

Interactive comment on “Carbon dynamics and CO₂ 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 #3

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This manuscript attempts to characterise the controlling processes on air-sea CO₂ exchange in the coastal zone. This domain is identified as important in terms of global carbon budgets and the topic as highly relevant to the prediction and management of global change.

However, due to the choice of model structure I feel that this paper claims too much. It examines two domains, the Belgian coastal and Western English Channel, but differentiates them principally by riverine influence. The papers conclusion that riverine influence is dominant may well be true but cannot be completely tested by this model experiment. The limited description of the physical model provided may be miss-leading but I got the impression that the system was modelled physically by three very large, completely homogeneous boxes, which could not possibly describe the complex depth resolved processes that significantly affect carbon and nutrient cycling in this region. Further lateral advective processes could not be adequately resolved by this model

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structure. One of the key lessons from coupled hydrodynamic - ecosystem modelling in the last few years is that unless the physics is reasonably accurate the ecology will not mimic observations. Hence it is of concern that the authors report a mismatch in modelled temperature and salinity. Rather than transform observations of pCO₂ etc to fit the models t&s fields, should the observations not be assimilated into the model so that both the modelled carbon chemistry and the ecology reflect the same temperature fields.

The authors correctly state that the Belgian Coastal zone is strongly influenced by riverine inputs, but a key vector for this is the sediment loading and its subsequent effect on light and production, which is not considered. It is difficult to judge the MIRO model as it is under review. The ecological simulation results are impressive, but to what extent is this down to parameter fitting to observations rather than the accurate representation of processes with parameters generated from process studies. The former would to some extent invalidate the conclusions as this study is contrasting the influence of different processes; if the model is based on the latter then there is no objection.

The model simulations of DIC and TA are not as good as claimed, therefore some doubt is thrown onto the good fit of pCO₂. Are the reasons for the fit the correct ones?

Another issue is the presentation of the four year simulation as what I assume is consistently a mean annual cycle. Some of the shapes of the graphs would suggest (because sharp features are retained) that there is very little inter-annual variability in the model. As the authors point out there is considerable inter annual variability in the period. Therefore I'd question the adequacy of the data forcing the model. In fact fig 8b suggests almost identical wind fields in 1996 and 1999. This indicates some detail is missing here. Is the model spun up prior to the 1996 simulation? The frequency of the TA/pCO₂ observations is not given.

Fig 1 a is not well reproduced, I'd favour enlarging and combining with 1b.

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Having said the above, given revision I would welcome the manuscripts publication. This is an important research area, there are many contributory processes, which cannot all be addressed in one study. This work will be very informative as subsequent studies chip away at quantifying the carbon cycle. I would like to see a more thorough description of the modelling techniques used and a discussion of how the chosen models might influence the conclusions.

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