Geosci. Model Dev. Discuss., 4, C92–C96, 2011
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Geoscientific Model Development Discussions

Interactive comment on "The role of phytoplankton dynamics in the seasonal variability of carbon in the subpolar North Atlantic – a modeling study" *by* S. R. Signorini et al.

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We strongly disagree with the reviewer that our study is not a significant contribution. One of our most valuable model results is that it shows that the observed surface ocean pCO2 trend cannot be explained by atmospheric anthropogenic forcing alone. Nor can the surface SST changes. It is only when a deep DIC trend supported by observations is applied to the model, combined with vigorous winter mixing, that the measured surface pCO2 growth can be accurately simulated. Therefore, the model has been used effectively in the identification of the "missing" forcing mechanism, e. g., the deep DIC decadal trend in intermediate waters of remote origin. In addition, the

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model experiments allowed us to investigate the effect of coccolithopores on carbon uptake, showing that they play an important role in the carbon balance of the region.

It is clear that the reviewer has a problem with constraining the deep layers of the model with DIC(T) and TA(S). But these relationships were based on the in situ measurements (CARINA and Suratlante). Findlay et al. (Biogeosciences, 5, 1395-1410, 2008) used an ecosystem model for the carbon and nutrient mixed layer dynamics in the Norwegian Sea based on similar deep ocean constraints. The carbonate system in their model is forced by deep alkalinity and DIC derived from OWS M observations. The reviewer also rejects the idea of relaxation of nutrients to empirical equations of temperature in the deep layers based on WOA05. However, with 1D dynamics, that is a requirement to maintain proper deep water forcing in the model. If this strategy were flawed then the surface nutrients would not match the measurements. An earlier version of the model, which included deep water nutrient and carbonate system constraints to in situ observations, was successfully applied previously in the eastern subarctic Pacific (Signorini et al., JGR, 106, C12, 31,197-31,215, December 2001). Regarding the tracking of POC, it is true that the model does not include an explicit POC pool, and that would be valuable to investigate other scientific questions such as the evaluation of changes in the strength of the biological pump (POC/PIC), for instance. This is planned as a future upgrade of the model and we thank the reviewer for pointing that out. However the export of particulate organic carbon from the euphotic zone is taken into account in the model via the sinking terms in the phytoplankton, zooplankton, and detritus equations. The upper ocean carbon balance in the model includes all the pools and rates (DIC, DOC, PIC, gross, net, and export production, and detritus) that are relevant to changes in the uptake of atmospheric CO2 in the region.

The reviewer also asks "How much does this approach capture observed variations?". Our evaluation of the model is illustrated in Figs. 6 through 9, which we believe provide evidence that our approach captures the observed variations.

Minor comments

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1. Sweeney et al. 2007 coefficients should be used in Wanninkhof 1992 equation to calculate gas exchange.

We are aware of the Sweeney et al. 2007 coefficients (Signorini and McClain, GBC, 23, GB2025, doi:10.1029/2008GB003246, 2009). We use NCEP Reanalysis II 6-hourly winds with k=0.31. The manuscript needs to be corrected as it states the use of 3-hourly winds (page 28). Sweeney et al (2007) use the value of 0.27 in the Wanninkhof 1992 gas transfer and NCEP/NCAR 6 hourly winds. Takahashi et al 2009 (Sweeney is a co-author there) take note of the Sweeney et al. (2007) result and in addition correct for the Suess effect and use a value 0.26 with 6 hourly reanalysis wind speed data. As far as we know, there is no universally agreed formulation to be used for the gas transfer coefficient with lots of research underway. Nevertheless, we conducted a sensitivity test with the model using k=0.26 and compared the results (DIC, pCO2, and CO2 flux seasonal cycles) with the original experiment using k=0.31. The overall differences are generally small. For DIC, pCO2, and flux the values are 0.04%, 0.49%, and 6.33%. Figure 1 shows the differences between the two runs.

2. Some information about the GCM physical model should be presented. "Personal communication" is not an appropriate reference for this model, particularly when the individual is the second author on this paper.

We will add a description of the physical model when we revise the manuscript after all the reviewers' comments are received.

3. Figure 6 a. The color scheme is confusing. Please be consistent with what is model and what is data throughout

This is a very good suggestion. All figures will be revised for clarity.

4. Figure 7 a. The figure is too small to see b. The caption needs more detail

These seem to be two comments for the same figure since Fig. 7 doesn't have a and b panels. Changes to the figure and caption will be made.

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5. Figure 10 and 11 – These are impossible to see. Show a mean seasonal cycle and then an anomaly figure for the variability.

These figures will also be revised for clarity as the reviewer suggested.

Interactive comment on Geosci. Model Dev. Discuss., 4, 289, 2011.

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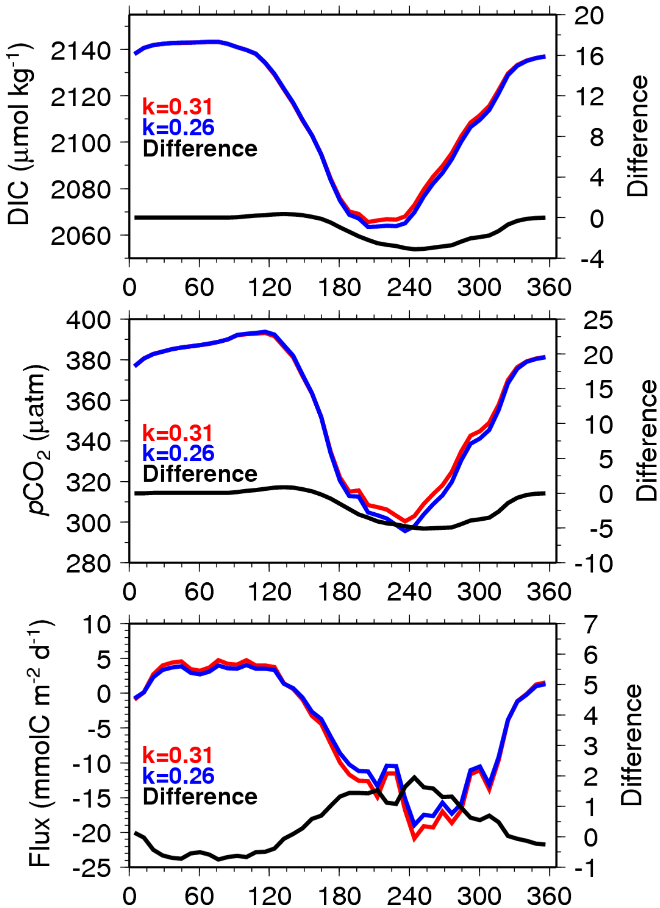

Fig. 1. DIC, pCO2, and flux with two different values of k.

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