 15 (e.g. chl at the shallow site decreasing and chl at the deep site increasing), then I think the authors can safely conclude that fluctuating turbidity is a significant factor regulating the observed bloom dynamics. If the mean and varying Kd cases produce similar results, then fluctuations in Kd may not matter very much and perhaps it's really just a parameter like mean Z_euphotic:Zmax that matters. If varying light field looks to be relatively unimportant for the case of the Scheldt, then perhaps a BZE model would work there. In such a case, I do not know whether the application to the Scheldt really fits in this high-frequency focused paper, since it would be an example of where the

high frequency interactions do not matter much. I would also suggest comparing these simulations with cases where Zmax is constant, to separate out that possible effect.

Furthermore, I suggest the authors explicitly calculate time series of Z_euphotic:Zmax for the Scheldt simulations and compare periods of a large ratio to periods when modeled net positive growth occurs. Then they may be able to conclude that this ratio, on whatever time scale, is controlling. They may also then be able to glean something to quantitatively indicate how "high" the ratio needs to be for positive growth (see p. 52, l. 20).

11) I am confused whether, overall, the authors are trying to use this paper to demonstrate the importance of short-term variations or mean values of the ratio of euphotic depth to mixed depth. The abstract referred to "short-term" (p. 38, l. 9), and much of the modeling work is devoted to teasing apart the short-term relationships. However, the discussion of the Scheldt simulations on p. 51 (l. 15) leaves me thinking that perhaps they are talking about mean values. I suggest the authors carefully delineate between the two concepts in this paper and make clear when they are speaking of one versus the other. Further, I think these two related but different concepts could be used to contrast two different ways of conceptualizing, measuring, and modeling estuaries: 1) as systems where mean quantities describe well the long-term dynamics and where the high-frequency fluctuations don't really matter, and 2) systems where high-frequency interactions have a significant impact on long-term trends.

Minor technical issues ——————-

1) p. 39/l. 23: I think it would clarify to add "short-term" or "hourly" or "high-frequency" in front of "suspended particulate matter (SPM) dynamics," if that is what is intended.

2) p. 46/l. 19-20: I have always taken tidal "amplitude" in its trigonometric sense, that being one-half the difference between the peak and the trough of a sine wave, or one-half the tidal range. Do the authors mean to say tidal "range" instead of "amplitude"?

3) p. 50/l. 17-18: Do the authors mean "average" Zmax?

4) I am not convinced Fig. 9 is necessary.

Interactive comment on Biogeosciences Discussions, 2, 37, 2005.