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Biogeosciences Discussions

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

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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.

RESPONSE: While the approach is not novel, the duration of the record is becoming long enough that one can begin to discern with greater and greater confidence the patterns that stand out from the interannual and multidecadal variability. Furthermore, our finding of an abrupt shift in the net terrestrial land sink around 1990, which is our main conclusion, is an entirely new result.

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).

RESPONSE: As noted above, our main conclusion is that there has been an abrupt shift in the terrestrial land sink some time around 1990. This is completely different from any of the material discussed in Canadell et al. and Le Quere et al. Clearly, our attempts to compare the implications of our results with the main conclusions of these papers have distracted from our main message. We will attempt here and in a revised manuscript to be clear on this.

Canadell and Le Quere et al. have concluded that climate is already having an effect on the carbon cycle leading to a reduction of both the land and ocean carbon sinks and that this is causing an increase in the airborne fraction. Here is what we have to say about each of these points and how we differ or agree with these authors:

(1) OCEAN CARBON SINK: Our analysis of ocean model simulations leads us to agree with Canadell et al. and Le Quere et al. that the ocean carbon sinks may indeed be responding to climate with a reduced uptake.

(2) LAND CARBON SINK: The present paradigm for the land carbon sink is nicely illustrated by Figure S2a of Le Quere et al., which shows that land models forced with CO_2 alone but no climate change have an ever increasing sink that goes from ~1 Pg C/yr in 1960 to ~3.5 Pg C/yr at present. Adding climate forcing to these models greatly increases the variability and has a tendency to decrease the sink overall. While we do not have a specific issue with any of these overall simulation results (indeed, our work with such models gives essentially the same results), we have low confidence in our ability to simulate the land carbon cycle correctly and to validate the models with observations. Thus an independent, observationally-based analysis is warranted, using the net land carbon sink as a diagnostic. The net land carbon flux is calculated from the three best-known terms in the carbon balance: the atmospheric growth rate, fossil fuel emissions, and the observationally validated ocean uptake model calculations. Our analysis of this diagnostic suggests to us that the net land carbon flux was pretty much in balance prior to ~1990, with land carbon sinks closely matching land carbon sources, and that after this time the land carbon sinks appear to have exceeded the land carbon sources by ~0.9 Pg C/yr, with the change having occurred rather abruptly and the atmospheric growth rate having decreased by a similar amount.

(3) AIRBORNE FRACTION: As discussed in the introduction to our paper, we are concerned about the use of the airborne fraction as a diagnostic of the efficiency of the land and ocean carbon sinks because numerous other processes than just changes in the sinks can influence it. This is specifically why we chose to analyze the net land carbon sink and atmospheric growth rate directly. Our analysis of these properties leads us to conclude that the atmospheric growth rate has been slower than might have been expected, opposite to the conclusion reached by Canadell et al. based on the increase in the airborne fraction, which Le Quere et al. also agree with.

NOTE: As noted by the reviewer, JLS is a co-author of the Le Quere paper. JLS contributed an ocean model result to the Le Quere et al. study, which has an excellent analysis of land as well as ocean models and the effect of CO2 increases and climate change on the land and ocean carbon sinks. While Le Quere et al were very cautious in the interpretation of the airborne fraction, we did not feel comfortable with the use of this parameter as an indicator of carbon sink efficiency and thus have adopted a different approach here.

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.

RESPONSE: This more detailed analysis of the behavior of ocean models over the past 50 years, the sensitivity to different wind products, different model configurations, etc., is indeed an interesting problem which we are addressing in a separate ongoing study. However, it is a separate problem than the one we address in this paper.

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 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.

RESPONSE: We will modify the text accordingly

p. 10584: ". . .ocean models can account for 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.

RESPONSE: Will clarify.

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.

RESPONSE: We agree that the language is vague. We will remove this material from the paper. The problems we were drawing attention to are in a now submitted paper where we show the supporting findings.

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?

RESPONSE: We will remove this and leave a discussion of this for the separate paper now submitted.

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)?

RESPONSE: As we noted above, this more detailed analysis of the behavior of ocean models over the past 50 years, the sensitivity to different wind products, different model configurations, etc., is indeed an interesting problem which we are addressing in a separate ongoing study. However, it is a separate problem than the one we address in this paper.

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.

RESPONSE: See comments above regarding what is new in our analysis relative to Canadell et al. and Le Quere et al.

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?

RESPONSE: As noted previously, and as we show in a paper presently in preparation, the airborne fraction is not a good diagnostic of the efficiency of the carbon sinks, thus even if we could demonstrate that the AF decreases over time (which it does not), this would not support our contention. Our conclusion of a lower than expected growth rate of atmospheric CO_2 is based on our finding of a faster than expected growth rate in the land carbon sink. However, this is not a model based result (except that ocean models are used to calculate the net land carbon sink); it is an observationally based result. We will modify the text to indicate clearly the distinction between our estimate of the expected land carbon sink and expected atmospheric growth based on extrapolating historical behavior, and the more usual definition in terms of models that this reviewer clearly has in mind.

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

RESPONSE: Will change.

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

RESPONSE: The land uptake estimates are so close to each other relative to the interannual variability that we do not believe blowing this figure up would help.