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Interactive comment on "Recent changes in the global and regional carbon cycle: analysis of first-order diagnostics" *by* P. J. Rayner et. al.

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Interactive Comment

Response to Referees' Comments

Peter Rayner

20 October 2014

Before addressing the referees' comments we need to point out an error in some figures and data. A programming error meant that the $\delta^{13}CO_2$ data had not been properly included. This most seriously affects Table 2. To quantify the effect we replicate the structure of Table 2 for the regional fluxes but showing the differences between the corrected and erroneous versions. We use the corrected versions of the uncertainties although these hardly change from the original version.

$\delta^{13}CO_2$ is modelled by assuming weak prior knowledge on the isotopic disequilibrium flux and its first derivative (Rayner et al., 1999; Rayner, 2001). This means that differences in the long-term first and second derivatives of $\delta^{13}CO_2$ will be absorbed by these variables. There is also relatively little $\delta^{13}CO_2$ data available from the one network we use. Thus it is unsurprising that the differences demonstrated in the table are slight, certainly smaller than the uncertainties. Thus we believe the error has no material impact on the results of the paper.

We thank the three anonymous referees for their comments which have allowed us to clarify various points in the paper. There are two points made by more than one referee. We address these first then deal point by point with the comments of each referee. We place referees' comments in typewriter font and our responses in Roman.

Table 1. Errors in land and ocean β values from the inversion for northern extratropics, tropics and southern extratropics for the periods 1992–2012 and 2002–2012.

Flux	1992–2012		2002–2012	
	β (yr^{-1}	uncertainty (yr^{-1}	β (yr^{-1})	uncertainty (yr^{-1})
northern land	-0.000	0.004	0.005	0.012
northern ocean	0.001	0.002	-0.003	0.005
tropical land	-0.001	0.008	0.002	0.021
tropical ocean	0.001	0.003	-0.001	0.006
southern land	-0.001	0.006	0.003	0.016
southern ocean	0.001	0.003	-0.004	0.006

Common Points

Here we paraphrase the referees' comments and refer back to them in the detailed responses.

`Expand on the comparison with gloor et al., (2010)`

This is a good point. the purposes of the two papers are a little different. We have expanded on the difference between the papers in the introduction and model description. We also removed the comment in the abstract comparing the two simple models since this is, indeed, covered by Gloor et al. (2010). We have also noted the conclusion of a possible increase in response, a different result from Gloor et al. (2010).

`the assumption of independent errors in the Global Carbon Project data is unjustified and threatens the paper's conclusions.`

We thank the reviewers for pointing this out. Because our analysis depends on trends in various terms the most likely type of error (positive temporal correlation) would actually increase the significance of the results. We now point this out in an extra paragraph in section 2.

The only case where this is not true is the calculation of the error in α in Section 2. We now bracket this uncertainty by assuming either independence (the default) or perfect correlation so that the assumed 5% error in the annual fossil fuel flux propagates directly into the sum. We also describe this case in Section 2.

Comments from Anonymous Referee 1

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This manuscript presents an interesting study into decadal
trends in the strength of carbon cycle feedbacks, extending the
approach applied in previous publications by a decomposition of
global tendencies into regional and seasonal components.  The
methodology and its application to inversions and ecosystem
models is interesting as it allows a different way of looking
at existing simulations.  As pointed out for the global
analysis, for which longer data records are available,
shorter-term results may not point at robust tendencies.  The
question remains if this lesson of caution doesn't also apply
to the inverse model and vegetation model results, which are
evaluated over shorter periods.  Whether or not robust, the
ideas presented in this work are definitely worth publishing.
Below are a list of corrections and suggestions that will
hopefully facilitate the reading, and a few scientific issues
that require some further attention.
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General Comments

Although the focus of the paper is on trends in the carbon cycle response to its forcing, the mean sensitivities that are derived deserve some attention also. The absence of a weakening in the oceanic response is quite significant in light of a few others studies, as is discussed. However, the mean values that are derived for the sensitivity of the ocean seem rather small, which calls for an explanation also. At least an effort should have been made to compare the numbers with Gloor et al, 2010.

We have added a paragraph to the discussion making this comparison. We point out that the comparison is difficult since Gloor et al. (2010) do not include the constant term in their regression (compare their Equation 2 with our Equation 5). Both approaches are perfectly valid for the different aims of the two papers.

SPECIFIC COMMENTS

Abstract, line 11: 'also' suggests a similarity with the previously sentence, which is not the case.

"Also" deleted.

Footnote 1: But if policy decisions change the long-term mean flux (due to some new infrastructure becoming functional) then the corresponding source may be constant, rather than constantly growing. This is different for a policy decision that causes the infrastructure to evolve in time. In that case the flux may integrate a single political decision, but

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otherwise not.

This seems a misreading of the footnote. It says that policy choices will add or subtract anthropogenic sources and these sources will probably last a long time. Put mathematically, each decision on a new infrastructure is a pulse in source space, and the lifetime of that pulse is often long. The point is not critical for this paper though so we deleted the footnote.

Eq.7: If 'M' and 'q' both represent burdens of carbon or CO2 in the atmosphere, then why not use the same parameter? They are not defined exactly the same, but this seems to make the equation unnecessarily complicated.

An excellent idea. We have changed this throughout and changed some text around Eqs. 5 and 6 to reflect this.

Page 9926, line 8: J=q i.o. 1/q

J here is meant to be a column matrix so we think the current form is correct. If it is being mistaken for a fraction we should discuss with copy editors how to clarify this within the house style.

Page 9926: Please mention briefly what dM/dt is based on.

We do not understand why this request should be made for one term in the budget (probably the best known) and not the others. We have added a paragraph in Section 3 summarizing the data sources.

Page 9927: How realistic is it to assume independence of annual estimates of F_{anthro}? The 1% seems quite optimistic. I wonder if it is supported by the size of the fit residuals.

See response to common points above.

Page 9927, line 5-10: Are these results shown somewhere?

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We have added an extra figure to show them explicitly.

Page 9927: 'The large uncertainty in beta' i.o. 'The large beta value'

No, we really are referring to the value here not its uncertainty.

Page 9927: 'the mean residual of the fit' i.o. 'the residuals of the fit'

Corrected.

Page 9928, line 2: What is the meaning of 'disjoint' in this context? That the corre- sponding processes are independent? This doesn't necessarily hold for a seasonal decomposition.

We mean that fluxes can be decomposed into a sum, we have made this explicit.

Page 9928, line 1-10: It is not directly clear which regression problem you solve in this case (in terms of J and y). I suppose you start now from equation 6 where $y = F_{\text{ocean}}$ for solving beta-ocean, and $y = F_{\text{land}}$ for solving beta-land?

Correct. We have made this explicit now.

Page 9928: How do you get 6 periods of 11 years for the period 1960 to 2010?

We calculate β for 11-year periods starting in every possible year of the study period. We now explain this explicitly on the previous page where the technique is first used for the global response.

Page 9929, line 5-10: Why is this best compared without the fossil component? Any difference between the GCP and CCAM fossil fuel prior would be mapped to the non- fossil component.

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The main requirement is that the model reproduces the observed
trend in CO2, right?
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No, testing that the model reproduces the overall trend in CO_2 only tests that our inversion scheme conserves mass (not a trivial test but not one we should inflict on the reader). That the components we're interested in follow the GCP after the inversion has made adjustments to the fossil and dealt with the interannually invariant prior is a tougher test. We note that reviewer 3 didn't think it was nearly tough enough.

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Page 9930, line 13: 'aliased into the calculated beta' io
'aliased into errors in calculated beta'
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Corrected.

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Page 9932, line 25: What is the reason for using a different
region definition for the inversion and the biosphere model?
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The main reason is the calculation of posterior uncertainties for fluxes. These form an input into Eq. 9 where we calculate the uncertainty on our diagnostics. The posterior flux uncertainties can be calculated for groups of regions but it is extremely difficult to map these back onto precise geographic boundaries. Given the large and systematic differences between inversion and terrestrial model results it is hard to imagine the small differences in regions playing much of a role.

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Page 9932, line 25 ...: Earlier it was mentioned that LUC
drives beta in the tropics. Then to properly interpret the
results of LPJ it is necessary to know if its LUC in recent
years resembles that of GCP.
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There are a couple of meanings of the word "drive" in play here. There is the physical sense in which land uptake is partly a response to LUC. Hopefully those processes are mirrored in the terrestrial biosphere models we use. Attribution of uptake to LUC is difficult, usually requiring parallel model runs with and without LUC. In our case, LUC

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is one of many forcings not dependent on concentration but which might masquerade as a first-order process. We mention other such forcings in the second paragraph of the discussion and have added LUC to the list.

For the inversion results, LUC has a more direct effect. Inversions solve for the net fux, so to produce the uptake flux compatible with the biosphere models we must subtract the LUC contribution. Thus part of the structure of the uptake estimates from the inversion comes from the GCP estimate of LUC. In this sense it is not necessary that GCP and (for example) LPJ LUC estimates are the same. The best way to clarify this point is to add LUC to the list of forcings as already described.

Page 9933, line 14: Since part of the paper deals with CCAM it would be better to specify 'model' here as an ecosystem model.

Done.

TECHNICAL CORRECTIONS

Abstract, line 11: 'Terrestrial models' i.o. 'terrestrial models'

Corrected.

Page 9926, line 'This' io 'this'

Corrected.

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Comments from Referee 2

General comments

As this work relies on an analysis framework developed by Gloor et al. [2010], a clearer distinction in the abstract and introduction between the two studies would help highlight the novel contributions of the current study.

See responses to common points.

Additionally, some discussion of how the Global responses results (Section 3) compare to the previous study may be useful

See response to general comments from Referee 1.

Regarding the amplitude of the residuals with time. A figure showing these values may be useful.

Added in response to request from Referee 1.

Detailed Comments

Abstract, lines 3-6, This text seems to be stating a conclusion already found by Gloor et al. [2010]. The authors should try to make the distinction more clear.

We have removed the comment.

Page 9921, lines 27-28, "but for different purposes." Clearly state different purposes.

We have added a summary point here and note that the use of the diagnostic is ex-

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plained in more detail two paragraphs later.

Page 9926, line 9, How does that constant term manifest itself in the above equations?

We have added a reference to Eq. 5 where this is demonstrated.

Page 9926, line 22, How does this 0.95 Pg C yrËĘ−1 value get used in the analysis? Is it added to R?

In fact we use it in $\mathbf{R}$. We have made this explicit in the relevant sentence. This also led us to note an error in Eq. 10. It was missing a power -1. We have corrected this.

Page 9927, line 4, I understand why mathematically assuming independence of the annual uncertainties allows for a cleaner and computationally cheaper solution but would inflating this value make sense because this assumption is likely over optimistic, e.g. errors are likely correlated because accounting methods and hence errors from year to year are similar.

See response to Referee 1.

Page 9927, line 7-9, What does the increase in the amplitudes of the residuals imply? Could a plot be useful here?

We have added a plot and the point is elaborated on P9934.

Page 9934, line 5-8, It seems this method would also be useful in comparing “bottom- up” and “top-down” methods to estimate CO2 flux. While intercomparisons would also prove to be useful within modeling arenas, the difference in the regional responses between the inversions and terrestrial models shown here again highlight the contrast between methods to estimate carbon exchange.

Indeed, we note this in the last sentence in the discussions.

Technical Comments

Abstract, line 11, capitalize "t" in "Terrestrial"

Done.

Page 9922, line 5, insert "the" after "since the early 2000s,"

Done.

Page 9926, line 9, capitalize "t" in "This is. . ."

Done.

Comments from Referee 3

Overall comments

Rayner et al. are revisiting an analysis done by Gloor et al. (2010) which looks at trends in the airborne fraction of anthropogenic emissions. Changing trends in this simple model might imply that natural sinks (and sources) are not responding in a linear way to the exponential rise in CO2. Gloor et al. (2010) were mainly concerned with the global changes in anthropogenic fluxes and changes in CO2 mole fraction where they felt the errors (uncertainties) in the temporal change in mole fraction and fossil fuel flux were small. They

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suggested that land use change adds a complexity because it has a much larger uncertainty. The land use change not only adds uncertainty to the calculation but it also prolongs the spin up time needed to get meaningful results from this simple model construct.

We thank the referee for prompting another reread of Gloor et al. (2010); it is a very fine paper. The reread, though, revealed even less in common between the papers than I remembered. Gloor et al. (2010) is predominantly concerned with findings about the airborne fraction of some preceding studies. They use a version of the β model to analyze the airborne fraction. There is little analysis of variations in β itself and no decomposition by space and time. We did have one clear overlap with Gloor et al. (2010), a comment in the abstract on drivers in changes of α. We have removed this.

Rayner et al. takes the Gloor et al (2010) study one step further by looking at both seasonal and regional trends in airborne fraction where they note, in particular, that the terrestrial uptake in the terrestrial northern hemisphere summer has been much higher in the last decade. Rayner also suggests in this paper that this "first-order" model approach is potentially useful for evaluating models' response to climate change which begs the question why build the model in the first place.

Overall, the idea that the uptake in the terrestrial northern hemisphere summer is in- creasing is fascinating. However, this paper does little to bring the reader up to speed so that they might understand why this might be a legitimate approach or what the pit- falls of this analysis might be. Simply taking the time to describe uncertainties carefully described by Gloor et al. (2010) or the details of the Rayner et al.

(2008) inversion or the "data" from Le Quéré et al. (2013) would be very helpful. In the case of the Gloor et al. (2010) analysis it would also be helpful to not only describe the uncertainties that are so carefully analyzed in Gloor et al (2010) but also describe what this paper has done differently. Many places throughout the text need more clarification to help the reader truly evaluate the merits of this simplifying approach to understand the high uncertainties of the regional and seasonal analysis. In its present form, I cannot recommend this paper for publication because it does not adequately describe the problem, technique used or the results in clear and concise way.

The referee raises a common but difficult point. How much background from previous work is necessary to evaluate a new paper and should this background be treated by referencing or in precis? The task is to provide the reader what they need as efficiently as possible. One can't decide this balance as a generality and equally it is hard to respond to such a general critique. The referee does state a couple of points in their specific comments where enlargement would help and we have responded to these.

Specific comments

9920 line 7 - "first order model" first introduced here but needs to be defined more clearly. From here the term is used sometime but not always. It would be helpful if this was more consistent.

An excellent suggestion, we have changed most occurrences of "first-order" to "linear" and used the name "β-model" to refer to it.

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9920 - line 9 - What is meant by "their"

We have changed this to "flux".

9920 line 17 - Problematic "because". "it is problematic because temperature . . .". I read this sentence multiple times and still have no idea what it is saying.

We have changed the sentence to "It is problematic since temperature responds to accumulated radiative forcing and radiative forcing by long-lived greenhouse gases is driven by accumulated sources."

9920 line 22 - inherently fascinating for whom?

I'm not sure what the referee is asking us to do here. Should we add a string of references?

9920 first two paragraphs need to reworked and simplified to help reader appreciate what the author clearly excited about.

There's not much guidance in this comment. Neither of the other two referees commented that the abstract was a poor summary of the paper.

9921 line 5 "such changes" be specific

"such changes" refers to the previous sentence. I think this is standard English usage but have changed it to "these changes" which is perhaps clearer.

9921 line 29 "different purpose"? specify

This point is common to all referees and we have expanded on the different intentions of the two papers.

9922 line 18 "CO2 forcing of the response from other drivers". Do you mean the other drivers might be a response to CO2 forcing?

No, we make no comment on the nature of the other drivers.

9922 line 26 is section 4 applying the same diagnostics to inverse estimates at regional levels?

Yes, we have made this explicit.

9925 line 17. "full range of flux estimates available in. . .". Not sure what this statement means.

We have clarified this to say we have not applied our diagnostics to the ensembles of results gathered by intercomparisons.

9926 line 22 "this" maybe mean-squared residuals.

I think the referee is referring to "this value". I don't see an ambiguity there so I'm not sure what the referee is asking us to correct.

9927 line 3 "assuming independence of annual values - what is the reason for assum- ing this? show reference

Instead of an examination of uncertainties in anthropogenic fluxes we note that the assumption we have made is the most conservative possible for the β model and have discussed the implications for the α model.

9927 line 9 "interannual variability has been used . . . ". Can you say what Cox and Wang found out?

We come back to that point in the discussion. Here we only need to motivate the presentation of the figure.

9927 line 17 "larger [value] occurring"

We have reworded the sentence.

9927 line 22 "with the large error bar a result of the large interannual variability" . . . the large error bars are a

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result of large . . .

We have split the sentence and reworded.

9927 line 25 "The change in trend over that time is approaching significance but is not robust". What is this supposed to mean? Why should I believe either the long term beta or the short term beta?

It is not the task of statistical tests to tell people what they are supposed to believe, that's a task for decision theory. We have provided a series of statements of probability about the results. If these are inadequate we need to know why. We have explained the tests we use a little more fully.

9928 line 8 "We obtain β=0.010± 0.001yr for ocean and β=0.006±0.002yr for land." Interesting that the land values have a longer response time than ocean.

Yes, an interesting point but for another paper.

9928 line 9 "This suggests we should increase the land ÎŠ uncertainty to . . .". Please explain

We have elaborated the reason by pointing out the difference between the assumed and actual magnitudes of the residuals.

9928 line 23 "We can apply similar diagnostics to inverse estimates of fluxes." Can you be explicit. Not clear how you are using inversion flux estimates or why?

We have made this explicit.

9929 line 1 "age since more stations now meet the 70ment". I see reference but simply explaining what you mean by 70might be helpful as it is I would have to read the paper to even guess

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at what this might mean.

This is a specific example of the general point raised by the referee on background from other papers. It is one of only two specific examples they note. We address this by describing the criterion explicitly.

9929 line 8 “We are here interested. . .”. Edit.

Done.

9929 line 9 “we adjust the mean fluxes to be equal.” Equal to each other? Please specify.

We have expanded this and added an explanation of why it will have no effect on the calculated β.

9929 line 13 “Next we can ask whether the GCP and inversion agree on the land-ocean . . .”. Explain why these are independent estimates or why this is a sufficient test.

We have added a sentence pointing out the independence of the estimates.

9929 line 14 “The groupings taken from Gurney et al.(2002) rather than a latitudinal separation”. Why is this?

We have added a sentence explaining the need to calculate posterior uncertainty from the inversion correctly.

9930 Line 14. “mean flux noted by Jacobson”. Explain

We have added a comment on uncertainty correlations in Jacobson et al. (2007).

9931 Line 6 “and probably do not”. This is speculation.

No, it is a paraphrase of the findings of Piao et al. (2008).

9931 Line 14 “sum of assimulation”. Be consistent in terminology.

changed to “production”.

9932 line 25 “complex TRANSCOM boundaries used in the inversion” here the bound- ary for inversion is refered to as TRANSCOM with no reference yet above Gurney is referenced with no explanation.

We have changed “transcom” to a reference to Gurney et al. (2002).

9931 line 22 “reasonable way to summarize the behaviour of the large-scale carbon cycle we can also apply it to models”. Not sure why you would not just sum up appropriate fluxes in the model. I agree that comparing this to a data driven estimate might be better however one has to be careful because the anthropogenic fluxes in a model might be the same so you are not really learning anything. I must be missing something.

There seem to be a couple of points here. One is a general comment on the utility of simple model diagnostics. Defending their use is beyond the scope of this reply or the article. I hope it is sufficient to say they are a common choice whenever complex models must be compared to each other or different estimates. I don’t understand the comment on the anthropogenic fluxes since the terrestrial models don’t use these.

9933 line 12 “deepening of growing flux minimum”. How about “increasing”.

“increasing minimum” is an ambiguous phrase, “deepening” is not.

9933 line 13 “strongly implicating concentration changes”. Not clear what is being implicated.

We have made this explicit.

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9933 line 18 "it is in a benign direction". Who decides what is good and bad?

We have replaced this language with "mitigate and exacerbate".

9934 line 24. "Models". Do you mean forward models?

Terrestrial ecosystem models, we have made this explicit.

References

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, doi:10.5194/acp-10-7739-2010, http://www.atmos-chem-phys.net/10/7739/2010/, 2010.

Gurney, K. R., Law, R. M., Denning, A. S., Rayner, P. J., Baker, D., Bousquet, P., Bruhwiler, L., Chen, Y.-H., Ciais, P., Fan, S., Fung, I. Y., Gloor, M., Heimann, M., Higuchi, K., John, J., Maki, T., Maksyutov, S., Masarie, K., Peylin, P., Prather, M., Pak, B. C., Randerson, J., Sarmiento, J., Taguchi, S., Takahashi, T., and Yuen, C.-W.: Towards robust regional estimates of CO_2 sources and sinks using atmospheric transport models, Nature, 415, 626–630, 2002.

Jacobson, A. R., Mikaloff Fletcher, S. E., Gruber, N., Sarmiento, J. L., and Gloor, M.: A joint atmosphere-ocean inversion for surface fluxes of carbon dioxide: 2. Regional results, glob. Biogeochem. cyc., 21, GB1020, doi:10.1029/2006GB002703, 2007.

Piao, S., Ciais, P., Friedlingstein, P., Peylin, P., Reichstein, M., Luyssaert, S., Margolis, H., Fang, J., Barr7, A., Chen, A., Grelle, A., Hollinger, D. Y., Laurila, T., Lindroth, A., Richardson, A. D., and Vesala, . T.: Net carbon dioxide losses of northern ecosystems in response to autumn warming, Nature, 451, 49–52, doi:10.1038/nature06444, 2008.

Rayner, P.: Atmospheric perspectives on the ocean carbon cycle, in: Global biogeochemical cycles in the climate system, edited by Schulze, E. D., Harrison, S. P., Heimann, M., Holland, E. A., Lloyd, J., Prentice, I. C., and Schimel, D., pp. 285–294, Academic Press, San Diego, 2001.

Rayner, P. J., Enting, I. G., Francey, R. J., and Langenfelds, R. L.: Reconstructing the recent carbon cycle from atmospheric CO_2, $\delta^{13}C$ and O_2/N_2 observations, Tellus, 51B, 213–232, 1999.

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