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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 analyse 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. 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).

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.

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). 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.

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.

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.

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.

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.

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.

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. 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.