Biogeosciences Discuss., 4, S1690–S1695, 2007
www.biogeosciences-discuss.net/4/S1690/2007/

Biogeosciences
Discussions

Interactive comment on "Carbon and nutrient mixed layer dynamics in the Norwegian Sea" *by* H. S. Findlay et al.

Anonymous Referee #1

Received and published: 24 October 2007

1. General Comments

The paper by Findlay et al. presents a study which combines modelling work with an ecosystem model of intermediate complexity and data from Ocean Weather Station Mike (OWS-M) located in the Norwegian Sea. Model formulations and parameters are taken from earlier work of the authors (Merico et al., 2004; 2006) and cover major nutrients (nitrogen, silicate), the inorganic carbon system (DIC, Alk), 4 phytoplankton functional types (diatoms, flagellates, dinoflagellates, and E. huxleyi (coccolithophores)), 2 zooplankton size classes, detritus as well as attached and free coccoliths.

The paper aims at modelling nutrient and carbon fluxes at OWS-M and evaluating the relative role of physics vs. biology (the role of coccolithophore blooms in particular) for

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

carbon cycling at that station.

In addition one may extract two important topics of this paper:

1.) Is there a (temporal) coincidence (or even cause-impact relationship) between calcite saturation state ($CO_3{}^{--}$ concentration) and coccolithophore bloom?

2.) Which complexity is required to model the observed seasonal cycle (and partly interannual variability) of pCO_2, DIC, alkalinity, and $CO_3{}^{--}$ at OWS-M?

1.1. Modelling the seasonal cycle:

The comparison between observations and model(s) carried out in this study is largely qualitative, i.e. by comparing the temporal patterns of modelled and the observed variables (Figs. 5-11). In some cases (f.e. the inability of the standard model to simulate the observed slow decrease of Si over the summer season) this may be sufficient. In many other cases it is not. For example: Are the observations of chlorophyll a consistent with the modelled double-peak spring bloom? Do the observed CO_2 data (Fig. 6 and 7) support the full model or would a reduced model (without coccolithophores) be sufficient? These and other questions require a quantitative comparison of model(s) and observations using clearly defined metrics. Various approaches have been suggested in the literature for data-model comparison, one of which is the Taylor-diagram (Taylor, 2001, JGR). For the purpose of this paper I suggest to try out a plot of normalised pattern statistics (see f.e. Fig. 5 in Taylor, 2001). This allows to (a) plot different variables into one diagram and (b) show differences between model versions (here: full vs. reduced model) as vectors. From a Taylor diagram of normalised pattern statistics, you immediately see f.e. whether the full model is an improvement in representing lets say the seasonal DIC evolution compared with the reduced model version while at the same time, lets say, improve the simulation of alkalinity, chlorophyll, and/or nitrate.

It is my strong impression that the paper would clearly gain from applying this or other relevant quantitative metrics for the data-model and model-model comparison.

The authors find that neither of their model versions is able to simulate the slow silicate drawdown over the summer months. The authors discuss various possibilities, and apply a sensitivity analysis to explore possible solutions to overcome this misfit. In addition to study the sensitivity to MLD, growth rates of diatoms and flagellates, and effects of grazing (including the silicate switch used in this model) she authors might consider a parameter optimization study for fully testing whether their model formulation is able to simulate the slow silicate drawdown with other realistic parameter sets before calling for other (non-modelled) solutions like iron limitation. I plea for such an approach also because a parameter optimization technique has not yet been applied to the Merico model and this paper simply adopts the parameters used in the Merico paper. An additional benefit from a parameter optimisation experiment is that you learn which parameters (and hence model formulations and compartments) are less constraint than others by the data set at use.

1.2 Coincidence between calcite saturation state and coccolithophore bloom.

The paper of Findlay et al. follows recent work of Merico et al and computes the seasonal development of CO$_3^{--}$ at OWS-M as simulated by their model and from observed total CO$_2$ and total alkalinity data. Both the data and the model support a seasonal cycle with an amplitude of about 50 umol kg^{-1} and the model clearly indicates that the summer maximum in CO$_3^{--}$ is due to biology and not physics (warming and outgasing of CO$_2$). The magnitude of the seasonal CO$_3^{--}$ cycle at OWS-M is in accordance with other data from the temperate and subarctic North Atlantic and the literature discussed by the authors.

However, what is the relevance of elevated CO$_3^{--}$ concentrations for the blooming of coccolithophores? Both this paper and the Merico et al. 2006 work intend to investigate whether the natural occurrence of E. huxleyi could in *any way be linked* with natural variations in CO$_3^{--}$. The questions is: Is it just mere (temporal) coincidence or is any kind of cause-effect relationship involved? As coccolithophore blooms usually follow a diatoms bloom, that diatom bloom carbon uptake will always decrease pCO_2 and increase CO_3^{--}. The reasons for diatoms to usually be the first-bloomers in the North Atlantic open oceans are multitudinous. In experimental settings, however, one can easily have coccolithophores as the first bloomer, f.e. by excluding silicate while providing high N:P nutrient ratios. For example, experimental work with mesocosms (Delille et al., 2005; GBC) has shown that coccolithophores can bloom under quite different initial conditions of CO_3^{--} : reducing the CO_3^{--} at the beginning of the experiments from about 210 umol kg^{-1} to about 90 umol kg^{-1} (reflecting initial pCO_2 of 200 to 700 ppm; CO_3^{--} values computed from DIC and alkalinity data presented in Delille et al. Fig. 1) allowed for coccolithophore blooms under all initial CO_3^{--} regimes. Notwithstanding there were difference in timing and POC:PIC production ration during the experimental blooms, but nevertheless coccolithophores did bloom in all experimental settings.

Hence, I dont see how the authors can really shed more light on this question (coincidence vs. cause-effect relationship) by modelling the OWS-M data. They just provide another example that there is at least a temporal coincidence of elevated CO_3^{--} concentrations and a moderate coccolithophore bloom *in their model runs*. One must say, however, that it is already difficult to judge how model and data agree at all on the significance of coccolithophore blooms at OWS-M. Alkalinity data, f.e., indicate some seasonality most likely related to calcification, in particular in the second model year (2003). However, interannual variations of alkalinity appear to be significant in the data but not in the model: in 2002, f.e. data and model alkalinities are well offset while in 2005 I hardly can see any clear seasonal change in the alkalinity data, but the model behaves as in all years suggesting a summer minimum in alkalinity. Overall the fit between data and model appears not to be very good (qualitative metrics will clarify this). The authors quantify a decrease in alkalinity of 38 uEq kg^{-1} and compare this with alkalinity changes of 50-60 uEq kg^{-1} in other parts of the North Atlantic characterized by frequent coccolithophore blooms. Yet this is obviously an upper bound of seasonal alkalinity excursions (and hence calcification) seen in the data. The additional argument provided by comparing pCO_2 data and model values (Fig. 6) is very much weakened by an unspecified shift down of model pCO_2 data to make them comparable to observations from the early 1990s and missing quantitative metrics. The authors might be able to support the modelled relative importance of coccoslithophores at OWS-M by referring in more detail (i.e. quantitatively, cell numbers, biomass etc.) to published work from the Norwegian Sea which they cite in their paper as general evidence for coccolithophores in the Norwegian Sea.

1.3 Which complexity is required to model the seasaonal cycle at OWS-M?.

This is a question which is at the heart of recent discussions regarding ecosystem model development (f.e. recent discussion in J. Plankton Res. Anderson, 2006 ff). The authors address it to the extend that they compare how their full model and a reduced version (excluding coccolithophores) compare to the observations (Fig. 6, 7). They could do a much better job if applying quantitative metrics and a modell parameter study.

2. Specific Comments

Part of this presentation deals with a comparison of the full model and one driven only by physics and excluding biological processes. In a way this repeats work by Garcon et al. 1992 (DSR) modelling Station Papa data from the North Pacific. At that time such a comparison was important in order to visualise how biogeochemistry (phytoplankton blooms in particular) impacts on carbon fluxes in high latitudes, f.e. by reversing air sea fluxes during summer compared with an artificial purely physically influenced ocean. However, now after another 15 years of biogeochemical studies and modelling of the role and quantitative importance of biology for surface ocean carbon dynamics this has become textbook knowledge.

As this aspect of the paper is hence not novel, I suggest to skip paragraphs and sentences dealing with non-biological model runs and simplify figure 6 and 7 accordingly.

With one exception: for fig. 7C, you may leave the non-biological model run as a reference line indicating that the seasonal temperature development also increases CO_3^{--} concentrations, but not comparable in timing and magnitude to the observations.

3. Technical Corrections

p 3233, l 9: place the reference to Fig. 4 into the text and remove it from the headline of 2.1.1

Figures:

Fig. 2 is not necessary in this paper. An interested reader will have to take a look at your earlier work (Merico et al. 2004, 2006) anyway, in order to understand details of your modelling approach.

Fig. 6. In the legend, or in the main text, you should tell us by how much the modelled pCO_2 data have been shifted down for this comparison.

Some of the figures are un-necessarely tiny:

Fig. 5 should be printed similar to Fig. 4, i.e. three panels (nitrate, silicate, chlorophyll) instead of 9, each panel having four years (one without data).

Fig. 7, as this is a very important one, should be larger as well; you may need to present Fig. 7a+b and Fig. 7c+d separately, in order to overcome BGDs automatic figure size processing.