Biogeosciences Discuss., 6, C4573–C4576, 2010
www.biogeosciences-discuss.net/6/C4573/2010/

Biogeosciences Discussions

Interactive comment on "Trends and regional distributions of land and ocean carbon sinks" *by* J. L. Sarmiento et al.

C. Heinze (Referee)

christoph.heinze@gfi.uib.no

Received and published: 1 March 2010

The paper by Sarmiento and co-workers addresses a hot topic in actual biogeochemical climate research, namely the changing ocean and land carbon sinks over time during the period of most recent significant increases in fossil fuel CO2 emission rates. The general approach – computing the land sink carbon sink from emission estimates, ocean model results, and atmospheric CO2 data – is not entirely new. Given the uncertainties in current carbon cycle budgeting efforts, the main conclusion from the paper appears to be in line with Canadell et al (2007, PNAS) and LeQuéré et al (2009, the main author is co-author of the latter paper). The study would thus rather corroborate earlier results through adding more ocean models to the analysis than providing a significantly new conclusion. The use of several ocean models could warrant publication

of the paper. As oceanographers, the authors of the paper could add a more in-depth analysis on problems in the ocean models which influence the result and then challenge earlier estimates of the ocean and land net carbon sink over the past decades. Such a critical appraisal would be most helpful for the carbon cycle community. Adding this, the paper could become very interesting. I recommend publication after a successful a revision.

Comments in detail:

Abstract: The following passage is misleading, as the 1% critical level in the analysis is - if a I am not mistaken - used for computing the difference between the climatological run by Mikaloff-Fletcher and the synoptically forced BOGCMs – all of them associated with considerable errors themselves and may not fully represented real processes: "The net land carbon sink appears to have increased by -0.88 (-0.77 to -1.04) PgCyr-1 after $\sim$1988/1989 from a relatively constant mean of -0.27 PgCyr-1 before then to -1.15 PgCyr-1 thereafter (the sign convention is negative out of the atmosphere). This result is significant at the 1% critical level." Furthermore, the passage gives insufficient time information – "from when" to 1988/89 and "to when" from 1988/89 are missing.

p. 10584: "...ocean models can account for $\sim$33%...": This is not backed up properly. Sabine et al. (2004) do not give this number directly. Also not the models take up the CO2, but the ocean.

p. 10585: "This study was originally motivated by two specific results from recent literature that raised some questions in our minds regarding some of the conceptions that we had formed about the atmospheric CO2 growth rate and land carbon sink." The sentence deals with vague personal issues and should be removed.

p. 10585: "...we were uneasy about...": This is subjective language which should be avoided.

p. 10585/6: Items 1, 2, and 3 in the list: Canadell et al. (2007) discuss the airborne fraction issues of short term variability and long-term trends and make a statistical analysis for its long-term trend. The authors seem to be unnecessarily confrontial here.

p. 10586: "For this to be true, the carbon sinks, which determine how much of the CO2 emitted into the atmosphere actually stays there, must be directly proportional to the sources, which is only likely to be true under certain circumstances." This is already reflected in the interannual variations of the airborne fraction and thus also clear from the analysis in Canadell et al. (2007). If the statement would be left in the manuscript: What would be the circumstances under which this is true?

p. 10591/2: "The observational analyses and model results suggest that the decline in oceanic uptake if it stands up to continued investigation, is likely a complex global scale phenomenon that alters the current distribution of oceanic sources and sinks, and that it involves changes in both the 'natural' carbon cycle that existed before the Anthropocene as well as to the rate of uptake of the anthropogenic perturbation per se." The authors mention here a major issue associated with their analysis: How good are the ocean model results really to allow their conclusions? I think that the paper could here make a fabulous step forward in discussing the problems associated with state of the art ocean models. This is partly done on p. 10602, l. 1-15, but could be more elaborate, e.g.: Are the initial conditions of the ocean models appropriate for the analysis (e.g. is a spin-up with perpetual detrended NCEP forcing appropriate)? Are the models rendering a correct long-term and decadal variability? Could the apparent increase in net land carbon sink rather be a net ocean carbon sink increase – or can this be excluded with certainty? Are the ocean models rendering short term variability in the ocean correctly (compare, e.g., with the results of Schuster and Watson, 2007; Watson et al., 2009, Science)?

p. 10603, conclusions: So far, I do not see the results presented in this paper in conflict with Canadell et al (2007) or LeQuéré et al (2009) especially in view of existing uncertainties in all flux estimates and inventory estimates over time. If the authors could

in fact document a decreasing trend in airborne fraction as their statement ("implying that the atmospheric growth rate decreased over time with respect to its 'expected' behavior") may suggest, they would need to describe this much more clearly and show a diagram of their estimated airborne fraction. Can the conclusion of a lower than expected atmospheric CO2 growth rate unequivocally be deduced form current models and observations?

Figures 1,3, and 6: The yellow lines are difficult to see in print.

Figure 1c: The y-axis needs to be spread, so that the different high variability curves from the different models can be identified.

I look forward to the revised version of this interesting paper.

Best wishes, Christoph Heinze

Interactive comment on Biogeosciences Discuss., 6, 10583, 2009.