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Biogeosciences Discussions

Interactive comment on "Factors governing the pH in a heterotrophic, turbid, tidal estuary" *by* A. F. Hofmann et al.

A. F. Hofmann et al.

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Based on the constructive comments of all reviewers, we substantially improved our manuscript. The changes are too many to list individually and the most important, central improvements are summarized in our integrated reply to all referee comments which can be found in our top-level author comment "Integrated reply to referee comments". Here, we restrict ourselves to the outline of important changes based on the comments of reviewer # 2. Furthermore, we mention briefly why we do not agree with some of the comments of this reviewer. Also here, we restrict ourselves to the most important points.

Based on the comments of reviewer # 2, we briefly explained the reason for a changing sign in the contribution of advective-dispersive transport to the rate of change of

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protons. We added more information/explanation on the technicalities of the model. We created whole estuarine proton bugets for three separate parts of the estuary: the upstream, midstream, and the downstream part. We also focused more on the explanation of the reasons for the distinct longitudinal (yearly averaged) pH profile along the estuary. We improved all our figures for better reading and more information content, amongst others by adding numerical values in the graphs themselves, where appropriate. Furthermore, we cited and briefly discussed Frankignoulle et al. (1996); Abril et al. (2000).

We feel, that our model represents reality in the Scheldt estuary reasonably well for our purposes. Especially, since it only serves as a case study to introduce our pH modelling method. Concerning the doubts of reviewer # 2 we would like to mention that the model presented in Hofmann et al. (2008b) is exactly the same as the one underlying the current paper. In Hofmann et al. (2008b) we do not argue that our model underestimates CO_2 degassing, but we argue that CO_2 degassing did actually decrease from the 1990ies towards the first decade of the 21^{st} century, and that previous higher estimates might be overestimates. However, to be correct, we agree that there are lots of uncertainties associated with CO_2 degassing estimates - also in our model. As shown in Hofmann et al. (2008b), the model has been compared to nitrification values and a very good fit was found. The importance of the parametrization of denitrification (also a mineralizing process) has been discussed in Hofmann et al. (2008b) as well. The primary production rates in the model along the estuary are similar to the ones from the MOSES model (Soetaert and Herman, 1993, 1995a,b). Together with the considerably good fit between model and measured (concentration) data (total ammonium, nitrate, oxygen, organic matter, and pH; see Hofmann et al. (2008b)) we are therefore confident that our model represents primary production and also oxic respiration reasonably well. Model test runs with four fold increased maximal mineralization rate parameters already show clearly underestimated organic matter concentrations and a recognizably worse model fit for oxygen, nitrate and total ammonium. Although the rel-

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ative importance of oxic mineralisation (and as a result, advective-dispersive transport) for the proton budget changes in the very upstream region of the estuary (this is the region where the labile organic matter concentrations is the highest allowing for the highest mineralisation rates), the relative importance of processes for the proton budget along most of the estuary does not change and the general pattern of the whole estuarine proton budget (Figure 5) also does not change substantially. This shows that (a) the oxic mineralisation parametrisation used in the model underlying this paper is constrained by the model fit (as given in Hofmann et al. (2008b)) and that (b) at the same time the general conclusions of the present paper are not very sensitive to the oxic mineralisation parametrisation.

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