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

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Response to Reviewers Comments

Firstly we would like to thank all three reviewers for their detailed and constructive comments, which has helped us to improve our manuscript.

The first two reviewers are very supportive, and I feel we have addressed the issues from third reviewer, Mike Raupach. Broadly, the second reviewer (anonymous) requested a more rigorous comparison of our findings with the other RECCAP regional chapters. We now include a table for comparison and a paragraph in the main text.

Mike R. wanted a consistent analytical framework, and testing models against observations. We have revised the introduction paragraph and state that the manuscript does address the changes in the magnitude of the sink but does not discuss the efficiency of the sinks (and cite the Raupach et al., BGSD paper where the sink efficiency is discussed). The DGVMs have in fact been extensively evaluated in the Piao et al., GCB, 2013 paper. Although we cite it in our study, we now elaborate more in the current manuscript. Detailed responses to all 3 reviewers' comments are given below:

Reviewer C8268 (Andrew Friend) was broadly supportive of the manuscript. He also sent an annotated copy of the manuscript with suggested edits. We have taken on board the suggested editorial changes.

Reviewer C8318 (anonymous) wanted a more rigorous comparison of these findings with the other RECCAP studies (not just a brief mention in the introduction). This is a very fair comment. Reviewer comment:

"The paper is scientifically sound and analysis appropriate. However, the essential feature lacking in the paper is through comparison with previous studies. In particular, RECCAP has recently published a suite of papers in BG (which include many of the authors of this the paper) synthesising the regional carbon sources and sinks over the past several decades and these studies should form the baseline information to which these simulations should be compared to. Therefore, to make the paper complete and publishable a more rigorous comparison with the RECCAP studies is needed. It is not good enough to just cite them in the introduction. I want to see these studies used in the discussion as well."

We have included the following short discussion of the model results in the context of the individual chapters in the discussions section, and included a table summarizing the chapter results and contrast them with model only results from this study. Note, model results were used in the budget estimates in several of the regional chapters:

"In Table 3, DGVM results are compared with the RECCAP synthesis papers documenting carbon sources and sinks for individual regions. Note, DGVMs provided one source of evidence for some regional papers. Over Russia, DGVMs agree on a sink however underestimate its magnitude, likely related to soil respiration (unsurprising as many DGVMs have a limited representation of permafrost, active layer thickness) (Dolman et al., 2012). In South America, DGVMs agree with inventory-based estimates on a sink in natural forests (Gloor et al., 2012). DGVMs also agree with other data sources on the sign and magnitude of the natural land sink over Australia (Harverd et al., 2012). Over Europe DGVMs simulate a smaller mean land sink than the synthesis study suggests (Luyssaert et al., 2012). However, the regional synthesis was conducted over the shorter time period 2001-2005. For the Arctic, DGVMs tend to simulate a lower sink than regional process-based models (McGuire et al., 2012). However over the 1990-2006 period DGVMs are in line with observations and inversions on the magnitude and sign of the natural land sink, and DGVM results also suggest a sink trend in line with observations. DGVMs simulate a land sink over South Asia in agreement with inversions; however there were limited data to compare trends from DGVMs and other products (Patra et al., 2013). For East Asia DGVM results agree remarkably well with remote-sensing model – data fusion and inverse models on the magnitude of the land sink over the period, 1990-2009. Finally, for Africa, DGVMs are broadly consistent with inventory and flux-based estimates, in simulating a land sink over Africa, albeit of lower magnitude (Valentini et al., 2014)."

Reviewer comment: "In this comparison, the authors should be able to provide insight into: 1. how important are fire and land use changes on the carbon sink over the last 2 decades? The atmospheric inversions provide measurements of the net fluxes to which the simulated ocean and land fluxes can be compared to explore this issue. The discussion makes some general statements on the importance of better simulating fire and land use change but at least from the agreement of the estimated trends with the global inversion perhaps these processes are insignificant - explore and discuss this issue in more detail."

The majority of DGVMs do not simulate wildfire and land-use changes are not included in these simulations; the aim of this paper is to explicitly evaluate the impact of climate change and variability and changing atmospheric CO2 content on regional biogeochemistry. The role of land-use change will be the focus of a follow-up study. We have therefore removed the statements in the discussion and conclusions about fire modelling.

Reviewer comment: "2. In the ocean, the comparison to the RECCAP should address whether using more sophisticated ocean carbon models (e.g. more complex ocean biology) changes the results from what is shown in the RECCAP studies. Further comparisons in the ocean can be should be made with recent observational synthesis such as the following references to help elucidate what key ocean carbon cycle processes are missing in the models. Lenton et al 2012 [Lenton, A., Metzl, N., Takahashi, T., Kuchinke, M., Matear, R. J., Roy, T., Sutherland, S. C., Sweeney, C. and Tilbrook, B.: The observed evolution of oceanic pCO2 and its drivers over the last two decades, Global Biogeochem Cy, 26(2), doi:10.1029/2011GB004095, 2012.] Takahashi, T., et al. (2009), Climatological mean and decadal change in surface ocean pCO2, and net sea-air CO2 flux over the global oceans, Deep Sea Res., Part II, 56, 554–577, doi:10.1016/j.dsr2.2008.12.009."

We have now included the following paragraph to address this comment:

All the models have been tuned to reproduce data synthesis on ocean surface pCO2 (Pfeil et al., 2013, Takahashi et al., 2009) and deep ocean (Key et al., 2004) reasonably well. Specific systematic data assimilation procedures, however, have not been applied. On decadal time scales, the ocean CO2 flux feedback to climate change (change in hydrography and circulation) and rising ambient CO2 (change in CO2 buffering) reacts only slowly on the global average due to the long time scales of oceanic motion and marine CO2 equilibration with the atmosphere. Changes in biogeochemical and ecosystem processes, such as locally varying gas exchange velocities, phytoplankton blooms, and associated particle flux pulses can lead to regional interannual variations in air-sea CO2 fluxes, but partially may cancel for averages over larger regions. Longer term trends due to a gradual slowing down of meridional overturning circulation due to a strengthening of density stratification, redissolution of CaCO3 sediment from the seafloor for fossil fuel neutralization, and potential changes of biogenic particle fluxes due to carbon overconsumption and changing ballasting can potentially hardly be backed up by observations over a two decade time frame (cf. Keller et al., 2014). Whether more complex models will render better results, will depend on how well the additional free parameters in more complex biogeochemical models can be constrained by measurements. So far, more complex - and hence potentially more realistic models – do not necessarily give better results than the present NPZD type models as applied here (Le Quéré et al., 2005, Kriest et al., 2010).

Reviewer comment: "3. The paper discusses the impact that N-limitation could have on the land carbon uptake uptake but how about some comments on P-limitation? I have listed a few papers that have considered the combined impact of nutrient limitation on the land carbon uptake. P-limitation should be most important in tropical forests and savannahs - do you see any indications that P limitation could be impacting the land carbon sink trend in these regions? What does it say about P limitation? Edwards E, McCaffery S, Evans J: Phosphorus status determines biomass response to elevated CO2 in a legume: C- 4 grass community. Glob Change Biol 2005, 11:1968-1981. Wang, Y. P., Law, R. M. and Pak, B.: A global model of carbon, nitrogen and phosphorus cycles for the terrestrial biosphere, Biogeosciences, 7, 2261–2282, 2010. Zhang, Q., Wang, Y. P., Matear, R. J., Pitman, A. J. and Dai, Y. J.: Nitrogen and phosphorous limitations significantly reduce future allowable CO2 emissions, Geophys. Res. Lett, doi:10.1002/2013GL058352, 2014."

None of the DGVMs in this analysis include a P-cycle. However the authors agree that P-cycle is potentially important for the magnitude of the present and future land sink especially in tropical ecosystems. This uncertainty has been now mentioned in the discussion section.

Reviewer comment: "In the abstract and throughout the manuscript please present the fluxes in a more consistent manner. A flux is often referred to as a sink but then given as a negative number which implies it is a source."

This is the chosen sign convention of the entire special issue, therefore should not be changed. We have checked for consistency.

Minor Details "page 11 - used S_O1 and S_O2 before they were defined. " Addressed

"page 12 - NBP is the same as net atmosphere to land co2 flux - do you really need another variable for this process? delete it and refer to net atmosphere to land co2 flux" Addressed (deleted NBP nomenclature)

"page15-16 - add comparison to the RECCAP estimates of the net flux and trends" We have compiled data from the individual RECCAP chapters and compare against the model estimates alone in a table.

"page 18 - give the estimate from Wanninkhof et al 2013 line 3 to 7 - make comments on the key processes before presenting the corresponding simulations - move these statements until after the simulations are presented."

Moved the statements on key progresses until after the simulation results are presented. Did not include the Wanninkhof estimate in the results section; the current results are compared with Wanninkhof in the discussion section.

"Discussion -cite the recent RECCAP analysis which include regional trend estimates. See earlier comment on results against model estimates"

"pg 28 line 20-25 - comment on missing P limitation in the models" P-Cycle is now mentioned at the end of the discussion paragraph 4.3.3. Model structure.

"pg 30. There are many general statements of the limitations of the DVGM, but from the limited quantitative assessment I see a general agreement with observations - how can you conclude the DVGMs are lacking? What do the regional reccap studies suggest? Do they provide additional evidence for need to include additional processes to improve in these models?"

Yes this is a good point. Several recent studies have evaluated DGVMs (Piao et al., GCB. 2013) for specific metrics and have drawn conclusions. We have deleted the paragraph on ‘modeling ecosystem structure and function in water stressed environments’, also identified by Reviewer A. Friend as not having strong justification from the current model results and analyses.

"pg 31 line 1 - I think the figure shows very similar global trends in the ocean uptake with and without climate variability and change. line 6 - give your estimated number for the trend so it can be compared to -0.04 PgC/yËĘ2. line 7-13 - include the global estimate with uncertainties to help justify your statements."

A new Figure 4, ocean fluxes, is included with better y-axis scaling. Global estimate included in the text.

"Pg 32 What do the RECCAP studies conclude for the regional changes? What new information has this study provided and how do the simulated regional trends compare to ocean observations?" See earlier comment on elaborated discussion on the oceans. A novelty of this paper is that it brings together both land and ocean carbon cycle estimates in one study.

"Pg 33, line 13 - another study you could cite. This recent paper explores the impact of climate change in an eddy resolving simulation and shows the opposite impact of climate change on ocean productivity with eddy resolution Matear, R. J., Chamberlain, M. A., Sun, C. and Feng, M.: Climate change projection of the Tasman Sea from an Eddy-resolving Ocean Model, J Geophys Res-Oceans, 118(6), 2961–2976, doi:10.1002/jgrc.20202, 2013." Added the citation.

"Pg 35 - from this study and the RECCAP studies can you comment on the role biological complexity plays in the ocean carbon uptake trends over the last 2 decades?" See earlier comment to #2. New paragraph included.

"Pg 36, line 18-20 - not convinced that the lack of wildfires and other land disturbances

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was demonstrated as a key limitation of the model simulations. What observations are you using that clearly demonstrate this point? Perhaps, I just do not like this paragraph since I feel for the simulated trends over the last 2 decades that it did not emerge from your analysis that simplifications in the models were causing inconsistent behavior with the limited observations. If you are either trying to extrapolate these results to the future projections or if you had better observations then some of these issues may be relevant, otherwise this paragraph is not justified."

The sentence has been removed. This also links to the earlier comment by this reviewer and reviewer A. Friend on the model limitation section in the discussions.

"references - missing some of the RECCAP papers cited on page 5." Thanks. Lenton et al., 2013, and Ciais et al. 2010 now included in the reference list. "table 1 - while you cite Wanninkhof et al., 2013 for easier reading you could include a summary table for the ocean models too." Addressed adding new table (Table 2).

"table 2 - a negative sink is a source - change word or sign to be consistent. it would be helpful if you made the significant trends in bold font, Label figures with a,b,c,d." Addressed (removed sink nomenclature). Figures labeled.

"Figure 2 - there are 6 panels but only 4 are described" Addressed.

"Figure 3 - no global trends in this figure look significant - not consistent with the information given in the text." Cancellation when average making signal to noise ratio larger (i.e. the trend). Also the grey bands represent the model spread not the mean IAV.

"Figure 4 - I see no difference between the red and black trend lines - seems to disagree with the comments in the text that with climate variability and change there is a significant change in the ocean sink trend." There is a difference, but it is a small change.

"Figure 5 - do you need the first column since the information is shown again in the second column" The middle figure now only shows the model agreement, therefore there is no longer duplication.

"Figure 6 - negative sink is a source - panel 3 has the wrong sign to be called a sink." Again we use the atmospheric sign convention, as explained in the main text. Nevertheless, we replace the word 'sink' with trend in land to atmosphere net CO2 flux.

"Figure 7- state how the onset and offset day was calculated. how did you deal with the different hemispheres?" The description of how leaf onset and offset is calculated can be found in section 2.5.1. The graphic is for the northern hemisphere only.

"Figure 10 - Figure 3 shows a similar trend for os1 and 2 but this plot show a clear difference (I assume W is global ocean?). The NP, EP and NAT are the 3 regions where climate variability changes the sign of the trend is there any observational data to support this result?" This result is consistent with the study by Le Quére et al. (2010). They document that observational evidence indicates that the simulations with climate variability and changing CO2 are more realistic than with constant climate, though corroboration for the Pacific Ocean is not firm.

Reviewer C8704 (Mike Raupach)

Reviewer comment, "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).

The regional findings can be summarized briefly as (1) the land CO2 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 CO2 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."

Points #1 and #2 are mentioned in the abstract. #3 relates to the efficiency of the global carbon sinks. However, the focus of this manuscript is on recent trends and drivers of regional sources and sinks of carbon dioxide. Discussion on the global sink efficiency has been published elsewhere.

Reviewer comment, "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: 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 trends, related to the corresponding global quantities? How are trends in responses (e.g. regional sinks) related to trends in drivers (e.g. atmospheric CO2)? These questions call for a framework that considers not only absolute quantities (fluxes or trends in PgC yôĂĂĂ2) 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 CO2 sinks are fundamentally driven by rising atmospheric CO2, modulated by geography and climate, one key metric is the ratio of sink strength to excess CO2, or the sink flux per unit excess CO2 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 CO2 (the primary driver): if the efficiency is decreasing, then the sinks are growing more slowly than excess CO2, 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 CO2 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 ôĂĂĂ0:8% 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 CO2 (CO2 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 CO2 emissions (specifically to departure of forcing from exponential); (4) volcanic effects on CO2 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 CO2 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 CO2 effects, 20% is from (2) carbon-climate interactions, 35% is from (3) sink responses to the nonexponential 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 CO2 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

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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 generalization 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."

Summary point#3 is the contentious issue, but that is what the c-cycle models are finding over the global scale for the two-decade period. However, in this paper we want to focus on the regional trends, processes and their drivers. The airbourne fraction discussion is published widely elsewhere (e.g. see discussion in IPCC AR5). Therefore, we have deleted the following sentences in the introduction: "A particularly striking observation is that the combined sinks in the ocean and land have increased approximately in line with the increase in fossil fuel emissions in recent decades (Keeling et al., 1995; Canadell et al., 2007; Raupach et al., 2008; Sarmiento et al., 2010; Gloor et al., 2010; Ballantyne et al., 2012). Thus, for the past two decades alone when fossil fuel emissions increased from 6.2 $PgCyr{-1}$ in 1990 to 9.1 $PgCyr{-1}$ in 2010 (Andres et al., 2012), the combined sinks by land and ocean must have increased by -1.5 $PgCyr{-1}$ or at a mean rate -0.075 $PgCyr{-2}$ (we adopt here the atmospheric perspective with regard to the sign of the fluxes, i.e., negative numbers indicate a sink for atmospheric CO_2, and a negative trend indicates an increasing sink or a decreasing source)."

This has been replaced with,

'To balance the global carbon budget, the combined sinks by land and ocean must have increased over recent decades (Keeling et al., 1995, Canadell et al., 2007, Raupach et al., 2008, Sarmiento et al., 2010, Gloor et al., 2010, Ballantyne et al., 2012).' Furthermore later in the introduction we state, ' This study addresses the changes in the magnitude of the global carbon sink but does not discuss the efficiency of the sinks, which is widely discussed elsewhere (Raupach et al., 2013, Gloor et al. 2010, IPCC

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2014).'

Reviewer comment, "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."

Extensive model evaluation of DGVMs can be found in Piao et al., GCB 2013 and Peng et al., 2014. Although we cite Piao et al GCB, we include several sentences to summarize the evaluation of the models in Piao et al and other evaluation papers. The previous model benchmarking section has been revamped accordingly:

'4.3.1 Model Evaluation and Benchmarking Piao et al., 2013 evaluated the DGVM model results for their response to climate variability and to CO2 trends, and the seasonal cycle of CO2 fluxes were benchmarked in Peng et al., 2014. Piao et al., 2013 found DGVMs to simulate higher mean and interannual variations (IAV) in gross primary production than a data driven model (Jung et. al., 2013), particularly in the tropics, however, this is the region where the data-driven model is most uncertain. DGVMs were able to capture the IAV in RLS, although the simulated land – atmosphere net CO2 flux appears too sensitive to variations in precipitation in tropical forests and sa-

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vannas. However, Poulter et al., 2014 found an increase in the sensitivity of the net flux to precipitation over the last three decades across continental Australia. Piao et al., 2013 found that the simulated net CO2 flux was more sensitive than productivity to temperature variations. When compared to ecosystem warming experiments the DGVMs tend to underpredict the response of NPP to temperature at temperate sites. DGVMs simulated an ensemble mean NPP enhancement comparable to FACE experiment observations (Piao et al., 2013). However, modelling ecosystem function in water stressed environments and changes in plant water use with elevated CO2 remain a challenge for DGVMs (Morales et al., 2005, Keenan et al. 2009, De Kauwe et al., 2013).

There is a critical need for comprehensive model benchmarking, as a first step to attempt to reduce model uncertainty. Several prototype carbon cycle benchmarking schemes have been developed (Randerson et al., 2009, Cadule et al., 2010). A more in depth evaluation & community benchmarking set needs to be agreed and implemented, which also evaluates models for their implicit land response timescales (especially relevant in the discussion on future tipping elements and non-linear future responses) and for the simulated carbon, water and nutrient cycles. New emerging frameworks now exist (Blyth et al. 2011, Abramowitz, 2012, Luo et al., 2012, Dalmonech & Zaehle, 2013, Harverd et al., 2013). One example within RECAPP is a multiple constraint approach applied to reduce uncertainty in land carbon and water cycles over Australia (Haverd et al., 2013).'

Reviewer comment, "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 CO2 mass balance: [atmospheric CO2 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 CO2 accumulation) = trend(FF emission) + trend(LUC

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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 CO2 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 RGR(X) = trend(ln jXj) ' trend(X)/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 CO2 or CO2 emissions). Of particular importance is the RGR for sink efficiency."

The global analyses (RGR and sink efficiency) are not the main focus of the paper, which is already long.

Reviewer coment, "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."

There are several studies on the carbon cycle response to Pinatubo (Lucht et al., Peylin et al., Mercado et al., Froehlicher et al); we add a short paragraph on this point.

'The Pinatubo eruption in 1991, at the start of the study period, had a major influence on many carbon cycle processes, leading to an enhanced land sink over the period

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1991-1993. This has been attributed to a combination of cooling induced reductions in high latitude respiration and enhanced productivity associated with changes in diffuse radiation (Peylin et al., 1995, Jones and Cox, 2011, Lucht et al., 2001, Mercado et al., 2009, Frölicher et al., 2013). The direct effect of aerosols on climate drivers are implicitly included in this study (i.e. responses to high latitude cooling, tropical drying, reduced net incoming solar radiation), however diffuse radiation effects are not included.'

Reviewer comment, "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 PgC yôĂĂĂ2. 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 _ x, Y _ y and Z _ 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 _ x (units); for B it was Y _ y, and for C, Z _ 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?"

The land section has been revised to address this stylist point. In particular the regional land section has been reduced in length with less numbers to enhance readability. A short summary paragraph is included near the end of section 3.2.1, 'In summary, the global increasing trend in land carbon sink is about two-thirds due to the tropics, and one-third due to the southern land region, with zero contribution from northern land. This partitioning in trend is quite different from the mean carbon sink fluxes themselves, which is more like (43:41:16) for (northern, tropical, southern).'

"3 Minor issues P20117 L6: Greenness is a function green leaf biomass and hence measures a stock, not a flux." Modified sentence

"P20118 L15: “world”, not “World”" Correction made

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

"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."

Yes models assume a fixed present-day land use throughout the simulation period. This is now explicitly stated in the text. The following sentence is added in section 2.5.1. Land, 'Note the RLS depends on a LUC model of emissions (the one of Houghton). Strictly speaking, comparison of model land to atmosphere net CO2 flux with RLS is therefore inconsistent because these models treat areas affected by LUC as pristine ecosystems, and these areas are generally associated with a high NBP (C

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sinks). Simulated net C flux from S2 is therefore likely to overestimate the RLS sink, by construction. '

"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]."

The following text has been added: 'Model spin up consisted of recycling climate mean and variability from the early decades of the 20th century (1901-1920) with 1860 atmospheric CO2 concentration of 287.14 ppm. The land models were then forced over the 1861-1900 transient simulation using varying CO2 and continued recycling of climate as in the spin-up. '

"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 steadystate climatology, or does it retain long-term climate change (warming etc)? If the latter, how much variability is rejected – (interannual, decadal, etc.)?"

The climatological forcing is done with a “normal year”, i.e., a typical mean synoptically from day to day changing forcing (but no ENSO years or so). The annual cycle for this normal year is then repeated (no year to year change).

"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." Addressed.

"P20123 L23: “From _ 1950 onward” should be the start of a new paragraph" Corrected

"P20124 L5: Seems to be a missing “CO2” after “preindustrial”" Corrected

"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."

The term NBP has been removed to avoid confusion with the terminology land to atmosphere net CO2 flux. Also it is stated that not all DGVM represent all the processes; a list given in table 1. In this present study the largest gross fluxes are evaluated, as all DGVMs are able to supply these data.

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

"P20125 L11: See comment on P20121 L11" Addressed (see earlier comment)

"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."

Now reads "Climate variability and change"

"P20127 L21: For all _ 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."

One standard deviation. Sentence modified appropriately.

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

"P20128 L5-L8: Do the two trends in the N-enabled land C sink (-0:02 and -0:05) 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

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would guess that there is not worthwhile confidence, and if not, this result is not supportable and should be deleted."

Sentence modified to account for reviewer's comment: 'The two DGVMs with a fully coupled carbon and nitrogen cycle (CN) also simulate an increase in the land sink, at -0.02 (P=0.6) for OCN and -0.05 PgC yr-2 (P=0.06) for CLM4CN.'

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

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

"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." Yes this is true. There may however be offset from the ecosystem response to climate change.

"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)." Yes. Sentence modified accordingly.

"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?" Indeed, sentence deleted.

"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?" Sentence modified, reads is an important driver.

"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." Sentence deleted.

"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

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

We have now expanded three tables in the supplementary material (Tables A4, A5, A6), to include fluxes and trends in area-based units. Reference to results in flux/area basis is now given in the main text.

"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." See figure

"P20136 L4: To interpret the 20% (of global NPP to southern land) we also need the area fraction." See earlier comment. Tables A4, A5 and A6 now include area-based units.

"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)."

Yes, as originally stated in the text, now slightly modified to reduce space, 'Southern Africa is the only southern sub-region with a significant trend in NPP of 0.041 $\pm$ 0.018 PgC yr-2 (P=0.00) (Table A5), due to a positive response of plant production to both CO2 and is likely in response to increases in precipitation over the last two decades (Table A7, Figure 5).'

"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

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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."

Added the initialization description in response to earlier comment. Models vary in their representation of soil moisture dynamics, in terms of numbers of soil layers, percolation / drainage, water holding capacities etc. It is possible for a gradual decline in top layer soil moisture over multiple years, which will affect respiration rates. It is also possible for different models to simulate different top-soil moisture status with the same driving climate.

"P20138 L9: “which leads to uptake everywhere”: I read this as “leads to net CO2 uptake everywhere”, contradicting L3-L5 above. I think the intent is that “uptake everywhere” refers only to the perturbation (anthropogenic) flux. Please rephrase."

Rephrased, 'Superimposed on this natural CO2 flux pattern is the uptake of anthropogenic CO2, which is taken up everywhere, but with substantial regional variation.'

"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)." No change necessary.

"P20139 L1-L8: Also important, as it shows that the regionally small trends are the result of compensating CO2 and climate influences." No change necessary.

"P20139 L13-L18: “Some regions – other regions”: say which regions." We delete the TWO SENTENCES (...In some Regions... In other regions...) AND REPLACE with: 'Attribution of regional trends to specific processes or changes in specific state variables in the different models is work in progress and difficult to achieve with high confidence as yet. This is due to the antagonistic effect of ocean warming on CO2 solubility and on dissociation of carbonic acid into bicarbonate and carbonate, as well

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as to the complex changes in ocean circulation and mixing, which themselves influence the biological carbon pumps of the ocean.'

"P20139 L23: Define RLS. More importantly, clarify the implication." Already defined on page 20125, line 7.

"P20140 L14: Why jump to CO2 fertilisation, and how is climate ruled out?" As stated on P20134 NPP trend in S1 almost five times as large as the climate induced trend

"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."

Modified the paragraph. Added evidence from Zscheischler et al., 2014, 'Zscheischler et al., 2014 suggest that negative GPP extremes dominate GPP interannual variability during the period 1982-2011; these extremes are evident particularly over temperate latitudes'

"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." See responses to issue 2.

"P20143: Big problems here: see major issue 1." See responses to issue 1.

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

This is p20145. The current formulation already uses words like "generally" and "tend", with reference C-N model results."

Interactive comment on Biogeosciences Discuss., 10, 20113, 2013.