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Biogeosciences Discussions

Interactive comment on "A model study of the seasonal and long term North Atlantic surface pCO_2 variability" *by* J. F. Tjiputra et al.

Anonymous Referee #1

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Review of Tjiputra et al., A model study of the seasonal and long term North Atlantic pCO2 variability. General comment: This is an interesting study that is well written and organized. Still, I have some critical comments. The assessment of the model performance is too optimistic. Whenever a significant non-T contribution to fCO2 occurs, large discrepancies between the model and the measurements exist (Fig. 4). This indicates serious shortcomings in the biogeochemical component of the model. The comparison of the model results with the CARINA data is confined to the Taylor diagramme, to make it more illustrative show also modelled vs. measured data or omit this section. Some of the interpretations of the trends in fCO2 and in the CO2 fluxes are also questionable (see below). Specific comments Introduction:: Please distinguish clearly between the uptake of anthropogenic CO2 and uptake, e.g. in the North Atlantic, caused by the natural cycling of CO2 between the ocean and the atmosphere. Obser-

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vations: Fig. 3, right panel: Add the mean seasonality obtained from measurements; 4.1 Regional seasonality of fCO2 p. 10195/line 7: “deviation” instead of “anomalies”; p. 10195/10196, NASPG: To explain the phase shift in the pCO2 draw down in NASPG you should also discuss the temporal development of the mixed layer depth that affects the light conditions for plankton and thus has a large influence on the start of the spring bloom. The differences in the seasonal DIC amplitude might be due to too low winter nutrient concentrations in the model. Nutrient regeneration produces also CO2 and has almost no net effect on DIC. Explain briefly “sophisticated multi-functional groups of phytoplankton”. 4.2 Regional trends in fCO2 and sea-air CO2 flux p. 10198/line 10: Only the signs of the trends agree. p. 10198/line 20 - 23: I can‘t see any agreement between the model and measurement derived interannual variability. Either abstain from this statement or document it in a more convincing way. p.10199/line 5: If the data of one particular year determine the slope of a regression line, it is certainly not reasonable to interpret this as a trend. In view of the interannual variability the detection of trends require longer time series. Trends in air-sea fluxes: For the interpretation of the trends in the air-sea fluxes it is necessary to take into account also trends in the gas exchange transfer velocity (wind) and in the CO2 solubility (SST). I have a problem with explaining the flux trends by diverging trends in fCO2 and atmospheric CO2. If , for example, the fCO2 trend exceeds that in the atmosphere and if the fCO2 is below the atmospheric level, then partial pressure difference is decreasing and the fluxes are decreasing. Vice versa, if the fCO2 is above the atmospheric level, then partial pressure difference is increasing and the fluxes are increasing accordingly. E.g., Northeast Atlantic: What does the positive slope of the trend line mean? Increasing uptake or decreasing release of CO2? Even if I have misunderstood something, this needs a discussion. Why don‘t you use annual flux balances to identify trends? p. 10201/lines 19 -26: I can also not agree with the explanations of the trends in the surface water fCO2: If due to the hydrographic conditions (heat balance) a continuous trend in fCO2 exists that deviates from the trend in the atmospheric CO2 in some regions, the partial pressure difference will change continuously resulting in fluxes that counteract the di-

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verging of the trends and will at the end produce trends that are the same for both the atmosphere and the surface water.

p.10205/line 15: NPP as such does not change the alkalinity. Or do you mean the consumption of nitrate that increases slightly the alkalinity? What's about calcifying organisms?

Interactive comment on Biogeosciences Discuss., 8, 10187, 2011.