

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

Anonymous Referee #3

Received and published: 11 September 2014

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 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.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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 increasing 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 pitfalls 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.

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.

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

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.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

9920 line 22 – inherently fascinating for whom?

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

9921 line 5 “such changes” be specific

9921 line 29 “different purpose”? specify

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

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

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

9926 line 22 “this” maybe mean-squared residuals.

9927 line 3 “assuming independence of annual values – what is the reason for assuming this? show reference

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

9927 line 17 “larger [value] occurring”

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

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?

9928 line 8 “We obtain $\beta=0.010\pm 0.001\text{yr}$ for ocean and $\beta=0.006\pm 0.002\text{yr}$ for land.” Interesting that the land values have a longer response time than ocean.

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

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?

9929 line 1 “age since more stations now meet the 70% temporal coverage requirement”. I see reference but simply explaining what you mean by 70% temporal coverage might be helpful as it is I would have to read the paper to even guess at what this might mean.

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

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

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.

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

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

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

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

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

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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.

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

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

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

9934 line 24. “Models”. Do you mean forward models?

Interactive comment on Biogeosciences Discuss., 11, 9919, 2014.

BGD

11, C5065–C5069, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

