Biogeosciences Discussions, 1, S205–S207, 2004
www.biogeosciences.net/bgd/1/S205/

Biogeosciences Discussions

Interactive comment on "Carbon dynamics and CO_2 air-sea exchanges in the eutrophied coastal waters of the southern bight of the North Sea: a modelling study" *by* N. Gypens et al.

Anonymous Referee #1

Received and published: 20 September 2004

The manuscript "Carbon Dynamics and CO_2 air-sea exchanges in the eutrophied coastal waters of the southern bight of the North Sea: a modelling study" by N. Gypens, C. Lancelot and A. V. Borges focuses on the carbon dynamics, notably on the control mechanisms of the CO_2 air-sea exchange in the southern bight of the North Sea. This area can be considered as an eutrophied system. A 3-box model is applied in order to unravel the different processes influencing the CO_2 air sea exchange, here explicitly biological processes, (seasonal) temperature effects and riverine inputs of organic in inorganic carbon.

Beside some detailed comments below, I have major concerns with the validation and thus the reliability of the model. It remains unclear, whether and how the model was initialized. Moreover, it is not clear, why the observational data where adapted to the simulated salinity and temperatures, rather than the other way round. There might be good reasons for this, however they are not clear to me. The major and obvious problem is however the validation of the CO_2 data, described in chapter 3.1.2. The model is very much unable to capture the feature, even of DIC end alkalinity. The differences for both values between observations and simulations are on the order of 100 or 200 mikromol, while the observational error is somewhere between 1 and 5 mikromol. Given these high discrepancies, it appears to be a miracle, why the pCO_2 is captured at least somewhat better. In my view, this however seems to be an incident, which must not be sold as a good model performance. A simple reason for this discrepancy might be found in the model initialization, however obviously I cannot assess this. Otherwise the model clearly reveals too high DIC and alkalinity values and moreover apparently misses an autumn sink for DIC and alkalinity as obvious from the much lower observational values. Again, the pCO_2 can only be considered as incidental. This sink for example could be the North Sea upstream of the BCZ, which would imply a carbon transport in northeasterly direction. For example, Borges and Frankignoulle (JMS, 19, 251, 1999) described a decoupling of production and respiration in this area, which would allow the export of organic matter to more northern areas. Still the alkalinity possibly points to a more physical problem with the simulations. It is also not clear from the text, whether the model was forced to reproduce the observational pCO_2, however chapter 2.1 seems to imply that the pCO_2 always is the results of the simulations. Any further version of the manuscript needs to clarify, how the calculations were performed and initialized in order to reproduce the carbon observations. The remaining investigations appear to be useful and interesting, however the above discrepancies do not enable such investigations. Since I see potential in the manuscript for improvement, I think the simulation, and thus the manuscript needs to undergo major revisions order to substantiate and verify the findings.

Detailed comments:

P562, l14: weak sink (rather than low)

P563, l5-27: I do not see a reason for confusing the introduction with low latitude or other rather unrelated studies. There are good studies mentioned in the introduction, which focus on similar areas and appear to be useful. Moreover, the sentence starting in line 15 (on the contrary) seems to imply a low latitude section, however continue with a wider range of studies. This is unclear and in part misleading, since no latitudinal dependence can be established hitherto and thus appears to be speculation. Even the introduction mentions source and sink regions in the mid and higher latitudes and the introduction should be confined to this. (By the way, Cai et al. 2003 clearly describe a sink for atmospheric CO_2 in their overall system and not a source.)

P566, l1-10: As mentioned above it is not clear, how the CO_2 model was run, how it was initialized and whether how it was fitted to the data. Please clarify.

Interactive comment on Biogeosciences Discussions, 1, 561, 2004.