Biogeosciences Discuss., 5, S2585–S2589, 2009
www.biogeosciences-discuss.net/5/S2585/2009/

Biogeosciences Discussions

Interactive comment on "The importance of ocean transport in the fate of anthropogenic CO_2" *by* L. Cao et al.

Anonymous Referee #2

Received and published: 10 January 2009

The manuscript investigates the results of a simulated pulse emission of 590 Pg C for a number of ocean circulation models. A number of papers have already investigated the differences in simulated oceanic CO2 uptake among different models. Some of these papers, cited in the manuscript, explicitly attributed those differences to differences in the model physics. Cao et al. aim at a better understanding of the causes of these differences.

A main finding is that the CO2 uptake modeled by the different models is, on decadal times scales, positively correlated with the simulated uptake of CFC11 and historical anthropogenic CO2, whereas on time scales of centuries to millennia, correlations with simulated natural carbon content become larger. The authors also show that differences in steady-state ocean transport among the different models lead to differences

in simulated uptake of anthropogenic CO2 that are of similar magnitude as impacts of climate feedbacks on CO2 uptake.

Overall, the paper is relatively well written and the main message is very clear and convincing. I have, however, two main concerns and a number of minor points (see below) that need to be addressed in a revision before the paper, in my view, should be accepted for publication in Biogeosciences.

Major concerns:

1. Correlation analysis: The modeled transient tracer uptakes/inventories shown in Figures 2 and 3 for the various models reveal, however, that the correlations are only so high (=good?) because of a few very unrealistic model results. The color scales of the different models are difficult to see on my printout (I suggest to include a second table that states the values shown in the figures for all models), but I think that the GENIE8 and HILDA models are well outside the observational errors for the CFC11 inventory. Disregarding these two models would give much lower correlations between CO2 uptake and CFC11 which, on long time scales may even become negative. While the correlations with historic anthropogenic CO2 uptake may not be affected that much, the del-14C correlations will also become much weaker if you disregard those models that are well outside the observational range of del-14C inventories. Putting it differently, if you include bad enough models that produce enormous outliers, your correlations will be good, although the regression coefficient depends more and more on the outlier. There is no guarantee that the outlier follows the "correct" mechanisms of ocean transport, although in the current study, the sign of the regression coefficients seems plausible. One possibility to deal with this is to introduce a weighting scheme that attributes higher weights to more reliable models. Alternatively, you could compute the regressions for only those models that produce tracer inventories within the observational error bounds.

2. Surface ocean response functions: I'm afraid I haven't fully understood why you

BGD

5, S2585–S2589, 2009

Interactive Comment

need to employ surface ocean response functions. I believe the idea is to isolate impacts of differences in ocean transport from differences in buffering capacity or air-sea gas exchange. Shouldn't the response function that depends only on ocean transport then be linear in the CO2 perturbation? The results show, however, that the uptake of the 5000Gt pulse is not linearly related to the uptake of the 1000Gt pulse. If I got it right, the nonlinearity is introduced by the cubic fit that is used to estimate sea water pCO2 from DIC (p4531, l.7). However, this cubic fit then accounts for (part of) the differences in the buffering capacity. OK, it is only that part influenced by uptake of "pulse CO2", whereas the full models without climate feedback may also differ in the buffering capacity before the CO2 pulse is released. Is there any evidence for this? This would help me to understand why you don't simply run the full models in the same mode as in section 3.1 (without climate feedback, without changes in terrestrial carbon cycle and without interactions with the sediments), but now for 1000Gt and 5000Gt, respectively, instead of a 590Gt pulse. I do not understand what additional information you gain by using the response functions. Comparing Figures 1 and 6, I also have some difficulties understanding why, e.g., the HILDA model seems to give lowest atmospheric CO2 values for the full model in Figure 1 but not for the surface ocean response model in Figure 6.

Minor points:

The description of the models should explain in more detail whether all models include both hard and soft tissue biological carbon pumps. It is mentioned later (p.4528, l.9ff) that because the model is run under constant climate, effects of the biological pumps should not matter. However, both biological pumps may also change in response to elevated CO2 (e.g. acidification) via processes that some models may include and others don't. Also, it would be interesting to discuss the treatment of air-sea gas exchange in the different models

abstract: The use of "excess CO2" and "anthropogenic CO2" is a bit confusing at this stage, as the reader is not yet able to understand the difference between the emission

BGD

5, S2585–S2589, 2009

Interactive Comment

pulse and anthropogenic CO2. I suggest to use a term like "historical anthropogenic CO2".

p.4525, l.3: change "and is kept constant thereafter" to "with zero emissions assumed thereafter"

p.4527, l.21ff "Many models..." Depending on how you define steady state, I find it surprising that any model should reach something like a steady state within 1000 (or 2000) years. Reformulate or explain in more detail.

p.4529, l.17: "Compared to the atmospheric pulse response functions, the use of surface ocean pulse response functions avoids the problem arising from nonlinearities of the carbon chemistry and gives therefore more accurate results." I do not understand this sentence. The nonlinearities are real. What problem does the method avoid? Why should results be more accurate by avoiding (neglecting?) the nonlinearities in the carbon chemistry?

p.4530, l.2: "Given THE COMPLETE HISTORY OF surface..." Can you please explain how equation 1 is solved in practice?

p.4530, l.20: Models with faster transport "generally" have larger CO2 uptake. This is what one would expect. Perhaps more interesting are the exceptions: Why do models with faster transport not always have larger CO2 uptake? l.25: here you refer to the pulse CO2 as "anthropogenic CO2". Further above, this was correlated with another (i.e. historical) "anthropogenic CO2". Perhaps use different terms to refer to these different tracers?

p.4531, l.16: please include short descriptions of the CC_SED and CLIMBER-2 models in the appendix. What is the motivation to include these models, e.g. CC_SED when you disregard the sediments? Mention whether you have turned off the sediments in these two models as well.

p.4534, l.17ff. Please be more specific about the tracers you mean (CFCs, del-14C,...?)

BGD

5, S2585–S2589, 2009

Interactive Comment

and what for you cite the many papers at the end of the sentence.

p.4542, l.17: Does any of the coarse-resolution EMICs exhibit natural variability of ocean transport? Is the difference between fill and response model a function of time?

Figures 1-5: colors are difficult to associate with the different models. It would be good to present the values of the different models in a table (e.g. year 1000 values for Figs. 1,4,5).

Interactive comment on Biogeosciences Discuss., 5, 4521, 2008.