Biogeosciences Discuss., 10, C8704–C8717, 2014
www.biogeosciences-discuss.net/10/C8704/2014/

Biogeosciences
Discussions
Open Access

Interactive comment on "Trends and drivers of regional sources and sinks of carbon dioxide over the past two decades" *by* S. Sitch et al.

M. R. Raupach (Referee)

michael.raupach@anu.edu.au

Received and published: 16 February 2014

1 Overall assessment

This paper builds upon the RECCAP (Regional Carbon Cycle Assessments and Processes) project, a major international effort by the carbon cycle community. The aims of the paper (P20118) are to quantify regional carbon exchange processes over 1990-2009, and to identify the driving processes. The paper will eventually be a significant contribution, meeting both of these aims. It synthesizes a vast amount of work by numerous terrestrial and ocean modeling groups to come up with a picture of regional carbon cycle processes over the last two decades that is globally consistent with previous work, e.g. Le Quéré et al. (2009).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The regional findings can be summarized briefly as (1) the land CO_2 sink has increased over the study period, almost entirely through increases in tropical and southern regions with negligible increase in northern regions; (2) the ocean sink has increased little if any; (3) globally and in most regions, both the land and (especially) the ocean sinks are not increasing as fast as the growth rate of excess atmospheric CO_2 above preindustrial. The last of these findings is not highlighted in the abstract, though it is given attention in the paper itself. I think it is one of the keys to the analysis.

2 Major issues

There are several big issues that together require an assessment of "major revision" for this paper. In roughly decreasing order of importance, these are: (1) lack of a consistent analytical framework; (2) failure to address the issue of testing models against observations; (3) inadequate use of global constraints; (4) failure to mention (let alone address) volcanic influences; and (5) stylistic problems, including inconsistencies between the land and ocean parts of the paper, and a turgid style with too many numbers pulled from tables and not enough high-level interpretation.

2.1 Lack of a consistent analytical framework

I do not think that the analytical framework in this paper is adequate. At this relatively advanced stage in the development of understanding of the global carbon cycle, and building upon major contributions cited in the introduction of the paper, a framework is needed to turn regional information about C exchanges into a globally coherent narrative. This would deliver further insights into questions such as: which aspects of regional flux patterns in space and time are driven by global factors, and which by local perturbations or stochastic factors? How are the regional C fluxes, and especially their

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

trends, related to the corresponding global quantities? How are trends in responses (e.g. regional sinks) related to trends in drivers (e.g. atmospheric CO_2)? These questions call for a framework that considers not only absolute quantities (fluxes or trends in $\mathrm{PgC\,y^{-2}}$) but also relationships or ratios between quantities and between trends. Much information in the paper is presented in terms of absolute trends, and the paper could answer its own framing questions much better by considering relative as well as absolute trends. This calls for extra columns in tables, and better discussion of results.

Since land and ocean CO_2 sinks are fundamentally driven by rising atmospheric CO_2, modulated by geography and climate, one key metric is the ratio of sink strength to excess CO_2, or the sink flux per unit excess CO_2 above preindustrial (∼280 ppm). This quantity has been called the “sink efficiency” by (Gloor et al., 2010). The present paper uses the term “sink efficiency” in a number of places, but doesn’t define it. Sink efficiency is fundamentally important because a gives insight into the critical question of whether the sinks are growing faster or slower than excess CO_2 (the primary driver): if the efficiency is decreasing, then the sinks are growing more slowly than excess CO_2, and vice versa. The sink efficiency and its trend can be found both from models (at regional and global scales) and also from simple carbon-budget observations (at global scale).

In two recent analyses (Raupach, 2013, Raupach et al., 2013), I looked at this quantity in detail, calling it the “sink rate” – a name used because it is the instantaneous rate of CO_2 drawdown by sinks. (The second paper, still under review for Biogeosciences, was with colleagues, some of whom are co-authors on the present paper).

At global scale, carbon-budget observations show that the combined global (land + ocean) sink efficiency over 1959-2009 has declined quite strongly, at about -0.8% per year (Raupach, 2013). All regional investigations of sink efficiency, including the present paper, are constrained by this global trend. Several broad factors can cause the sink efficiency to change with time. Four of the most important are:

(1) nonlinear responses of sinks to CO_2 (CO_2 fertilization of NPP, ocean chemistry,

etc.)
(2) responses of sinks to climate change (effects of temperature, precipitation etc.)
(3) responses of sinks to the trajectory of forcing by CO_2 emissions (specifically to departure of forcing from exponential);
(4) volcanic effects on CO_2 sinks.
Of these, the first two are prominent in this paper (and are familiar) but the third and fourth are missing entirely. These are critical omissions for any work aiming to elucidate the processes affecting trends in sinks, the second of the two major goals of this paper.

An attribution with a relatively simple carbon cycle model (Raupach et al., 2013) locates most of the global decline in the sink efficiency in the ocean sink (that is, the land sink has grown at a similar rate to excess CO_2 but the ocean sink more slowly). This finding is consistent with the present paper, where the land sink over 1990-2009 is found to increase whereas the ocean sink has stopped increasing.

The same analysis (Raupach et al., 2013) also quantified the contributions of the above four main factors: about 20% of the decline is from (1) nonlinear CO_2 effects, 20% is from (2) carbon-climate interactions, 35% is from (3) sink responses to the non-exponential trajectory of observed emissions, and 25% is from volcanic effects. The important issue here is not these precise numbers (which may well be modified by further developments) – rather, it is the principle that there are at least four factors responsible for trends in sink strengths and sink rates. A central problem with the present analysis is that, in its efforts to discern the processes leading to these trends, only the first two of the above four factors are considered. This is likely to be grossly misleading, as the other two factors may well be comparably or more important. This limited view is exposed, inter alia, on P20143.

The neglect of factor (3) above is related to a widespread perception that a linear carbon cycle (with no nonlinear responses to CO_2 and no climate effects on sinks) would yield a constant sink efficiency, irrespective of the emissions trajectory. This belief is incorrect. The confusion may have stemmed from Gloor et al., (2010), who sought to discern the effects of emissions trajectories on the airborne fraction (AF), and in particular whether a constant AF could be attained with a non-exponential emissions trajectory. To do this they imposed a priori the assumption of a constant sink efficiency. This is self-contradictory, because a constant sink efficiency is only attainable when both of two conditions are attained: that the carbon cycle responses are linear in excess CO_2, and that emissions increase exponentially. In this respect the sink efficiency is similar to the AF, which also is constant only under both of these conditions. [The result for the AF was derived by Bacastow and Keeling (1979), and the generalisation to the sink rate or sink efficiency, and indeed any ratio between fluxes and stores in the carbon-climate system, was done in Raupach (2013)]. These are strictly mathematical results, logical consequences of the governing equations. Thus, factor (3) above is critical.

2.2 Failure to address the testing of models against observations

I am not calling for this paper to include detailed comparisons of DGVMs and OBGCMs with observations, but I am calling for pointers to other work where such comparisons have been done, and discussion of the implications. One example is the comparison of a large number of DGVMs, including most of the ones used here, with data from FACE studies (De Kauwe et al., 2013) for basic water and carbon fluxes and ratios, principally the water use efficiency, GPP/transpiration; also see Wårlind (2013). The result was massive scatter, with the model range failing to even bracket the observations at one of the two flux sites. In large-scale applications of DGVMs, it is important to acknowledge an unpleasant reality: when tested against observations, the DGVMs often don't work very well.

One way forward is through the use of multiple data sets of different kinds to constrain models. This "multiple-constraint" approach has been used successfully in RECCAP itself (Haverd et al., 2013a,b). This could be highlighted in the discussion and conclusion part of this paper, as a pathway for progress on the ongoing challenge of getting models to match observations tolerably well.

2.3 Use of global constraints

There are global constraints on all the quantities being described in this paper, both fluxes and trends. This is obvious and well known for the fluxes, from the CO_2 mass balance: [atmospheric CO_2 accumulation] = [FF emission] + [LUC emission] + [land-air flux] + [ocean-air flux]. However, it also applies to the trends, because just as for the fluxes themselves, we have (where trend(X) is the slope of X over some period, here 1990-2009):

trend(atmospheric CO_2 accumulation) = trend(FF emission) + trend(LUC emission) + trend(land-air flux) + trend(ocean-air flux).

The trend (and flux) estimates in this paper can be subject to these consistency checks. A table of global estimates for 1990-2010 of all the terms in both the flux and trend equations is necessary, and will establish the consistency of the various model-based estimates with mass-balance requirements. This goes beyond Table 2 by including trends for atmospheric CO_2 accumulation, FF emission and LUC emission, at global scale, and the mass-balance residuals. Model-ensemble information for regions (summing to global values) would be much more useful in this table than the individual-model values, which can be relegated to supporting material.

Also of major importance is the relative growth rate (RGR), defined for a quantity (X) as $\mathrm{RGR}(X) = \mathrm{trend}(\ln |X|) \simeq \mathrm{trend}(X)/\mathrm{mean}(X)$. The same table can give (as well as trends) the RGR for all the fluxes discussed here, including land and ocean regions. The RGR establishes whether responses (such as the land and ocean C sinks) are growing more or less rapidly than drivers (such as excess CO_2 or CO_2 emissions). Of particular importance is the RGR for sink efficiency.

2.4 Volcanic influences

The Pinatubo eruption in 1991, the start of the study period, was one of the largest natural perturbations on the carbon cycle in the last century, having a major influence on many carbon cycle processes (Frölicher et al., 2013). I've indicated above that this has major implications for sink efficiency at global scale. Some discussion of this influence, and its regional imprints, seems essential in this paper. Reference can be made to the excellent analysis of Frölicher et al. (2013) and papers cited there.

2.5 Stylistic issues

There is a marked discrepancy in approach and style between the land and ocean sections of the paper. In the land sections, most results for trends are given as absolute trends in $\mathrm{PgC\,y^{-2}}$. These numbers do not mean much by themselves: the important issues are how regional trends contribute to the global trend, and their relationships to trends in underlying drivers. I've argued above that the best way to look at this is through trends in sink efficiency – a concept used in the ocean section, but hardly at all in the land sections.

Thus, the presentation needs to be rethought, especially in the land sections (3.2 and others). The reader should not have to wade through a turgid mass of figures and then do a lot of mental arithmetic to reach the important high-level conclusions. For instance, a main story across the whole of sec 3.2 seems to be that the global increasing trend in land C sink is about 2/3 due to the tropics and 1/3 to southern land, with a zero trend in northern land, and this partitioning in trend is quite different from the partitioning of the C sink fluxes themselves, which is more like (45:40:15) for (northern, tropical, southern). Similar high-level conclusions are needed on the trends in regional land C sink efficiencies.

In the land sections, a common phrasing is “the flux/trend was $X \pm x$, $Y \pm y$ and $Z \pm z$

for regions A, B and C, respectively". This is very hard to read: the reader has to associate a thicket of numbers with another thicket of regions. Where numbers have to be given, much clearer would be "The flux/trend for region A was $X \pm x$ (units); for B it was $Y \pm y$, and for C, $Z \pm z$". (Usually the units are only needed once). To see how often this occurs, search "respectively". Many of the numbers that are laboriously stated in this way are simply pulled out of the tables. The reader can easily go to the tables for this. What the text needs to do is to state the implications: is the number for A larger or smaller than for B? What are the relative contributions of A and B to global fluxes, trends, sink efficiencies etc?

3 Minor issues

P20117 L6: Greenness is a function green leaf biomass and hence measures a stock, not a flux.

P20118 L15: "world", not "World"

P20118 L20-21: Inconsistent definition and use of LUC, LULCC

P20121 L11: Does a "constant land use" refer to an assumption that land cover is held constant? What about the LUC emission flux? It is implied later (P20125 L11) that LUC emissions are from Houghton (2010), and of course imply deforestation. Comment is needed about the contradiction between this and the imposition of constant land use.

P20122: What was the spin-up time and protocol for S_L1 and S_L2? Given that protocol, is there any issue with drift, as there is for the ocean models (next page)? Why is there no explicit preindustrial steady-state simulation for land as there is for the ocean with S_O3 (P20124)? [The answer may lie with the absence of very long time scales in land response functions compared with ocean, but comment is needed].

P20123: This summary of initialisation protocols for the ocean models is very useful.

P20123 L23: It is critical to define “climatological atmospheric BCs”. It this a steady-state climatology, or does it retain long-term climate change (warming etc)? If the latter, how much variability is rejected – (interannual, decadal, etc.)?

P20123 L22: S_O1, S_O2 have not been defined yet. Consider moving the definitions to the top of this section, before the discussion of initialisation.

P20123 L23: “From $\sim$ 1950 onward” should be the start of a new paragraph

P20124 L5: Seems to be a missing “CO_2” after “preindustrial”

P20124 L22, and P20125 L2: The equation defining NBP needs to be given as soon as NBP is introduced, not in another 6 lines. In this equation, presumably only NPP and RH are evaluated by all DGVMs. How were the other fluxes (fire, riverine C flux, harvest) evaluated consistently? Also, what about other fluxes: grazing, herbivory, and the aeolian C flux (which can be a significant fraction of NPP in semi-arid environments)? While these can’t be included in the present work, their existence should be noted.

P20124 L24: The sign convention has been stated already (P20116 L24).

P20125 L11: See comment on P20121 L11

P20126 L23: X(S_O2) $-$ X(S_O1) is referred to as “the contribution of climate variability”, but 3 lines later (L26) as representing “the impact of climate change and variability on the ocean C cycle”. This is a critical distinction and the ambiguity makes some of the ensuing results hard to follow. See comment on P20123 L23.

P20127 L21: For all $\pm$ ranges, the assumed confidence interval needs to be stated: 1 standard deviation, 2 standard deviations, etc. Failure to do this would make the entire uncertainty apparatus useless.

P20127 L21: Define precisely the meaning attached to P. Is it the probability of a trend statistically indistinguishable from zero? To what confidence level?

P20128 L5-L8: Do the two trends in the N-enabled land C sink (-0.02 and -0.05)

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

refer respectively to OCN and CLM4CN? (See elsewhere about the use of "respectively"). More importantly, are these trends statistically distinguishable (at some stated confidence level) from the ensemble-mean trend of -0.06 ± 0.03? If the 0.03 is ± 1 SD, I would guess that there is not worthwhile confidence, and if not, this result is not supportable and should be deleted.

P20128 L13-L15 Similar question about significance, now for NPP.

P20128 L11: "1990 and 2009" should be "1990 to 2009"

P20128 L20-L30: If the NBP trend is being overestimated by the C-only models, then other trends (ocean sink, emissions . . .) must also be adjusted.

P20129 L5: The fact that trend(RH) $<$ trend(NPP) does not "explain" the increasing land sink trend at all; it is merely consistent with it. In fact the result cannot be any other way, by the definitions: if the definition of NBP is simplified to NBP = NPP $-$ RH, then trend(NBP) = trend(NPP) $-$ trend(RH).

P20129 L21: Does "of opposite sign" mean that CLM4CN and LPJ gave opposite trends for the fire flux? If so, how can anything meaningful be said?

P20131 L3: "one of the most important drivers": do you mean that it is the most important, or that there are other important drivers? If the latter, what are they?

P20131, sec 3.2.1: Given the great differences in drivers across biomes, especially light, temperature and precipitation, a whole-globe correlation like this does not mean much.

P20131 sec 3.2.2 and later: The important thing is not so much that the absolute flux for a region is so-and-so, and the absolute trend is such-and-such; rather it is the regional patterns, expressed (say) as relative contributions of regions to the global flux or trend. These need to be interpreted with regard to the area fractions as well: thus, the relative behaviours of flux density (flux/area) across regions also need to be considered, along with the area-integrated fluxes. See major issue 4.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

P20132 L21: To interpret these trend differences for Europe etc, information about areas is also needed. See previous comment.

P20134 L24: “there are some areas”: which areas? Can these be delimited with statistical confidence? Foreshadow a discussion explaining the spatial patterns of NPP trends.

P20136 L4: To interpret the 20% (of global NPP to southern land) we also need the area fraction.

P20136 L16-L22: Is this related to the large NPP response to precipitation? More interpretation and fewer in-text numbers are needed here (as elsewhere).

P20137 L13: “The response of RH to changes in precip is not obvious as this depends on initial soil moisture”: I’m surprised that soil moisture initialisation is an issue for the upper soil layers (the ones important for RH) on a 20-year time scale. Deep soil water changes may contribute to transpiration for deep-rooted systems, but that’s another issue. If there are initialisation problems here, then it adds to the need for a good description of DGVM initialisation in Sec 2.4.1.

P20138 L9: “which leads to uptake everywhere”: I read this as “leads to net CO_2 uptake everywhere”, contradicting L3-L5 above. I think the intent is that “uptake everywhere” refers only to the perturbation (anthropogenic) flux. Please rephrase.

P20138 L18-L22: This finding (that trends in ocean C uptake are not only small globally, but also regionally) is important and worth much more prominence. It takes away the possibility that the small global trend is the net result of large opposing trends in different regions (a situation quite different from the net C flux, where there are large opposing fluxes in different regions).

P20139 L1-L8: Also important, as it shows that the regionally small trends are the result of compensating CO_2 and climate influences.

P20139 L13-L18: “Some regions – other regions”: say which regions.

P20139 L23: Define RLS. More importantly, clarify the implication.

P20140 L14: Why jump to CO_2 fertilisation, and how is climate ruled out?

P20140 L28 and next few lines: This is mere anecdote. Temperate areas experience drought often, and presumably always have. It is unsound science to extrapolate in this way.

P20141 L6-L15: The need for better models is acute, and is indicated by the major problems with model-observation comparisons. See major issue 2.

P20143: Big problems here: see major issue 1.

P20147 L27: Not all C-N models show reduced land C sink: eg Wårlind (2013) PhD thesis.

4 References

Bacastow, R. B. and Keeling, C. D.: Models to predict future atmospheric CO_2 concentrations. In: Workshop on the Global Effects of Carbon Dioxide from Fossil Fuels, Elliott, W. P. and Machta, L. (Eds.), United States Department of Energy, Washington DC, 1979.

De Kauwe, M. G., Medlyn, B. E., Zaehle, S., Walker, A. P., Dietze, M. C., Hickler, T., Jain, A. K., Luo, Y., Parton, W. J., Prentice, C., Smith, B., Thornton, P. E., Wang, S., Wang, Y. P., Wårlind, D., Weng, E. S., Crous, K., Y., Ellsworth, D. S., Hanson, P. J., Seok-Kim, H., Warren, J. M., Oren, R., and Norby, R. J.: Forest water use and water use efficiency at elevated CO_2: a model-data intercomparison at two contrasting temperate forest FACE sites, Global Change Biology, 19, 1759-1779, 2013.

Frölicher, T. L., Joos, F., Raible, C. C., and Sarmiento, J. L.: Atmospheric CO_2 response to volcanic eruptions: the role of ENSO, season, and variability, Global Biogeochemical

Cycles, 27, 239-251, 2013.

Gloor, M., Sarmiento, J. L., and Gruber, N.: What can be learned about carbon cycle climate feedbacks from the CO_2 airborne fraction?, Atmospheric Chemistry and Physics, 10, 7739-7751, 2010.

Haverd, V., Raupach, M. R., Briggs, P. R., Canadell, J. G., Davis, S. J., Law, R. M., Meyer, C. P., Peters, G. P., Pickett-Heaps, C., and Sherman, B.: The Australian terrestrial carbon budget, Biogeosciences, 10, 851-869, Doi 10.5194/Bg-10-851-2013, 2013a.

Haverd, V., Raupach, M. R., Briggs, P. R., Canadell, J. G., Isaac, P., Pickett-Heaps, C., Roxburgh, S. H., van Gorsel, E., Rossel, R. A. V., and Wang, Z.: Multiple observation types reduce uncertainty in Australia's terrestrial carbon and water cycles, Biogeosciences, 10, 2011-2040, Doi 10.5194/Bg-10-2011-2013, 2013b.

Le Quéré, C., Raupach, M. R., Canadell, J. G., Marland, G., Bopp, L., Ciais, P., Conway, T. J., Doney, S. C., Feely, R. A., Foster, P., Friedlingstein, P., Gurney, K. R., Houghton, R. A., House, J. I., Huntingford, C., Levy, P. E., Lomas, M. R., Majkut, J., Metzl, N., Ometto, J., Peters, G. P., Prentice, I. C., Randerson, J. T., Running, S. W., Sarmiento, J. L., Schuster, U., Sitch, S., Takahashi, T., Viovy, N., van der Werf, G. R., and Woodward, F. I.: Trends in the sources and sinks of carbon dioxide, Nature Geoscience, 2, 831-836, 10.1038/NGEO689, 2009.

Raupach, M. R.: The exponential eigenmodes of the carbon-climate system, and their implications for ratios of responses to forcings, Earth System Dynamics, 4, 31-49, doi: 10.5194/esd-4-31-2013, 2013.

Raupach, M. R., Gloor, M., Sarmiento, J. L., Canadell, J. G., Frölicher, T. L., Gasser, T., Houghton, R. A., Le Quéré, C., and Trudinger, C. M.: The declining uptake rate of atmospheric CO_2 by land and ocean sinks, Biogeosciences Discussions, 10, 18407-18454, 2013.

Wårlind, D.: The role of carbon-nitrogen interactions for terrestrial ecosystem dynamics under global change: PhD Thesis Department of Physical Geography and Ecosystem Science, Lund University, Lund University, Sweden, 2013.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper