Biogeosciences Discuss., 5, S3078–S3088, 2009
www.biogeosciences-discuss.net/5/S3078/2009/

Biogeosciences Discussions

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

L. Cao et al.

Received and published: 23 February 2009

We would like to thank both referees for the thoughtful and constructive comments on our manuscript. These comments provide new insights and greatly help improve the manuscript. In the following we answer all the comments from both reviewers.

Response to referee #1

Comments: This paper analyzes the oceanic uptake of a pulse input of CO2 to the atmosphere as simulated by a random assortment of ocean models. There are two potentially interesting findings in this paper, one a comparison of model simulations with observed CFC and radiocarbon distributions (Figures 2 and 3); the other a comparison of the impact of differences between model transports versus differences between climate feedbacks on the airborne fraction of CO2 (Figure 6). However, in my opinion,

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

the authors fail to follow through adequately on the consequences of their comparisons with the observations, and as a result, I believe that the analysis in Figure 6 may be misleading.

Specifically, Figure 2 and especially Figure 3 show that some models clearly outperform other models in fitting the observations. Despite this, the authors include all models in estimating the correlations shown in these Figures and used in their analysis of Figure 4 as well as in their analysis of Figure 6. If these incorrect models were eliminated, the correlations in Figure 4 would mostly disappear and the analysis of Figure 6 would likely show that the climate response is the greatest source of uncertainty. In effect, what I would argue is that these authors have not adequately taken into account what we have learned about ocean transport and what constitutes an "acceptable" model.

Response: We acknowledge the reviewer's comments that the manuscript did not discuss adequately the model's performance in the context of observational constraints. As suggested here and also by reviewer #2, we also calculated correlations by including only models whose simulations of geochemical tracers fall within observational bounds and included these results in revised Fig. 2 and 3 (also see corresponding response to reviewer #2). We have also revised texts in the updated manuscript to strengthen the discussion of model's performance against observations.

We acknowledge that, in general, recently developed ocean models performed better than the OCMIP models in simulating geochemical tracers. The 8-level GENIE model, which was used recently for a number of studies on the long-term CO2 uptake, is an exception. This gives one example that although improvement has been made in ocean carbon cycle models, cautions still need to be taken in the role of ocean transport in simulating CO2 uptake, which is the main purpose of this study. In the revised manuscript, we have added discussions on these issues.

The reason of including all model results in Fig.6 is that we would like to highlight

BGD

5, S3078–S3088, 2009

Interactive Comment

the importance of ocean transport in the model projection of CO2 uptake. Models with unrealistic simulation of geochemical tracers (as compared to observations) do not necessary mean that they are "unacceptable"; and they were and are used in the simulation of CO2 uptake. Therefore, we feel it is appropriate to include all model results in Fig. 6 for the comparison of transport and climate effect on CO2 uptake. We have added discussions in the revised manuscript to address these issues.

Comments: One other thing that bothered me about this paper: the title asserts, in effect, that ocean transport is important in the fate of anthropogenic CO2, as if perhaps this were an original finding. The authors, who are mostly long-standing experts in the field, are obviously well aware that there has been plenty of research done on this problem in the past, but the treatment in this paper is rather shallow. I can think of several model comparison studies and parameter sensitivity studies that could usefully be referred to in this paper. Given how much progress has been made on this problem, for example in the model comparison studies of Orr et al. (2001) and Matsumoto et al. (2004) and in parameter sensitivity studies such as that by Mignone et al. (GRL, 2006), it seems to me that this study could have done a lot more.

Response: We are well aware of the previous model comparison studies on CO2 uptake, including the studies the reviewer listed here, which are cited in the manuscript. We do not claim that this study is original in the research looking at the effect of ocean dynamics on CO2 uptake, but we believe this study makes a unique contribution to this research area and is a useful extension to previous studies. Orr et al. (2001) and Matsumoto et al. (2004) looked at the behavior of OCMIP models and related their modeled present-day CO2 uptake to the simulation of CFCs and radiocarbon. Mignone et al. (2006) investigated the sensitivity of CO2 uptake to the Southern Ocean dynamics by altering surface winds forcing in a single model.

We see the relevance of our study to these studies, but our study is distinct from them and other relevant studies in several important aspects: First, it compares and analyzes the behavior of oceanic CO2 uptake in response to pulse CO2 emissions of

BGD

5, S3078–S3088, 2009

different sizes for many prominent ocean carbon cycle and Earth system models, in addition to the OCMIP models. Since many models presented here are currently used in the study of different aspects of the carbon cycle, it is informative to have a comprehensive comparison of their behaviors in the simulation of CO2 uptake and simulated ocean transport. Second, it uses ocean response function to separate the role of ocean transport from other factors in CO2 uptake, which was not done in the previous studies. Third, it compares the effect of ocean transport and climate feedback for a large number of models, which presents information on the uncertainty in oceanic CO2 uptake associated with uncertainties in ocean transport and climate feedbacks. There are studies looking at the similar issue, but not in the way we did here. For example, Joos et al. (1999) as cited in the paper, compared the effect of ocean transport and climate feedback on CO2 uptake by varying vertical diffusivity and climate sensitivity in a single model. Given these points, we believe that our study greatly extents previous works on the similar issue and make a contribution to the study of ocean dynamic and carbon cycle.

In the revised manuscript we have added discussions on these issues and revised the title of the paper.

The vertical scale in Figure 6 should be expanded.

Response: this figure has been revised as suggested.

Response to referee #2

Comments: 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

BGD

5, S3078–S3088, 2009

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.

BGD

5, S3078–S3088, 2009

Interactive Comment

Response: We acknowledge the reviewer's comments that the high correlation shown in Fig. 2 and 3 are largely a result of a few models whose simulated inventories of CFC and radiocarbon lie well outside the observational errors (please note that CFC results are not available for HILDA. GENIE8 and UL models well outside the observational errors for the CFC11 inventory, not HILDA). After taking into account the reviewer's comments, in the revised manuscript (Fig. 2, 3) we show two sets of correlation coefficients: one for the regression taken for all models and the other for the regression applied only to models that realistically simulate tracer inventories. The purpose of doing this is to show that the simulation of geochemical tracers can be used as indicators for modeled CO2 uptake (strong correlation between CO2 uptake and CFCs/radiocarbon inventories when all models are considered), but does not provide a stringent constraint on modeled CO2 uptake (no clear relationship between CO2 uptake and CFCs/radiocarbon inventories for models whose simulation of CFCs/radiocarbon fall within the observational range).

We would like to emphasize that one purpose of the paper is to show that model-simulated uptake of anthropogenic CO2 is correlated to different extent with their simulations of geochemical tracers, but not to judge model performance against observations. In other words, the point of this paper is to show that model's performance in simulating geochemical tracers can somewhat be used as indicators for their projection of oceanic uptake of anthropogenic CO2. For example, if a model greatly overestimates the inventory of natural 14C in the deep Southern and/or Pacific Ocean, it is very likely that the model would overestimate the long-term oceanic uptake of anthropogenic CO2.

As suggested by the reviewer, a table listing model results shown in Fig.2 and 3 has been added in the revised text.

Corresponding modifications have been made in the text to address the issues discussed above.

BGD

5, S3078–S3088, 2009

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

Response: There are two main purposes of using ocean response functions: 1) to isolate the impacts of ocean transport from other factors, such as buffering capacity and air-sea gas exchange on the oceanic CO2 uptake, as stated by the reviewer; 2) it is not practical to rerun the full OCMIP models to look at their responses to 1000 and 5000 PgC CO2 pulse as discussed in the paper, and ocean response function provides an efficient substitute for these models.

The uptake of CO2 using ocean response functions is not linearly related to CO2 perturbation because of the nonlinearity in carbonate chemistry, as the reviewer stated. We emphasize here that as we stated in the original manuscript (p4531, lines 7-9),

BGD

5, S3078–S3088, 2009

when we calculated CO2 uptake in response to 1000 and 5000 PgC pulse using response functions, we assumed the same buffering capacity (as represented by the derived cubic relationship between surface DIC and ocean pCO2 from GENIE16 simulations) in the calculation. We also assumed the same air-sea exchange rate. In this way, the differences in calculated CO2 uptake are only associated with differences in the rate of ocean transport (as represented by the ocean response functions).

In the revised manuscript we have added a table (table 3) listing modeled steady-state surface DIC, alkalinity, temperature, salinity, and the rate of air-sea exchange to show that the models differ in their steady-state buffering capacity and air-sea exchange, in addition to the rate of ocean transport. This strengthens the discussion of why ocean response functions are used.

The reviewer might misread the color scales of different models in Figure 1. The UL model, but not the HILDA model gives the lowest atmospheric CO2 values (the highest oceanic uptake). As the reviewer suggested, we have added a table listing the values for all models that would facilitate the readers.

We have revised corresponding texts in the modified manuscript to address the issues discussed above.

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.

Response: OCMIP models used here do not include marine biology. All other models

BGD

5, S3078–S3088, 2009

include both hard and soft tissue biological carbon pumps and no model runs include effects of elevated CO2 (e.g., acidification) on biological pumps. Corresponding texts have been revised to clarity these issues. The rate of air-sea exchange for different models is now given in table 3.

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

Response: The texts have been revised as suggested.

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

Response: revised.

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.

Response: We mean that 1000 years after the CO2 pulse, the deep ocean has not equilibrated with the perturbation in atmospheric CO2. Texts have been revised in the revised manuscript.

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?

Response: the sentence has been revised in the modified texts to clarify the confusion. "The use of surface ocean pulse response functions overcomes the problem arising

BGD

5, S3078–S3088, 2009

Interactive Comment

from nonlinearity of the carbon chemistry and gives more accurate results compared to atmospheric pulse response functions".

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

Response: In practice, we solve equation 1 by using complete history of model output of global and annual mean surface DIC and air-sea CO2 flux, together with the information of the top model layer thickness. An appendix has been added to discuss in detail 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?

Response: In fact, models with faster transport always have larger CO2 uptake. Texts have been revised. The use of "anthropogenic CO2" has been revised correspondingly in the updated manuscript.

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.

Response: We have included the descriptions of these two models in the appendix (as in the original manuscript). The motivation to include these models is to bring as many model results as possible into the comparison of climate effect and transport effect on CO2 uptake. In the manuscript, we have stated that sediment effects are disabled in these two models.

BGD

5, S3078–S3088, 2009

Interactive Comment

p.4534, l.17ff. Please be more specific about the tracers you mean (CFCs, del-14C,...?) and what for you cite the many papers at the end of the sentence.

Response: The texts have been revised as suggested.

p.4542, l.17: Does any of the coarse-resolution EMICs exhibit natural variability of ocean transport?

Response: Natural variably in ocean transport is seen in models with explicit representation of ocean dynamics.

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).

Response: As suggested, a new table (Table 2) in the revised manuscript has been added to present model results.

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