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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.

Anonymous Referee #1

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Sarmiento and co-authors present an analysis of the trends in global land and ocean CO2 sinks. The authors combine the land use change emissions with the residual land sink, and analyze the changes in this "net land sink (NLS)" through time. This is an interesting analysis as the authors show that the NLS has increased in the past 50 years, with possibly an abrupt change occurring around 1988/1989. The authors argue that an increase in NLS contradicts recent publications by Canadell et al and follow up studies which have claimed that the residual land sink and the ocean sink are responding to climate change.

RESPONSE: The reviewer's summary of our main conclusion regarding the increase in NLS is exactly right. As regards our disagreement with the Canadell et al. and follow up studies, we did not intend to convey that we disagreed with their finding that the land and ocean carbon sinks are responding to climate. What we disagree with is their overall conclusion that the observed increase in the airborne fraction indicates that the sinks have lower efficiencies than expected. 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 based on models 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 the 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. However, it is clear from comments from both reviewers that our attempts to compare and contrast our findings with the conclusions reached on the basis of the analysis of airborne fraction by Canadell et al. and Le Quere et al. have served as a distraction from the main message of the paper, and we propose as a solution to eliminate most of this material from this paper (a separate paper on this topic has now been submitted) and to make sure that our message is clearer.

The possibility that the CO2 sinks may be responding to climate change is heavily debated in the field. This paper could potentially provide an interesting angle to this debate and contribute to resolving the current scientific discussion. However, I have several major criticisms of the main arguments presented which prevent me from recommending the paper for publication in its present form. My major concerns are:

1) The authors assume in the paper that an increase in the residual land sink is inconsistent with a response of the residual land sink to climate. This is not true. Because CO2 emissions have greatly varied historically, as mentioned in the paper emissions grew at rates of 4% per year for nearly 20 years (1960-1979), it is well possible that the residual land sink may be responding to recent climate change while still increasing with time. By ignoring this possibility, the authors then state in the abstract an in the main paper that the atmospheric growth rate did not increase as fast as expected (see also point 9 below).

RESPONSE: Actually, as noted in our comment on the land sink above, we agree with what the reviewer is saying about the behavior of the land carbon sink over time and will clarify this point. In writing this paper, we preferred to take an agnostic approach on the land carbon sink mechanisms, because we do not believe that the data are able to tell us what the mechanism is, and we do not want to use the models to tell us. While the models are informative, they also have high uncertainty and a tendency to give you back pretty much what you put in. Our approach is to use the past behavior of the net land carbon sink to tell us what the future behavior might be. We obviously need to be very clear and explicit about this.

2) The treatment and discussion of uncertainty is not systematic throughout the paper, particularly for the land use change uncertainty. The authors first argue in the introduction that estimates of land use change are too uncertain to provide reliable estimate of airborne fraction and may lead to substantial biases in the trend analysis. Yet when time comes to discuss the possible effects of trends in land use change on their estimated trend in NLS, they cite one single estimate of land use change (Houghton et al) and state that "a decrease in land use emissions is not supported by existing publications". This statement and the discussion ignores the many publications that have assessed trends in land use change in recent years, including VanMinnen et al. (Climatic Change 2009), Shevliakova et al (GBC 2009), McGuire et al. (GBC 2001). If you compare the estimates of land use change from these models with estimates of land use change based on satellite data for the more recent periods, the overall data are consistent with a decrease in land use change. If such a decrease had occurred, it could easily explain the increase in NLS. Yet this possibility is not discussed seriously in the paper. Instead, the abstract seems to promote a hypothesis based on direct measurements of NLS, which is at least as uncertain as estimates of land use change. The authors analysis of trends in NLS could bring some new light in the current scientific discussion, but only if they can provide a balanced discussion of the possible causes.

RESPONSE: We agree that there is a very large uncertainty here and did not intend to dismiss a change in land use as a possible explanation for the net land sink signal that we see. We did analyze the other estimates referenced by the reviewer (cf. Le Quere et al., Figure S1 top). The only ones of these estimates that extend beyond our changepoint in 1989 are the Shevliakova et al. and Houghton estimates, and neither of these shows a significant drop in the sources at anywhere near 1989. The McGuire et al. (2001)a and Van Minnen et al. (2009) results show a decrease in the land carbon sink starting ten years earlier around 1980, but the estimates end in ~1992, too early to be of use for our analysis. We will add something to this effect in our discussion.

3) The paper mis-interprets the results of Canadell et al. (2007) and does not acknowledge the follow up analysis presented in Le Quere et al. (2009). Concerning point 1. in the introduction, a measure of the effect of uncertainty in LU on the airborne trend was assessed in Le Quere et al., and could be acknowledged here. More importantly, in point 3., the factors that can potentially influence trends in airborne fraction were fully spelled out in Le Quere et al.

(2009).

RESPONSE: A separate study has just been submitted by a subset of us that specifically addresses the interpretation of the airborne fraction. It is clear that the tone that we adopted here in our critique of the previous studies is a distraction from our main message regarding the time trajectory of the NLS. We conclude it would be best to leave a discussion of this issue for the papers where we show the supporting findings and will remove the material that this reviewer has problems with.

This paper showed with a series of land and ocean models that the impact of recent changes in climate on both CO2 sinks appears to be responsible for the positive trend in airborne fraction. This is not only a result of the positive trend in airborne fraction. The Canadell paper already included a comparison with model-estimated airborne fraction trends (from the C4MIP simulations) to support its statement on the role of the sinks in driving positive airborne fraction trends. Thus the reference in the Sarmiento et al. paper to "implicit assumption" is not justified. The conclusions were based on a comparison with model results. In general, the introduction and rationale of this paper are very negative. I think that the best rationale for looking at NLS is that the uncertainty in this term is smaller than the uncertainty in the residual land sink.

RESPONSE: As already noted, we have no quarrel with the model studies in Canadell et al. and Le Quere et al. regarding the impact of climate on CO2 uptake in the models. This will be clarified.

4) There is some confusion throughout the paper regarding the existence of a NLS, which no one denies, and the possibility that it may be responding to climate, which is debated. The confusion first arises in the abstract, where important statements are made regarding the increase in NLS after 1988/89, immediately followed by statements regarding an assessment of a large NLS. The abstract seem to suggest that observations of a large NLS support the increase in NLS, whereas they are not necessarily related. Similarly in the introduction, direct NLS estimates are presented right after the discussion in trends in airborne fraction without an explicit transition. The authors need to clarify their manuscript throughout to clarify when they refer to the mean sinks and when they refer to trends, and to make it clear if their results imply that the mean and trends are related.

RESPONSE: In our analysis, it is the increase in the mean before and after ~1989 that indicates to us that there has been a change. We do not see clear evidence of a trend, if we define a trend as a gradual linear or exponential-like increase in time. Figure 6a showing the smoothed land uptake and figure 6b showing the abrupt change in the slope of the cumulative uptake are what suggest this to us, as well as other ongoing work on this. We will rewrite the paper to be clearer and more explicit about this point.

5) I am unconvinced by the use of the Mikaloff-Fletcher estimate as a basis for the expected ocean CO2 sink. The expected ocean sink should roughly follow the growth rate in atmospheric CO2 (to a first order). Yet the atmospheric CO2 fluctuations are barely visible on Figure 1b. This estimate gives a far larger growth in CO2 sink through time than the OGCM. There are many estimates of the ocean sink available (e.g. the OCMIP results), which the authors can use to check that the inverse analysis does not over-estimate the trend, as the OGCM would suggest. This is important as it impacts the trend in NLS.

RESPONSE: Actually, as shown by Sarmiento et al. (1995, GBC), the oceanic uptake rate is approximately proportional to the cumulative increase in atmospheric CO_2 from the beginning of the industrial revolution, which does not vary by much from year to year. The review paper by Gruber et al. (2009, GBC) shows that all the oceanic uptake estimates by tracer validated ocean models, and data based methods, agree very well with each other and with the Mikaloff-Fletcher et al. estimate that we use. If we were to use a collection of models such as OCMIP, particularly if we were to indiscriminately use a wide collection of such models without regard to how well they fit the observed tracer distributions, it is likely that we would introduce uncertainty which is not warranted by the strong constraints that we have on the ocean carbon sink. We will add some discussion in the text to this effect.

6) I found it confusing that the authors refer to "top-down" estimates for both their budget approach and for inverse studies. The manuscript would be easier to follow if they used a different name for the budget approach. I also found confusing to have results in the introduction. The information is presented without the appropriate methods, and the reader is left with little information to interpret the results.

RESPONSE: Good points. The paper will be modified accordingly.

7) In the introduction, the ocean models cannot account for 33% of the fossil fuel emissions. Take any carbon-climate model and force it by increasing fossil fuel emissions alone, and the fraction taken up by the ocean will be less than 33% (closer to 25%). The oceans are influenced by both the emissions of fossil fuel and land use. This 33% fraction is misleading as it shows a larger role for the oceans than is mechanistically realistic. There are other ways to phrase this information that would be more correct.

RESPONSE: We see two possible issues that the reviewer is raising here. We assume that the most likely issue being raised is essentially one of whether the

% ocean uptake should be calculated with respect to the fossil fuel source only, as we have done, or with respect to the sum of the fossil fuel and land use sources. We have used the former approach because of the large uncertainty in the land use source. We will add wording explaining why we have done this and also explain that the % would of course be less if it were calculated with respect to the sum of the fossil fuel and land use sources, and include a calculation of this alternative quantity.

A second possible issue being raised here has to do with whether our estimate of the oceanic uptake is correct. Indeed, if we forced our model with only the FF emissions, the atmospheric CO_2 would not necessarily match the observations and the oceanic uptake would be biased for that reason. However, our estimate of 33% of the total uptake is based on ocean models forced with the observed atmospheric CO_2.

8) The use of "we are concerned" in the introduction exacerbates the apparent conflict between the proponents and the opponents of airborne fraction analysis, and puts it at a personal level rather than at a scientific level. Same for "raised some questions in our minds". This is unnecessary.

RESPONSE: We did not intend to set a confrontational tone by our choice of wording, indeed, much the opposite. However, while we are convinced that the conflict is more than just apparent and we are working on several fronts to clarify the issues, we now feel it is best to remove all this confrontational material and let the science speak for itself – including especially a follow-on paper on the airborne fraction that a subset of us have just submitted.

9) The final statement in the conclusion that the "net land carbon sink appears to have increased relative to expectation" is not supported by evidence presented in this paper. There is no evidence of what the expectation is for the NLS. For the ocean sink, this is indeed justified with the use of models forced by constant climate. Unless the authors present land model results, they cannot say what are the expectations.

RESPONSE: There are considerable uncertainties in the lands models and rather than use land models to determine the expected behavior of the land carbon sink, we here take a complementary approach of asking what the expectation would be if the behavior of the system were to continue as observed in the past. The long period of time prior to ~1990 when the NLS was nearly constant suggests as a reasonable expectation, that it might have continued to remain constant in the future, and thus the increase in the NLS after ~1990 is a "surprise." We will reword the discussion so as to make this point clear and also make more careful reference to alternative definitions of "expectation" in this context, and to the land modeling studies, which do indeed show an increase over time.

The land sink in the Canadell paper increased through time, and that is still perfectly coherent with an increasing airborne fraction. It is thus incorrect to say that the evidence presented here differs from that presented in Canadell et al..
Interactive comment on Biogeosciences Discuss., 6, 10583, 2009.

RESPONSE: See extensive comments in our first response above.