**Biogeosciences
Discussions**

Interactive
Comment

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

Anonymous Referee #3

Received and published: 2 April 2007

Marion Gehlen and colleagues present the results of an extension to a cutting-edge model of ocean circulation and biogeochemical cycling in which they take into account a dependence of CaCO3 production by marine calcifiers on the ambient carbonate chemistry of the surface ocean. To my knowledge this is the first time that a representation of marine (pelagic) carbonate production that is responsive to surface ocean saturation state (and pH) has been incorporated into a complex ecosystem model. Related to this comes the ability to resolve 'potential' and 'net' CaCO3 production. I like this aspect and more could be made of it. However, the terminology is clunky and should be improved. Assuming that 'potential' and 'net' CaCO3 production parallels the well established concepts of primary and new (or export) productivity for organic carbon, why not use similar terminology? i.e., the total CaCO3 production prior to graz-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

ing and recycling (dissolution) within the surface ocean becomes 'primary carbonate production' (rather than 'potential') and the flux of CaCO3 that escapes recycling and is exported out of the surface (/mixed) layer of the ocean becomes 'export carbonate production' (rather than 'net').

In developing the representation of surface ocean CaCO3 production and water-column dissolution, the marine cycle of dissolved inorganic carbon (DIC) and alkalinity (ALK) will inevitably have changed. However, this paper currently lacks sufficient information as to what the ocean cycles of carbon and alkalinity look like, particularly when you say that the model can 'reproduce the large scale distribution of biogeochemical tracers in the world ocean' - I do not doubt you on this point, just request that sufficient model-data comparison is included. For instance, does the development of the representation of production and fate of CaCO3 in the model lead to an improved model prediction of pre-Industrial DIC and ALK compared to the data or does it degrade the quality of the simulation possible with the previous version of the model? In an ideal World, model developments would always drive improvements in simulation quality. However, I note from the Abstract (line 10) that initial (presumably pre-Industrial) atmospheric CO2 concentration is 286 ppm, whereas it should ideally be 278 ppm. This might reflect an ocean CaCO3 pump that is too strong and/or shallow(?) The following additions would be helpful in better judging the model:

* Information regarding the pre-Industrial composition of the ocean (e.g., DIC and ALK as global mean concentrations) associated with the 286 ppm predicted equilibrium atmospheric CO2 concentration.

* 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. Why?

* An extension to Figure 2 showing the 'observed' saturation horizon and how it compares to the model predictions. (You could calculate the 'observed' saturation horizon

depth from 3-D gridded GLODAP natural DIC and ALK, T and S, and for completeness, perhaps also using World Ocean Atlas information regarding [PO4] and [H4SiO4] distributions.)

You might also think of using the GLODAL ALK data-set rather than Goyet et al. [2000]. For instance, Key et al. [2004] find GLODAP ALK are systematically higher than Goyet et al. [2000] in the upper water column, which could explain some of the model-data misfit in Figure 2 (upper panel). Re-running the model initialized with GLODAP might thus further improve the quality of the ALK simulation.

There is a specific caveat about the model assumptions that you should discuss. As you will be aware, perhaps only half of all pelagic CaCO3 production in the open ocean is due to coccolithophores such as E. huxleyi, with some estimates giving production by foraminifera globally dominant (e.g., Schiebel [2002], Global Biogeochemical Cycles 16, DOI: 10.1029/2001GB001459). However, as a logical first step in the never-finished process of model development you have parameterized all carbonate production as being associated with nonophytoplankton. I have no problem with this. However, you should discuss the implications of this assumption. For instance, it would be instructive to contrast the marine carbon cycle and oceanic anthropogenic CO2 uptake response obtained with the assumption that all carbonate production is associated with the nonophytoplankton functional type, compared to assuming production by a different phytoplankton or zooplankton functional type (but perhaps retain the empirical E. huxleyi response in order to simplify the comparative analysis). This might reveal something of the uncertainty associated with structural (rather than parameter values) assumptions made in the model.

If I was not so lazy, I could of course calculate the year in which atmospheric CO2 reaches 1144 ppm. However, I am lazy. And I doubt that I am alone in this. Please explicitly state the years over which the model is run and/or the duration of the model experiment. It would be extremely helpful to readers to get a better idea of just when in the future (next year, year 2020, 2300 Ě ?) the results of increased CO2 uptake relate

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

to. What would also be helpful is to have a time-series as an additional figure, perhaps incorporating the time-evolution of: atmospheric CO_2, mean surface saturation state (and/or pH), biological (net) carbonate export (and perhaps dissolution, which presumably lags somewhat the surface production response as CO_2 invades the ocean thermocline?) and cumulative anthropogenic CO_2 uptake compared to the baseline (i.e., as a result of predicted changes in marine carbonate cycling).

Minor points:

o Page 534 / lines 15-17: No reference attached to the statements about CO_2 uptake by different reservoirs and there probably should be.

o 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 CO_2 on its own.

o Page 534 / line 22: The original reference for ocean 'acidification' is Caldeira and Wickett [2003] (Anthropogenic carbon and ocean pH, Nature 425, 365, 2003).

o Page 535 / lines 9-14: You could do with some references here to back up the statements.

o Page 535 / lines 11-12: It is not completely clear when you say 'calcification decreases with decreasing saturation state with a threshold value well above =1 (saturation)' what exactly this threshold refers to - a threshold for an effect on calcification to start or when calcification ceases entirely, for instance? Please clarify.

o Page 535 / lines 24: The Greek letter (which I cannot even remember the name of!) is not referred to anywhere else in the paper and so should be omitted because quoting it serves no useful purpose.

o 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'. If this is not a direct corollary of your subsequent statement that the 'overall buffer capacity of seawater decreases', then you must either derive (/prove) the result here yourself or reference a paper where

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

this result is demonstrated. I would say that it would be sufficient to simply re-phrase the sentence, although better might be to discuss this in context of the Revelle factor (although strictly this is applicable only at constant alkalinity) (see: Zeebe, R. E., and D. Wolf-Gladrow, CO2 in seawater: Equilibrium, kinetics, isotopes, Elsevier Oceanographic Series 65, Elsevier, New York, 2001).

o 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 Ě ?

o 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).

o 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 $(W-1) < 0$.

o 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 CO2.

o 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.

o Page 545 / line 17: It will mean just about nothing to anyone to be told that the 'oceanic DIC inventory' increase by '2.1%', particularly as you have no-where stated what pre-Industrial (baseline) ocean DIC inventory is!

o 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 CO2 in the atmosphere from a release of

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

just 750 GtC. Or is this the cumulative anthropogenic CO2 that has invaded the ocean? What then is the total release?

o Section 4.3: I don't understand why you didn't use the CO2 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 Ě ?

o Section 4.3 (end): In your discussion of uncertainty of organism (and ecosystem) calcification response to increasing atmospheric CO2 (and decreasing surface ocean saturation state) you should include some discussion regarding the (unknown) potential for adaption.

o Table 2: You could also include some recent inverse-modelling estimates of global CaCO3 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 PO4 and ALK distributions.

o Table 2: The estimate of 0.3 PgC yr-1 you cite from Feely et al. [2004] (Figure 5) is the estimated global CaCO3 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 :)

o Figure 1: Please include an R2 value for this fit.

o Figure 2 / top panel: The x-axis scale is rather compressed - please expand between more appropriate limits so we can better see what is going on - e.g., 2250-2450 umol

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

kg-1, or 2300-2450 umol kg-1. Also, there is no reason why the same y-axis scale should not be used as is in Figure 3, which would allow easier by-eye comparisons to be made between alkalinity profiles and CaCO3 dissolution rates and settling fluxes.

o Figure 2 / bottom panel: Much more useful information (about water mass distributions and biological pump imprinting) could probably be gotten across if you increased the number of colour contours. Also, surely this is not the calcite 'lysocline' that you have plotted - you presumably mean the calcite 'saturation horizon' (i.e., the depth at which W = 1.0)???

o Figure 4: Again, more information could be provided with a better chosen colour-scale. For instance, W is every-where < 8 at the resolution of your model, so it seems unnecessary scaling all the way to 10. You may as well black-contour and label the W = 4.0 contour in the lower panel as per the upper one.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU