

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.

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The paper by Gypens and co-authors is an important demonstration of the individual significance of the biological, physical, and chemical processes to the air-sea exchange of carbon dioxide that was worked out from a three-year study of a southern coastal section of the North Sea. The paper leads the way to future studies of similar nature in other coastal environments that may be characterized by other ecosystems and combinations of hydrological, chemical, and physical conditions. The authors make an intriguing summary observation that is supported by literature references in their paper: “Existing data of carbon dioxide air-sea fluxes suggest that temperate marginal seas act as sinks for atmospheric carbon dioxide ... On the contrary sub-tropical marginal seas and near-shore ecosystems influenced by terrestrial inputs such as inner and

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outer estuaries, mangroves, and non-estuarine salt marshes act as sources of carbon dioxide to the atmosphere.” Indeed, the land-derived organic carbon as a remineralized source of carbon dioxide is likely to be more important in the near-shore, proximal coastal zone and estuarine outlets transporting organic carbon to the ocean. On a global scale, the surface area of the estuaries, about 1 million square km (Borges, personal communication, 2002), is a small fraction of the continental shelf area to 200-meter depth, about 27 million square km. However, in sub-tropical or tropical environments, the abundance of calcareous planktonic and benthonic organisms may be an additional factor that contributes to the carbon dioxide flow from seawater to the atmosphere due to the calcium carbonate production.

The fact that ecosystem calcification has not been considered in a number of recent papers on the carbon and nutrient cycles of the North Sea suggests indirectly that it is not perceived as a significant component of the carbon cycle, even if the calcium carbonate production by molluscs in the littoral zone is a biological calcification process, albeit of a magnitude not known to us. In fact, if the North Sea ecosystem includes mainly non-calcareous primary producers then this important feature emphasizes that this shallow-water region is a specific type where the carbon cycle does not involve production, dissolution, and storage of calcium carbonate.

We suggest to the authors to consider the points outlined below that would make a revised paper clearer.

1. What units are used for the partial pressure of carbon dioxide: are these ppm (mass/mass), as given in the paper, or ppmv (parts per million by volume)? Do the ppm values in the text and Figure 3 indicate partial pressure in the atmosphere or the internal partial pressure in seawater that was computed from the concentration of undissociated carbon dioxide? Explain the subscript 33.5 in DIC and TA.
2. In the discussion, the effects of temperature on the air-sea flux should be made clearer: it seems that there are both a temperature-dependent solubility effect and

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primary production effect, but they are not clearly distinguishable one from the other in the paper.

3. In Figs. 5 and 8, what is meant by “suppression” and “forcing”? Is only one process “suppressed” at the time? If yes, what is done with the others? Also, if “forcing” means the operation of only one factor, what about the rest?

4. In Table 1, it is not clear what the columns Wind speed, River discharge, and Temperature represent. Are they related to the data plotted in Figs. 5, 6 or 8?

5. It would be helpful if the carbon dioxide concentrations or partial pressures (Fig. 5) calculated with “suppression” of biology and riverine C inputs were accompanied by the flux values that are being suppressed in the model. This may be done by adding a short table of carbon balance for the seawater volume studied, including the reservoir masses, imports, and exports of the relevant carbon species.

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