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Interactive comment on "The fate of pelagic CaCO$_3$ production in a high CO$_2$ ocean: A model study" *by* M. Gehlen et al.

M. Gehlen et al.

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We wish to thank the reviewer for his constructive comments. The referee raises several interesting questions which we address as follows.

(1) Terminology: The referee suggests the use of 'primary' and 'export' CaCO$_3$ production instead of 'potential' and 'net' CaCO$_3$ production. His suggestion is based on an analogy to the autotrophic organic C production and export. The term 'primary' refers to autotrophic production. Calcium carbonate is synthesized by primary and secondary producers. Accordingly, we would rather use 'gross' instead of 'primary' production for CaCO$_3$. The 'export' production bears a close analogy to our 'net' production. It is however defined with respect to a particular depth horizon (e.g. the mixed layer, euphotic layer, 100 etc), while CaCO$_3$ net production might extent below the mixed and euphotic layer if heterotrophic calcifiers are included. Because the present study is only

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a first step in refining calcification in PISCES, we wish to select a terminology suitable for the following studies as well. Since the developments around calcite production by nanophytoplankton were implemented, we extended our approach to the calcification by zooplankton. That is why we wish to use 'net' production.

(2) Ocean carbon and alkalinity cycles: Implementing a dependency for $CaCO_3$ formation on $[CO_3^{2-}]$ did not result in large changes in the modeled $CaCO_3$ production compared to the previous version (or standard version) of PISCES (Aumont and Bopp, 2006). In the model standard version the ratio of inorganic to organic C (PIC/POC) is a function of nutrient availability, temperature and nanophytoplankton biomass. It is bound between 0.01 and 0.8. The same upper and lower limits are used in the present parameterization. From Fig.1 it can be seen that for saturation states with respect to calcite prevailing in large areas of the pre-industrial surface ocean $(\Omega c > 4),$

PIC/POC computed from equation 3 ranges between 0.73 and 0.77.

Updating the parameterization of $CaCO_3$ dissolution in the model had larger consequences. The PISCES standard version (Aumont and Bopp, 2006) uses the dependency of $CaCO_3$ dissolution on undersaturation proposed by Maier-Reimer (1993). We implemented a first order kinetic reaction with respect to undersaturation and derived the dissolution rate constant from a data base of $CaCO_3$ fluxes recorded by sediment traps. It would need several thousand years of model integration in order to bring the alkalinity cycle close to equilibrium. We run the model for 400 years, long enough for a reorganization of dissolved inorganic carbon (DIC) and alkalinity (ALK) fields down to 1000m in response to the modified descriptions of calcification and dissolution. This appears as a valid starting point for our study focusing on changes in $CaCO_3$ production and dissolution over a period of 140 years (= the duration of a model experiment) and the associated feedbacks on air-sea exchange of CO_2. In order to document ocean cycles of C and alkalinity, we will include sections across the Atlantic, Pacific and Indian oceans comparing model output for DIC and ALK to observations. We will quote global mean concentrations of DIC and ALK in the revised version of our manuscript. We will

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further compare modeled depths of the calcite saturation horizon to GLODAP.

As quoted by the referee "The form of the observed global mean ALK profile (Figure 2) appears successfully reproduced by the model in general, but the model over-predicts ALK above ca. 2000 m water depth and under-predicts at greater depth." One obvious reason would be the lack of an appropriately long spin-up after implementing the new parameterizations for $CaCO_3$ production and dissolution. This can be ruled out. A follow-up version of the model (Gangstø et al., submitted) was submitted to 5600 years of spin-up. Modeled ALK profiles still show the same tendency for overestimation of ALK in the upper ocean and underestimation at depth. From the point of view of the biogeochemical processes included in the model, the departure between modeled and observed global mean ALK profiles in the upper ocean might reflect an underestimation of net $CaCO_3$ production. This could follow from an overestimation of $CaCO_3$ dissolution associated with zooplankton grazing on nanophytoplankton set to 50

The model spin-up was done at 278 ppm. The atmospheric partial pressure of CO2 is imposed during the spin-up and the experiment. It is not predicted. The experiment covered the range from 286 ppm to 1144 ppm atmospheric CO2. The difference between the atmospheric pCO2 of the spin-up and the starting point of the forcing used for the experiment, resulted in a slight model drift. Model results were corrected for this drift.

(3) Diversity of pelagic calcifiers: We will extend our discussion on the diversity of pelagic calcifiers by including estimates of the share of coccolithophores other than E. huxleyi, along with foraminifera and pteropods. We respond in part to the suggestion of evaluating the impact of assigning calcification to other groups than nanophytoplankton in a study by Gangstø et al. (submitted). Gangstø and coworkers address the contribution of aragonite to the global marine $CaCO_3$ cycle. Aragonite production is assigned to the size class of pteropods which is mesozooplankton.

(4) Reply to specific comments:

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+ The duration of experiments is 140 years; we will include time-series in an additional figure.

+ Page 534 / lines 15-17: No reference attached to the statements about CO_2 uptake by different reservoirs and there probably should be. - Feely et al. (2004)

+ Page 534 / line 21: Adding CO_2 to sea-water does not result in a 'decrease in alkalinity'. ALK is unaffected by adding or subtracting CO2 on its own. - Yes of course, this needs to be corrected.

+ Page 534 / line 22: The original reference for ocean 'acidification' is Caldeira and Wickett [2003] (Anthropogenic carbon and ocean pH, Nature 425, 365, 2003). - We will include the appropriate reference.

+ Page 535 / lines 9-14: You could do with some references here to back up the statements. - References will be included for the dependency on calcification on the saturation state of seawater, as well as on the threat of acidification to marine calcifiers.

+ Page 535 / line 25: It is not sufficient to suggest that the ratio of moles CO_2 released to moles $CaCO_3$ precipitated is 'bound to increase'. - We acknowledge the point raised by the referee, but we feel that a full discussion of it would be beyond the scope of this paper and reproduce other peoples work. We will quote Zeebe and Wolf-Gladrow (2001). The Greek letter will be deleted.

+ Page 536 / line 15: I don't like the word 'impossible'. Surely the purpose of you constructing a model is precisely that it can then give you 'a priori knowledge' about the feedback? - Yes, the model will allow evaluating the feed-back. We will replace 'impossible' by 'difficult'.

+ Page 538 / lines 2-3: This first sentence makes only incomplete sense on its own. Perhaps either expand on the paragraph or incorporate it into the overall description PISCES in the previous section (2.1). - The two sentences are part of section 2.1. We will change the wording.

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+ Page 539 / line 10: 'Experimental PIC to POC ratios are plotted as a function of undersaturation' - no - you have plotted them as a function of 'saturation state' or 'over-saturation', not undersaturation which would be $(\Omega - 1) < 0$.

- Right. It will be corrected in the revised version.

+ Page 541 / lines 12-13: You should state more clearly that you are using an 'off-line' tracer-transport model (i.e, no climate feedback) if this indeed the case. You might then like to speculate how your results might be different if 'climate' (and surface temperatures and ocean stratification) was also allowed to evolve in response to increasing atmospheric CO_2. - We will make it clear that we use an 'off-line' tracer-transport model. It would indeed be tempting to speculate on the impact of climate change based on the predicted changes in ocean productivity and in particular the enhancement of nanophytoplankton in response to increased stratification (Bopp et al., 2001; Bopp et al., 2005). On the other hand we prefer to quantify the impact of climate change by running appropriate experiments, rather than being too speculative at this point.

+ Page 545 / line 1: I know what you mean by 'f.i.', but I have never before seen this abbreviation of 'for instance' used anywhere. Best to just spell it out in words. - This will be done.

+ Page 545 / line 17: It will mean just about nothing to anyone to be told that the 'oceanic DIC inventory' increase by '2.1- The pre-industrial DIC inventory is 35578 GtC.

+ Page 546 / lines 9-10: I am confused as to what the 'total perturbation' is that is 750 GtC in size. Surely you don't reach 1144 ppm CO_2 in the atmosphere from a release of just 750 GtC. Or is this the cumulative anthropogenic CO_2 that has invaded the ocean? What then is the total release? - Assuming that under constant climate the land biosphere and the ocean make up for CO_2 sinks of the same order of magnitude (Friedlingstein, 2001), the atmospheric pCO_2 increase scenario corresponds to 3320 GtC emitted over the duration of the experiment.

+ Section 4.3: I don't understand why you didn't use the CO_2 emissions trajectory that Christoph Henize did in his 2004 paper - that would have allowed you to make valuable direct comparisons with his model experiments. Could this not be done? - Instead of using CO_2 emissions trajectories, we preferred to follow the approach taken by the Climate Model Intercomparison Project (CMIP, http://www-pcmdi.llnl.gov/projects/cmip/index.php) that is to force the simulation with an atmospheric pCO_2 increase of 1

+ Section 4.3 (end): In your discussion of uncertainty of organism (and ecosystem) calcification response to increasing atmospheric CO_2 (and decreasing surface ocean saturation state) you should include some discussion regarding the (unknown) potential for adaption. - The topic of adaptation potential is hidden in the two citations Langer et al. (2006) and Hendericks and Rickaby (2006). We will discuss it more explicitely.

+ Table 2: You could also include some recent inverse-modelling estimates of global $CaCO_3$ production (equivalent to your net estimate I guess), such as the 'preferred' estimate diagnosed from surface ocean nutrient and alkalinity distributions by Jin et al. (Global Biogeochem. Cycles, 20, doi:10.1029/2005GB0025322006) of 1.14 PgC yr-1 as well as the estimate of 1.21 PgC yr-1 obtained by Ridgwell et al. (2007) from a low resolution GCM optimization of ocean PO_4 and ALK distributions. - We will follow these suggestions.

+ Table 2: The estimate of 0.3 PgC yr-1 you cite from Feely et al. [2004] (Figure 5) is the estimated global $CaCO_3$ burial flux, is not the same thing as your 'CaCO3 flux at lower boundary and thus cannot be compared. In fact, it cannot be compared both because it is burial rather than surface sediment rain flux (as you note), and because the Feely et al. [2004] estimate includes shallow marine sediments (including coral reefs). A more appropriate comparison would be with the Feely et al. [2004] estimate of 0.4 PgC yr-1 reaching the deep ocean, which is not too far from your model estimate anyway. - We did not mean to compare Feely et al. (2004) burial flux and the model flux at the lower boundary. We write "The $CaCO_3$ flux at the lower model boundary reflects

the boundary conditions and its inherent criteria of mass conservation" (page 542, L26 and page 543, L1-2). To avoid any misunderstanding we might repeat this statement in the table text, which at this point simply states "model lower boundary condition".

+ Figure 1: Please include an R2 value for this fit. - The figure will be updated during the revision of the manuscript.

+ Figure 2 and Figure 4 will be edited to improve its clarity. On Figure 2, bottom panel the calcite saturation horizon is presented and not the lysocline as was indicated by mistake.

Interactive comment on Biogeosciences Discuss., 4, 533, 2007.

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