

## ***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 Anonymous Referee #2

(1) In his first specific comment, Referee #2 considers that "the main flaw of this paper is that the authors ignore the effect of bathymetry on algal growth, i.e. the occurrence of shallow areas (tidal flats)". We are surprised by this remark. First of all, we certainly agree that, as stated by Ref. #2, "the balance between light-limited net growth and biomass-driven loss processes changes drastically along the bathymetric gradient, i.e. from the shallows to the deep channel". We would like to recall that computing this balance by depth-integration of the production and loss terms is at the centre of our modelling approach (see equations 1 and 4). However, the main focus in our paper is

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clearly not on the role of the spatial (horizontal) variability of the water depth along the estuarine cross-section or longitudinal profile: this aspect has already been explored very convincingly by a number of authors (see hereunder). Our main attention has been directed towards the importance of the temporal, short-term variability and interaction of physical forcings, including the tidal fluctuation of the water depth. We thus insist that our paper is not dedicated to studying the role of extended shallow zones and their interaction with deeper channels, although we are well aware that this type of environment presents a wide extension in the Dutch part of the Western Scheldt (Hooge Platen, Molenplaat, Ossenisse, Valkenisse, ..., all situated downstream of km 60). On these tidal flats, benthic algal growth certainly plays a significant role in the overall budget of primary production (see for example de Jong & de Jonge 1995). But we explicitly state that our focus is on "a turbid, well-mixed estuary, where the mixing depth is larger than the euphotic depth": we therefore clearly address in our paper the case of estuarine areas where benthic production can be neglected. Finally, we would like to emphasize that our purpose is not to develop another model of the Scheldt estuary, but rather to follow the same approach than the one used by Lucas et al (1999a and 1999b cited by Ref. #2) in their study of the processes governing phytoplankton blooms in the South San Francisco Bay (SSFB) using the TRIM-BIO model: "We use numerical experiments of phytoplankton population dynamics (...) to explore the sensitivity of estuarine phytoplankton dynamics to various processes, as opposed to faithfully reproducing in detail an observed bloom (...) Therefore, although we use SSFB as a natural laboratory, (...) our goal is to gain insight into physical-biological mechanisms potentially important to a broad class of estuaries." We thus do not see the point of using "more realistic morphology" when applying our model, considering the purpose of the present work. Of course, we certainly do not pretend that a correct description of the spatial variability is not needed in estuarine models. We only show here that the temporal, short-term variability of a number of parameters should also be taken into consideration.

Following the suggestion of Ref. #2, we have included in the revised version of our

paper a reference to the Lucas et al (1999a; 1999b) paper cited above, as well as to May et al (2003). More specifically, we acknowledge the fact that these authors have already explored how the short-term variations of a number of physical and/or biological parameters influence estuarine production (in particular: water depth and its interaction with bottom grazing, wind-driven resuspension). None of these authors have however considered the influence of tidally-induced sediment resuspension and its coupling with the light cycle. Lucas et al (1999 b), in their discussion over physical-biological phasing, conclude that "other mechanisms of hourly scale variability not included in TRIM-BIO - such as short-term variations in water column irradiance caused by tidally driven sediment resuspension and diel light cycle - could contribute additional hourly scale physical-biological phasing processes", which is precisely the scope of our paper.

(2) In a second comment, Ref. #2 asks if using a geometric instead of an arithmetic average would improve the estimation of GPP based on a constant (averaged)  $k_d$ . In our model, the relationship between GPP and  $k_d$  is not a simple exponential, as stated by Ref. #2. It is actually a "double" exponential (see equations 6 and 12), and therefore does not become linear after a log-transformation. We have however tested the suggestion of Ref. #2 and extended the first set of simulations (the case of the so-called "simple", sinusoidal forcing functions) to the case of a log-averaged extinction coefficient. The net algal growth has been estimated over a 30-days period for an average depth of 8 m (i.e. a tidal variation of the water depth between 5 and 11 m). We compared the results (not shown) computed with respectively a time-varying  $k_d$  (sinusoidal), a constant, time-averaged  $k_d$  (arithmetic mean of the former), and a constant, time-averaged  $k_d$  (geometric mean). While a time-varying  $k_d$  allows to compute a positive net algal growth with time, both the 30-days profiles obtained with an average  $k_d$  (arithmetic and geometric) exhibit a decrease with time. These two profiles are almost similar and clearly prove that log-averaging has a limited influence on the final result. In this particular case, it does not even shift the response from negative to positive growth.

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(3) Ref. #2 criticizes us for the "very uninformative" way with which some settings of our model are reported. This remark is followed by one example only, i.e. the undocumented astronomical routine used for the computation of incident light. This missing information is now given in the revised paper: the total incident light energy  $E$  at the top of the earth atmosphere is computed from the sun elevation ( $\sin b$ ), the earth orbital radius ( $R$ ) and the solar constant ( $K_{\text{sol}} = 1367 \text{ W.m}^{-2}$ ):  $E = K_{\text{sol}} \sin b / R$ , where the sun elevation and the orbital radius are taken from Meeus (1998). In the case of a cloudless sky, a constant factor for light transmission through the atmosphere is considered (72%). Reflection at the water surface is taken as a constant value (6%). The PAR fraction of the total incident solar light is equal to 0.5. In the presence of a cloud cover ( $CC$ , expressed as a fraction between 0 and 1), a nebulosity factor  $Neb$  is taken into account, according to:  $Neb = 1 - 0.585 CC$  (adapted from Vila et al 1996).

To answer to the last sentence of this 3rd comment ("What really matters here is the daily light dose (PAR averaged or integrated over the day) AND the day length (i.e. how the light is partitioned over the day), we have extended our discussion on the effect of the short-term variability of the physical forcings. This is presented in more details in the answer to Ref.#3, comment 10.

(4) Concerning the technical corrections, we fully acknowledge the remark of Ref.#2 concerning the units in the various equations. An extra chlorophyll-to-carbon ratio is indeed missing in equation 1, which should read:

The correction, together with a remark on the units used, is incorporated in the revised paper.

New references:

de Jong, D.J. and de Jonge V.N.: Dynamics and distribution of microphytobenthic chlorophyll a in the Western Scheldt estuary (S.W. Netherlands), *Hydrobiologia*, 311, 21-30, 1995.

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