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Biogeosciences Discussions

Interactive comment on "Does terrestrial drought explain global CO_2 flux anomalies induced by El Niño?" *by* C. R. Schwalm et al.

Anonymous Referee #2

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This study analyse CO2 flux anomalies simulated by two independent methods (bottom-up and top-down) in relation with MEI ENSO index. The authors conlcuded that the global total CO2 flux anomalies do not corresponds to ENSO cycle and not all flux anomalies exhibit a net source to the atmosphere during El Nino events. These results are counterintuitive to the common understanding of the global carbon cycle, but are based on the model results from well established methodologies. I have several reservations on the presentation of the materials, and the conclusions drawn in this paper. The paper could be considered for publication in Biogeosciences after resolving these issues.

Major comments: (mostly reiterated from my comments BG-D submission)

I fully agree El Nino do not force the same phase of CO2 flux anomaly as in the tropics

and rest of the world. It has been discussed in Patra et al. (Tellus, 2005), only some of the tropical land regions show positive correlations between MEI ENSO index and CO2 flux anomaly. The northern mid- and high latitude regions are influenced by independent modes of climate variabilities, such as the NAO/AO, PDO or climate anomalies for some years, such as the hot European summer of 2003 (Ciais et al., 2005) or the anomolous occurence of fires in the boreal regions of Siberia, Alaska and Canada, which are probably not linked with any of the dipole modes of climate variabilities.

Studies suggested that the southwest United States exhibits a greater ecosystem uptake during the El Nino events, while during La Nina phase of ENSO total carbon uptake in that region is much less, due to more frequent occurrence of fires (in California; see Swetnam and Betancourt, Science, 1990). For the Amazon region, long-lasting debate on CO2 flux anomalies continues on compatibility of flux towers derived fluxes with inverse modelled or ecosystem model results (Saleska et al., Science, 2007 and references therein). The light availability, temperature and rainfall are all critical parameters for numerical simulations ecosystem fluxes (Churkina and Running, Ecosystems, 1998). Here the Del_biotic anomaly accounts for water only, if my understanding is right. A more appropriate biogeochemical modelling study would include nitrogen and phosphorus cycle and their feedbacks in to the ecosystem during droughts (through atmospheric aerosols).

Gurney et al. (2003) is cited repeatedly in the paper to support the argument that inversions are underdetermined due to sparse observation network that are commonly used by various groups. I am surprised they did not make any effort to show which sites are used in the Jena inversion (e.g., by overlaying the site locations on Fig. 1b?). For the matter of fact this is not a new finding (Rayner et al., 1996 already talking about the most cost effective observation network expansion). What makes me wonder what are the justifications that the authors took up such an audacious task to interpret the grided inversion fluxes at 3.75x5 degrees? They are well aware of the spatial correlation lengths of $\sim$1200 km that goes in to Jena inversion, which in fact hasn't been stated

until last line of page 4222. Leave alone the tropical areas, is this inversion suitable for grid scale flux retrieval for Europe or USA, where there are a few continental sites?

For the above stated reasons, I have feelings the fundamental limitations of each of the flux products used in this study haven't been taken in to account before performing the analysis. Contrary to my 'personal' liking, they choose to do sophisticated statistics to prove the statistical signifcance of their correlation analysis. This path is unfortunely being choosen more often these days in many publications, without considering very basics of the products under scrutiny. I am taking this opportunity to express that, because this paper has a chance for setting some basic rules for future publications - what products are suitable for what kind of analysis. A logical flowchart could be laid out in this paper.

Lastly, I strongly recommend you to include CO2 concentration anomaly, which is caused mainly due to the biosopheric flux flux variability, in Fig. 5. That will enable you to verify whether the fluxes anomalies you get are realistic. Patra et al. (Tellus, 2005) can serve as a good example for such an analysis and also if you decide to break down the globe in to smaller regions in order to understand regional drivers of CO2 flux anomalies.

At this point, I am not sure whether listing specific comments are useful, though I have marked numeruous on the printed hard copy.

Interactive comment on Biogeosciences Discuss., 8, 4209, 2011.