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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.

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In this manuscript, Schwalm et al show that while a correlation (albeit weakly so) exists between global ENSO anomalies and drought-induced flux anomalies (fire + biotic anomalies), this correlation does not map consistently across space in the 5 identified ENSO positive events analyzed for neither a flux tower upscaling nor an atmospheric inversion. The implication is that ENSO is not a reliable predictor of how any one particular region responds in terms of terrestrial carbon anomaly to ENSO-induced changes in drought conditions or fire frequency (and to those particular changes ONLY). Though this is a contentious conclusion in contrast to a wide range of previously published literature, I think the authors do a good job defending the method and drawing conclusions consistent with the analysis method chosen. That said, I think the particular analysis significantly limits the kinds of interactions we might expect to see between terrestrial carbon uptake and ENSO, and the discussion could be improved to reflect these (e.g., changes in growing season length, role of crop failure, case studies of particular ENSO years, light quality, temperature optimum for NEE). Additionally, there are some areas of correlation that appear to be downplayed because they are small in "area", but that does not preclude them to be "hotspots" of carbon flux changes, no? Finally, I find the discussion and motivation would benefit from a more thorough exposition of prior research on this topic. Currently, it is rather dismissive of much of these papers.

While I find the manuscript well written, I feel that it would be a stronger paper if it addresses the concerns noted above in addition to the following points:

1) I'm surprised the paper does not start off with the well-known link between atmospheric CO2 anomalies and ENSO anomalies. It would be useful to discuss the variability in atmospheric CO2 growth rates as a function of MEI to place into context the short time-frame considered here and to further bolster motivation that there is indeed some link at the global scale between ENSO and carbon cycle. For example, using the method described here to define MEI anomalies, what is the mean and variability of atmospheric CO2 growth rate observed at Mauna Loa over the past 60 years compared to non-anomaly years? Does this variability relate into the correlation found in the total global anomaly in Jena inversion or the upscaling to MEI? If not, what other mechanisms would drive this?

2.) I'm concerned about the lack of representation in the tropics by FLUXNET. While the previously published paper by the same lead author details the methodology used here to construct Delta_Biotic, I do think the discussion here could use more discussion of how well FLUXNET truly samples the kinds of tropical ecosystems we might expect to be particularly drought sensitive and have large changes in terrestrial carbon sink strength (e.g., dry tropical forests in Central America or tropical peatlands in SE Asia). Currently, the manuscript cites a paper on the potential for flux towers in India to defend FLUXNET representation in S. America or Africa. I dispute this finding, especially given the length of records for these flux tower sites and the kinds of ecosystems sampled. I recommend discussing the total number and types of towers used in the tropics and the range of drought sensitivity showed by these sites.

3.) Similarly, given the range of water deficit anomalies present in the MERRA record, how far out of the bounds of extrapolation do the strongest ENSO linked events occur? In other words, is the linear correlation between anomalies of NEE and water deficit observed in FLUXNET over several years at any one site truly representative of some of the more severe drought conditions experienced during a strong positive ENSO? I recommend addressing this in the discussion.

4.) The existence of spatiotemporal correlation between MEI and MERRA water deficit anomaly, but one that does not map onto delta_biotic = FLUXNET derived seasonal land-cover specific sensitivity (summed by IGBP fractional land cover in each grid cell) * MERRA water deficit anomaly, suggests to me that FLUXNET sites are not particularly sensitive to water deficit. Is that true? Or am I missing something here? Were MERRA anomalies downscaled by landcover type within each grid cell? How well does MERRA water deficit match flux tower derived water deficit, especially given energy balance closure issues?

5.) The issue of scale here is addressed for the inversion, but not necessarily for the upscaling, which limits the strength of the conclusion. For example, there does appear to be areas of consistent changes in flux with ENSO (e.g., S Africa, continental US), but they may be poorly correlated across time on a point-to-point comparison. Precipitation variability driven by synoptic and deep convection systems across small areas can be quite large for the same larger scale climatic conditions. Would, for example, the areas with consistent responses in the upscaling change if you first aggregated the responses up to the inversion spatial scale? Clearly, this is true at the limit of global average (given its correlation to MEI), so there must be some scale where the terrestrial anomalies become more consistent. It might be interesting to do a test of this with regularly increasing scale, to better convince a reader of the conclusion on lack of

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spatiotemporal consistency.

Minor points:

P 4211 Lines 12-13 I suggest brining up these papers that DO show consistent responses into the discussion and argue why they are in contrast to your finding.

P4214 Line 12 Uncertainty in IGBP landcover, especially for the tropics, could be significant, but one not included in the uncertainty analysis I believe. I recommend address this especially for land cover types expected to be sensitive to drought: cropland, tropical seasonally dry forests, savannas, peatlands. Is there anyway to estimate the level of uncertainty this may add to your analysis?

Fig. 3 Inset is missing

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