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

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Control of phytoplankton production by physical forcing in a strongly tidal, well-mixed estuary. X. Desmit, J.P. Vanderborght, P. Regnier, and R. Wollast

Answers to Dr K. Muylaert, Referee #1

(1) Dr Muylaert asks us to explain how the change in water surface has been taken into account, leaning on the following statement: "If the system consists of a channel with vertical banks (as suggested by figure 1), surface will increase during low tide". Our understanding of this statement is that a horizontal spreading of the water volume would be conceptually needed in order to explain the decline of the water level at low tide. In other words, the conservation of water volume would impose that a two-fold decrease in the water depth should be somehow compensated by a two-fold increase in the water surface area. We cannot agree with this representation, simply because

the water volume in an estuary is not constant with time, at least in a Eulerian approach where the estuary is defined by fixed geographic boundaries. We follow here this type of approach, because we are interested in local variations at a fixed position. We thus consider an idealised, unit-area water column of oscillating depth and thus, of varying volume. As pointed out by Anonymous Referee #3 (§5, p S50), this modelling approach is similar to the one of Lucas and Cloern (2002): these authors also used "a zero-dimensional model to account only for changing local dynamics (i.e., horizontal variability and transport are neglected)." On the contrary, we agree with the second part of the same remark ("When the morphology consists of a deep central channel with extensive intertidal shallows, theoretically, an inverse pattern could occur and surface might decrease during low tide"). However, the morphology of the two sites selected in our paper (the so-called "shallow area" at km 120 and "deeper area" at km 80) does not include any extensive intertidal flats. This particular point is discussed in more details in our answer to Referee #2.

(2) The second comment of Dr Muylaert concerns Fig. 11-12 of our text, which he would like to see combined into a single figure. He also suggests a more detailed representation of the integrated primary production over time. This point is addressed in details in our answer to the comments 8 and 10 of Referee #3. Our revised paper contains modifications that comply with the suggestions of both referees, who have similar views on this specific point.

(3) The third comment deals with the role of transport processes on phytoplankton dynamics. We fully agree that transport plays an important role on phytoplankton distribution in estuaries. However, in a zero-dimensional modelling approach, which does not take transport into account, it is conceptually impossible to draw any conclusions about residence time, in particular about "the minimum water residence time in the estuary required to allow for positive phytoplankton growth". Achieving this objective requires a complete description of the longitudinal gradient using a 1D, 2D or even 3D transport-reaction model, which is out of the scope of the present paper: our pur-

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pose here is to explore how physical forcings affect the rate of phytoplankton growth and its integration over the depth. We only claim here that, in the absence of lateral biomass input, an increasing biomass concentration along the longitudinal axis is only possible if the depth-integrated phytoplankton growth, as estimated with the present zero-dimensional model, is positive. This is a necessary, but not a sufficient condition. On the other hand, a negative depth-integrated phytoplankton growth in the zero-dimensional model will always result in a decreasing concentration gradient in the estuary, whatever the water residence time.

(4) Concerning Dr Muylaert's comment on the diel variation of the photosynthetic parameters, we think that the various results presented in our paper clearly demonstrate the possible impact of this factor. We indeed show that a diel variation of aB and PBmax not only affect the phytoplankton growth. It also introduce a harmonic coupling between aB, PBmax and kd and amplify the importance of using a time-dependent kd. We therefore fully agree with the remark that diel variations "may be important enough to make the difference between positive and negative population growth", and we have modified our revised paper accordingly by adding a specific comment.

(5) Dr Muylaert finds that the description of the model is in several places wordy and suggests that we drastically shorten the explanation concerning photo-inhibition and why it is not included in the model. This is unfortunately not in phase with the general comments of Ref. #3 who believe that we "generally did a good job of addressing [our] assumptions". For the sake of clarity, we therefore chose not to follow this suggestion.

New references

Lucas, L.V. and Cloern J.E.: Effects of tidal shallowing and deepening on phytoplankton production dynamics: a modelling study, Estuaries, 25, 497-507, 2002.

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